Chymists and Chymistry: Studies in the History of Alchemy and Early Modern Chemistry edited by Lawrence M. Principe

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The editor of this volume of 22 contributions to a conference held at the Chemical Heritage Foundation in Philadelphia on 19 July 2006 invokes in his introduction the memory of an earlier conference, convened in Groningen 17 years prior. He credits that conference with catalyzing a collaboration and informal networking among historians interested in alchemy that led to an efflorescence of alchemical studies and precipitated a need for a new conference to ascertain where the field has gone and to rally a second generation of enthusiasts. I was among those at the 1989 Groningen conference and remember clearly that it commenced with a kind of anti-benediction presented by Nathan Sivin, who proceeded to tell us that the history of alchemy was a dead or dying field, that the few who continued to work in the subject area came to it from other disciplines, not the history of science, and that until specialists in chemistry, religion, and other disciplines took off their blinders and worked together, combining their viewpoints, the field would not again generate new knowledge. And behold, this is what has happened! The chapters of this collection reflect both existing lines of research and those newly undertaken, in many cases in the spirit of collegiality that Sivin hoped that the Groningen conference had conjured.

The 22 contributions to this volume collectively provide a rich sample of current work in the many corners of the history of late Renaissance and early modern alchemy. I will stick with this traditional term instead of adopting the editor's preference for the early modern word 'chymistry', because it is indeed the continuity of chemical practices described in this volume with ancient and medieval alchemy that

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is salient. In many instances, these short chapters introduce work in progress or succinctly epitomize studies published by their authors at length elsewhere. In some cases, the authors engage past historiography explicitly and occasionally also address current competing interpretations.

Didier Kahn's survey of King Henry of Navarre's well-known patronage of Paracelsian chemical medicine when he ascended the throne of France as Henry IV, which further provoked the long-running hostility of the Paris medical faculty toward all things Paracelsian, focuses on the far-reaching network of chemical physicians who served his and his father's courts as diplomats. Footnotes alert the reader to the extensive printed scholarship in French on court alchemy in France, much of it by Kahn himself and François Secret.

The next two chapters concern Andreas Libavius' negotiation of the ideological boundary between alchemy as an occult art and chemistry as a medieval, demonstrative science. Bruce Moran attends to just this theme, namely, Libavius' views on the Paracelsians' verbal obfuscations and the traditional alchemist's claim that only the truly adept, those illuminated by grace (*donum Dei*), are equipped to read properly the many alchemical emblems and metaphors that characterized the *corpus alchemicum*. Not rejecting the Hermetic alchemy, Libavius sought to make it generally accessible and subject to all scholars' scrutiny—a necessary step for scientific dialectic. Moran's contribution suffers from trying to fit too much into a short space; and readers would be well rewarded by consulting his recent extensive treatment of Libavius' efforts at discipline formation, *Andreas Libavius and the Transformation of Alchemy* [2007].

Peter Forshaw uses Libavius' hostility to Paracelsian hermeneutics as a foil for examination of Heinrich Khunrath and the differing attitudes toward the overlap of theology and natural philosophy that characterize many approaches to late 16th and 17th century chemistry. Both men were students of Jacob Zwinger at the University of Basel, who was actively sorting out the useful contributions of the Paracelsians to chemical medicine, and both valued Hermes Trismegistus' *Emerald Tablet* as a preferred expression of ancient alchemical truths. Libavius read the *Tablet* as coded laboratory procedures for preparing the philosophers' stone and admonished his contemporaries to eschew Paracelsian exegesis. Khunrath, in contrast, read

the *Tablet*'s singular teaching, so aptly summarized by James Joyce's 'the tasks above are as the flasks below,' as a cosmic statement of the fundamental unity of the microcosm and macrocosm and the complementarity of prayer and experiment as guides in alchemical work—an idea that Khunrath expressed in the familiar *ora/labore* image in his *Amphitheatrum sapientiae*, surely one of the best-known printed illustrations from early modern alchemy. This difference in readings of the *Tablet* aptly captures the watershed interpretive crisis of late Renaissance alchemy, namely, whether to pursue the book of nature as the full counterpart to holy scripture bringing to bear cabalistic methods or to include inquiry into material composition and transformation among the humanist scholastic disciplines, subject to open disputation and, eventually, published experimental verification.

In the wake of Thomas Kuhn's theory of scientific revolutions and Michel Foucault's emphasis on epistemological ruptures, it has become commonplace to locate chemistry's paradigm shift in the nomenclature reforms of Antoine Lavoisier and his circle in the second half of the 18th century. Stephen Clucas rejects this notion of a 'postponed Scientific Revolution' for chemistry, arguing that proponents of this idea have uncritically mixed all alchemical discourse into one bin and neglected the efforts of Andreas Libavius, Robert Boyle, and other critics of the obscurity of medieval alchemy to reform how chemistry was discussed. Building on recent extensive study of Libavius in this regard by Bruce Moran, Clucas argues that Libavius' effort to reform chemistry within an Aristotelian framework met with limited success because of general dissatisfaction with Aristotelian natural philosophy in the 17th century. Turning away from Foucauldian analysis, Clucas points to Boyle's criticism of the Paracelsians not for their modes of description but for their lack of experimental rigor as marking the true chemical revolution, one characterized by a methodological reform and not by a rupture in the field of discourse. His thesis is that establishing experimental proof as the arbiter of truth was a more salient revolutionary development than the break with the old nomenclature. This in general makes good sense; but it does not address Bill Newman's persistent pleas to consider that experiment was not wholly alien to the medieval and Renaissance Aristotelian tradition and it does not embrace recent attention to the active scientific reforms by Neo-Aristotelians in

the 17th century. Robert Boyle is still too much at the center of this narrative.

Dane Daniel explores the contention made by Carlos Gilly that Paracelsus' religious treatises were intentionally neglected by 16th-century Paracelsian enthusiasts and that when they became salient in the early 17th century, they were associated with Valentin Weigel and other enthusiasts in an effort to deflect criticism away from Paracelsus' medical and philosophical writings. Gilly's thesis helps to explain the nature of religious discussions by the Rosicrucians and other early 17th-century pietists who found in Paracelsus' religious texts a source for reform. But Daniel notes that Paracelsus' religious tracts were eagerly read by Alexander von Suchten, Adam Bodenstein, and Michael Toxites, and that these texts were widely copied and circulated. These facts and the publication of Paracelsus' *Astronomia magna*, which despite its name is fundamentally a Christian theological and anthropological text, contradict Gilly's hypothesis that the early Paracelsians intentionally avoided commentary on, or even exposure to, Paracelsus' theological treatises so as to protect them. Daniel's conclusions do not invalidate Gilly's useful insights but rather sharpen their application to the early decades of the 17th century and raise the important point that we know too little about the early reception of Paracelsus' ideas.

Larry Principe and Bill Newman have recently brought to the forefront of discussion about the history of early modern chemistry the debates over substantial change in 17th-century chemical discourse that arose as a raft of new experiments and theories challenged the reign of Thomistic Aristotelian matter theory. Margaret Garber shows how this debate worked out in Catholic Prague, where concerns about defending Eucharistic theology were paramount, and provides a satisfying example of the importance of taking local circumstances into account in historical analysis. In 1635, the attempt by J. Marcus Marci, dean of the medical faculty, to publish his version of the chemical theory in which material transformation was accounted for by the expression of active seminal principles within matter was blocked by the Jesuit dean of the arts faculty, who sought to maintain the traditional Thomistic teaching of substantial form. Marci had been schooled in the Thomistic tradition at Olmutz but abandoned it in light of laboratory demonstrations which showed that chemical forms persisted in transformations and were not destroyed and created as

Thomistic Aristotelian theory required. Marci eventually succeeded in publishing his book in completed form in 1662, after the structure of the university changed and he was elected rector, with the power to grant himself the needed *imprimatur*.

Not all Jesuits resisted the new metaphysical principles that chemists introduced to explain substantial change. Hiro Hirai explains in how Athanasius Kircher adopted Paracelsian ideas of seminal principles from Marci, Joseph Duschesne, and other writers within the Paracelsian tradition and adapted them to Aristotelian generation theory. Drawing on Marci's ideas about the plastic and attractive powers vested in seeds, recent developments in corpuscular chemistry, and traditional Aristotelian embryology, Kircher conceived of Paracelsian seeds endowed with innate heat, which fostered generation of organic bodies when lodged in suitable elemental wombs. In the depths of the Earth, these seeds formed a sulfurous, saline, mercurial vapor. A similar volatilized water was used to explain the subterranean generation of metals in Johann Grasseus' *Arca arcani*, which William Newman argues was used by Isaac Newton in formulating his ideas about the generation of metals. Newman's study of the Newtonian text *Humores minerales* reveals that the English virtuoso elaborated Grasseus' ideas into a theory that metals were constantly being created in the upper regions of the Earth by volatilized metallic fumes that rise from the core and coagulate dissolved metallic juices that are sinking downward. As the heavy metals continue to sink under their own weight, they are destroyed by powerful solvents at the core and re-volatilized, creating a cycle of metallic generation and destruction. The ideas in *Humores minerales* are similar to Newton's discussion of salts in the better known Newtonian text *Of Nature's Obvious Laws*, which is written on the other side, providing clues to the development of Newton's ideas.

Barbara Obrist offers an erudite and engaging analysis of an image of the near-naked lady *natura* confronting the alchemist that is featured in a manuscript titled 'The Complaint of Nature', which has been attributed to Jean Perreal (1516). This beautiful illustration, which is reproduced on the dust jacket to this volume, portrays lady nature as a vivid and sexually-accessible emblem of fertility, and contrasts strongly with the typical period images of nature as the concealed Diana. Building on an interpretive framework developed in Bill Newman's *Promethean Ambitions*, Obrist understands Perreal's

image as part of a dialectic between art and nature, employing the naked *natura* to chide the alchemist for mechanically copying her works rather than reproducing them through deeper understanding of her secrets; her hair, loose in the back and hanging down to her waist, contrasts with Alan of Lille's *Plaint of Nature*, where *natura* has the carefully braided hair of a virgin, suggesting an intentional intertextuality on the part of the author. Unlike the medieval classic *Romance of the Rose*, where nature acts to create using hammer and anvil, in *The Complaint of Nature* she exhibits creation as an organic process undertaken in the womb and not as a mechanical one; she invites the alchemist to model his work not on the mechanic but on animal generation.

In her chapter on 'deconstructing the chemical marriage', Allison Kavey takes on the popular but difficult task of commenting on the broader sexual ideas implicit and explicit in early modern alchemical discourse. Her sample is 31 texts printed in England 1580–1680, 18 of which exhibit sexual metaphors. Historians of science and early modernists in general are quite familiar with the gendering of gold and silver and the ample visual images that are based on the production and reproduction of metals in 'wombs' and from 'seeds', but Kavey's analysis goes further. The sexual dimorphism of hermaphroditic mercury, for example, is well-known to historians of alchemy; but Kavey argues that Mercury, associated with Ganymede, a young messenger serving the other gods, was portrayed in 16th/17th-century English literature as a young homosexual partner for an older man, opening up an entirely different interpretive dimension. Yet, while she notes that the various sexual pairings in the foundational text *The Emerald Tablet* are all heterosexual, she reads these in terms of a broader conception of sexuality than the standard chemical marriage implies:

> they nonetheless present multiple parents and imply multiple sexual pairings for [the production of] the single Stone. . . . In alchemical writing, however, the potentialities of combinations were determined by shared(?) natures, rather than the partners' sexes, and made possible by radical alterations in gender. [129]

One would think that the requirement that metals be like each other in order to mix would imply a homosexual identity but 'gender flexibility, rather than same-sex combinations, proves the key to successful alchemical work' [130], in part solved by the androgynous powers

of the variable mercury. Her addition to the historiography of the Scientific Revolution, not surprisingly, is wrapped up in the changing discourse within alchemy:

> Their language choices, and their ultimate rejection of sexual metaphors, reflect a clear and precise choice to avoid engaging in the ongoing medical debate about the meaning of biological and anatomical sex and the propriety of same-sex coupling in favor of providing readers with accessible language and examples through which to understand alchemy. [135]

That is, the changing discourse that is manifest in the 17th-century choice to de-gender chemical language in favor of un-metaphorical experimental discourse reflects a conscious attempt to make chemistry more directly accessible to the readers—a change in chemical (scientific) ideology and not a change in the cultural understanding of sexual metaphors. I think Robert Boyle would have agreed with this assessment.

Two articles on alchemical apparatus and spaces remind us that archeological studies of the material culture of alchemy can provide important perspectives on how alchemy was actually practiced, which can otherwise only be inferred from written sources. Marcos Martinón-Torres focuses on recovered crucibles, chemical analysis of which reveals traces of chemicals they once contained. Study of their form and composition, which was relatively stable in early modern central Europe, yields clues as to how they were used and the wide circulation of alchemical technologies. R. Werner Soukup surveys the results of extensive archeological study of a 16th/early 17th-century laboratory at Oberstockstall Castle, which was owned by the Fugger family. Recovered alloy of gold, silver, and copper speaks to the laboratory's principle use in assaying the production of Tyrolean mines to direct capital investment; but residues of antimony trichloride and calomel revealed by X-ray diffraction suggest the production of Paracelsian medicines as well.

Taking a social-constructivist approach to the study of alchemical fraud in early modern Europe, Tara Nummedal analyses the case of Hans Nüschler, who entered into a contract with Duke Friedrich of Württemberg to produce gold from silver and to prove the gold at his own expense in the Duke's laboratory. When the trials failed, Nüschler turned to fraud in an attempt to cover his failure but was

discovered, arrested, and then hanged like previous alchemical frauds. Nummedal concludes that the willingness of alchemists like Nüschler to enter into contracts and assume all the risk of the demonstration means that they believed in honest transmutation and that they could make these procedures work, rather than intentionally duping patrons into financing their failures, as depictions of alchemists in popular literature attest. In her view, fraud was not an ethical issue as much as a legal category constructed by the wealthy patrons in response to the client's failure to fulfill his contract. Nummedal is concerned that study of alchemical fraud might be interpreted as undermining attempts of historians of science to take alchemy seriously and argues that examination of cases like Nüschler's 'opens up a whole world of entrepreneurial alchemical practice' in the period and reveals that

> alchemy was not merely a bookish or symbolic object of study in the sixteenth century, but was also thoroughly immersed in the world of profits, money and political authority. [180]

The careful reader will see that the issue of fraud, fraught with methodological issues about determining intentionality from court cases, is not directly implicated in her larger argument. Ultimately, she makes the important point that fraud and reactions of alchemists to fraud are topics that touch on issues of who had the authority to define the legitimacy of alchemy and how this was done.

Victor Boantza's study of the chemical ideas and laboratory work of Samuel Duclos, a little known founder of the Paris Academy, takes on the hegemony of Robert Boyle in many accounts of 17th-century chemistry and shows the limitations of social constructivist accounts of science based on study of Boyle. The long-lived characterization of Boyle's role in leading alchemy to chemistry, which has only been seriously challenged and revised in the past decade or so, was based in part on Fontenelle's distinction between chemistry and physics that was based on the reduction to mechanical principles, in which he identified Boyle with physics and Duclos with chemistry. According to Boantza, this characterization 'has cast an enduring spell upon the historiography of early modern chemistry', enabling historians to focus on Boyle's work as decisive and to ignore Duclos' work [182].

Duclos joined the Paris Academy in 1666 at age 68, charged with building and managing chemical laboratories for analysis. He was a very active member and figures prominently in early Academy records, being 'mentioned more than any other academician' during the early years [184]. The Academy charged Duclos with reading and commenting on Boyle's work, beginning with *Certain Physiological Essays* (1661), and he used the opportunity to confront Boyle's deployment of mechanical philosophy in chemistry. Boantza here undertakes to revise our understanding of Duclos' chemistry and to shed light on the production of chemical knowledge in this seminal period through careful study of Duclos' critical reading of Boyle. He observes that 'insofar as skill and erudition are concerned Duclos emerges as superior to his English counterpart' [185]. Duclos' method was to isolate excerpts from Boyle's text and subject them to comment and experimental verification through 'lecture-demonstrations' [185]. The result is that whereas Boyle's experimental reports seem to depend heavily on anecdotes reported by other virtuosi, whose credibility Boyle vouches for, Duclos straightforwardly reports his own reasoning and experimental demonstrations, and criticizes Boyle for failing to verify experiments adequately through repetition, complaining about the variability of reagents and thus the undependability of experiments. Duclos argued that dependability was achievable through careful method. This analysis shows the limitations of applying social-constructivist arguments too widely to the problem of how facts were established in early modern science.

Following Boantza's reassessment of the process of making chemistry conform to the methods and standards of physics in the 17th century, Luc Peterschmitt refutes the idea that chemistry became scientific with its reduction to mechanistic physics by arguing that the Cartesian programs did not permit a distinctive chemical theory and, therefore, could not support such a paradigm shift. His method is to consider three Cartesian mechanical philosophers from the mid to late 17th century; and his approach is inherently philosophical, not historical. The somewhat unsatisfying conclusion of his inquiry is that there was no mechanical chemistry in this period because chemistry and mechanical philosophy are incompatible. A related but more historical approach is that of Bernard Joly, who uses a dispute over the validity of *chrysopoiea* between the mechanist

Nicholas Lemery and the traditional chemist Etienne-François Geoffroy to bring insight to the contributions of mechanical philosophy to the development of chemistry. Geoffroy extended J. J. Becher's experimental production of iron from clay and oil, substituting wood ash for clay. Geoffroy suggested that the iron was transmuted from the principles of the wood upon burning. But Lemery insisted that the iron was already present in the wood, drawn from the earth with the tree-sap as it the tree grew, and was not a product of combustion. He then came up with a Cartesian-like mechanical explanation to explain why a magnet could not detect iron in the wood or clay prior to combustion. The irony, Joly finds, is that Lemery's mechanical approach, which was once thought to be a step in the major break from Renaissance vitalist alchemy initiated by Robert Boyle, was rendered obsolete by Newtonian physics, whereas Geoffroy's more traditional ideas led to 19th-century affinity theory.

Continuing his long-standing exposition of the place of Georg Stahl in the history of chemistry, Ku-Ming Chang claims that it was Stahl's careful reviewing and commenting on J. J. Becher's vitalist ideas about the nature of material change that led Stahl to abandon his earlier enthusiasm for 'immanent vitalism' and to develop a more materialist metaphysics that supported the later 'Enlightenment vitalism'. Beginning with his foreword to Becher's *Chymischer Glücks-hafen* (1726), Stahl turned against alchemy, publishing two more anti-alchemical books as commentaries on forewords to Becher texts, including *Natur-Kündigen*, which illustrates the belief that terrestrial metals are nourished by planetary influences and produced by metallic seeds—ideas Stahl came to reject. Stahl concluded that there was nothing like fermentation at work in metal production and that the seed-tincture idea cannot be right, and so he adopted a particulate theory:

> Once Stahl rejected Becher's cosmological picture in which the cosmic vital power and the *semina* of metals were merged, he renounced all possibilities of Renaissance vitalism. [222]

Ultimately, Ku-Ming positions Stahl in a key transition from Renaissance immanent vitalism, where all matter that contains a metaphysical seed possesses innate vitality, to what Peter Reill calls Enlightenment vitalism, where only organic matter is endowed with vitalism, which then is a property of organic matter. The lines of connection

between Stahl's reformulation of 17th-century vitalism and Enlightenment and 19th-century discussions of vitalism are intriguing and give me new stimulus to ponder the legacy of Paracelsus' Renaissance vitalism.

John Powers outlines the historical changes in Hermann Boerhaave's understanding of alchemy. Boerhaave began extensive alchemical experiments after being named Professor of Chemistry at the University of Leiden in 1718, including George Starkey's mercurialist processes for the making the philosophers' stone—all failed. But, despite these failures, Boerhaave defended the principles of alchemy in his 1732 textbook *Elementa chemiae* but specified the need for experimental verification. Boerhaave's experiments did not eliminate belief in alchemical transmutation but did convince him 'that he had taught the wrong theory of metals for thirty years', namely, that the mercury theory of the composition of metals was wrong [237]. Instead, he now favored the idea that metals were formed from 'Guhr'—an oily fluid filtering through the earth. But, by 1636 his days as an experimentalist were over and he did not follow up on this hypothesis. In the end, the influential Dutch teacher shines forth as an exemplar of the emergence of early modern scientific sensibility—a convinced experimental philosopher, open to all claims, but accepting none unless verified by experiments.

How occult ideas within chemistry were displaced by mechanical chemistry and Cartesian and Newtonian matter theory is taken up by Hjalmar Fors, who examines the specific case of the Swedish Board of Mines in the period 1680–1760. The Board's chemical laboratory was initially established for iatrochemical preparation to supply drugs to surgeons in the Board's employ and continued mainly in this capacity to 1689. Then, under the leadership of the Paracelsian-minded Urban Hiärne, the laboratory was operated more independently as a kind of *de facto* royal laboratory. Fors disagrees with earlier accounts of Hiärne as the first important Swedish chemist 'in a modern sense', arguing that Hiärne's continuities were with earlier German thought and not with Enlightenment chemistry, which was introduced to Sweden by Georg Brandt. Brandt studied chemistry under Boerhaave in Leiden; and under his leadership, the Board of Mines became quite oriented toward mechanical chemistry and Cartesian and Newtonian ideas, an orientation followed by his apprentice Axel Cronstedt, who

openly denigrated alchemy as mystical, like magic and astrology, and, therefore, unsuitable as a modern science.

Lest we be too lulled into a renewal of the history of the positive march toward progress in the sciences with Boerhaave and his students, Claus Priesner reminds us that unrecorded scientists who were members of the Gold- und Rose-Cross and Illuminati societies continued efforts to transmute metals in Enlightenment Germany. Samuel Richter (Sincerus Renatus) laid the foundation for Gold- und Rose-Cross as a successor to the Brotherhood of the Rosicrucians in his *Stone of the Sages* (1710) and *Theo-Philosophia* (1711); and the society flourished from 1765 into the 1780s before being officially dissolved 1792. The society, which was mainly Protestant like its Rosicrucian forebears, had some connections with the Illuminati, an anti-religious group of social/intellectual reformers formed in Bavaria in 1776.

Wouter Hanegraaff provides a narrowly-focused story of Giovanni Corregio, an Italian alchemist working in the second half of the 15th century. In his later years, Corregio fashioned himself as a prophetic Neoplatonist, like the Hermetic prophet Pimander, after Ficino's influence, and wrote a treatise on the Phoenix-stone that he dedicated to Pope Julius de Rovero, apparently in a desperate attempt to keep himself out of dire poverty.

Gabrielle Ferrario investigated the origins and transmission of an important medieval alchemical manuscript, the *Liber de aluminibus et salibus*, affirming Julius Ruska's attribution of the manuscript to an anonymous physician of 12th-century al-Andalus rather than to al-Razi or some earlier Arabic writer. The text was first translated into Latin by Gerard of Cremona in the 12th century, used as a source by Vincent de Beauvais and Roger Bacon, and printed in 1560 as part of *Compendium alchimiae*. Multiple Latin manuscripts exist but only one in Arabic and one in Hebrew, suggesting that its popularity came within the Latin alchemical tradition.

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