Arthur J. Hopkins and the Color Theory of Greco-Egyptian Alchemy

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The 14 October 1923 issue of the *Boston Sunday Globe* carried a headline in one-inch capital letters which read:

AMHERST PROFESSOR AND WIFE SOUGHT SECRET OF ALCHEMY BUT FOUND CAMEL RIDING A HARDER TASK

What followed was a heavily illustrated, albeit rather flippant, article describing a trip to Egypt the previous year made by Arthur J. Hopkins, a Professor of Chemistry at Amherst College in Amherst, Massachusetts, and his wife Margaret. Hopkins was considered something of an authority on the origins of Greco-Egyptian alchemy, which had flourished between the 1st and 7th centuries AD, and the ostensible purpose of this trip was so that Hopkins could determine whether any remnants of this ancient craft were still to be found in modern-day Egypt. Though, as we will see, Hopkins' rather eccentric quest would prove unfruitful, he is, nevertheless, remembered today by historians of chemistry as the author of one of the most plausible theories of the origins of the pseudo-science of alchemy, and it is the purpose of this article to review, for the present generation, both his life and accomplishments.

Who Was Hopkins?

Arthur John Hopkins (figure 1) was born on 20 September 1864 in Bridgewater, MA, the youngest of two sons of Lewis S. Hopkins, a medical doctor, and Fanny J. Washburn (1). Like his older brother Frank, after graduation from Bridgewater High School, Arthur attended Amherst College, from which he received his undergraduate degree in chemistry in 1885. After a series of short-lived jobs, including serving as a high school principal (1886-1888) in Cotuit, MA, and teaching science (1888-1890) at the Peekskill Military Academy in Peekskill, NY, he entered graduate school at Johns Hopkins University in Baltimore, from which he received his doctoral degree in 1893 for a thesis done under Professor Harmen N. Morse (1848-1920) on *The Reaction Between Manganese Dioxide and Potassium Permanganate* (2).

Figure 1. Arthur John Hopkins (1864-1939) as a young Professor of Chemistry at Amherst College.

Arthur's choice of Johns Hopkins for graduate work and of Morse as his thesis supervisor were no accident and were most likely prompted by the fact that Morse was also an alumnus of Amherst College, from which he had received his own undergraduate degree in 1873. It was during his graduate school days that Arthur also met a local Baltimore girl by the name of Margaret Sutton Briscoe, whom he would marry in 1895 (3).

After his graduation from Johns Hopkins, Arthur was able to land a job with Westminster College in New Wilmington, PA, but within a year he received a letter from an associate informing him that a position in chemistry at Amherst had suddenly become available because the then current Professor of Chemistry, Elijah P. Harris, who had held his position since 1868, was suffering from ill health and was in need of assistance (4). Despite being warned by a friend that the position at Amherst "was not at all desirable, either in

point of honor or salary," Arthur jumped at the chance to return to his undergraduate *alma mater* (5). Anxious that someone might snatch up the job ahead of him, he wrote to a friend back at Johns Hopkins (6):

... make no mention of the existing vacancy at Amherst to any of the other men at the university, whether students or professors, and especially not to Amherst men as the fact of the vacancy is fortunately little known.

Despite these precautions, in the end there were some 30 applicants for the job, though Arthur, armed with letters of recommendation from both Morse and Ira Remsen, proved victorious and began work at Amherst in the fall of 1894 with a starting salary of $1200 per annum – a change in job status which no doubt allowed him to marry Margaret the following April.

In 1907, after having progressed through the ranks of instructor, assistant professor, and associate professor, Arthur was promoted to full professor and as Harris' successor as department head (7) – a position which he would retain until his own retirement in September of 1934, after 40 years of teaching at Amherst (figure 2) (8).

His retirement years were busy. An avid mountain climber and sailing boat enthusiast, he would help lay out hiking paths to the peaks of many of the mountains in the Holyoke range, would construct an observation tower on top of Mt. Toby, and would write an unpublished manual advocating a decimal system for sail boat navigation, as well as publish his magnum opus on the origins of alchemy (see below). He would finally pass away on 10 November 1939 at age 75 (9). Having requested cremation rather than burial, his ashes were scattered among the hills of his beloved Holyoke range.

Figure 2. Hopkins as he appeared about the time of his retirement in 1934. Note the characteristic pipe and the cane.

Two Who Dared to Live

Hopkins' life at Amherst reads almost like a Hollywood cliché of what the public imagines life at a small American liberal arts college is like. He and his wife were well known characters about town. For their first 45 years at Amherst, they lived, along with their only child – a daughter named Cornelia – in a colonial-style structure known as the Boyden House that was located at the corner of Woodside and Walnut Avenues, and which they rented from the college for $38 per month. After Arthur's death in 1939, Margaret moved to an apartment on Prospect Street, and following her own death in December of 1941, the College decided to tear the house down. This prompted a newspaper article bearing the title "Alchemist and Writer Formed Vivid Amherst College Household" (10).

Margaret, it seems, was a writer of popular short stories and novels under her maiden name of Margaret Sutton Briscoe, and had managed to pack the house with antiques, nicknacks, and travel memorabilia that occupied "every available inch of space," whereas Arthur, nicknamed "Hoppie" by his students, was known for his briar pipe and his insistence on wearing "golf stockings and knickers after most men, even golfers, had discarded them." And, of course, the reporter imagined that Arthur also continued to work after his retirement on the secrets of alchemy in "his home laboratory surrounded by an amazing collection of base metals." The article concluded with the epitaph (10):

Their house was a rallying point for alumni. They were the type that never grows old ... Their house will now be dismantled and this column is dedicated to two people who dared to live.

Chemical Publications and Teaching

As might be anticipated for a teacher at a small undergraduate college in the years before the ACS began to emphasize the importance of undergraduate research, Hopkins' publication record in chemistry is scant and includes only nine papers and notes (11). With the exception of a long paper on the periodicity of the specific gravities of the elements published in 1911 (12), most of these are of little interest today and largely deal with issues related to the teaching of undergradu-

ate chemistry, such as the use of nichrome gauze, the standardization of balance weights, and the calculations used in quantitative analysis.

One item that I found among his papers, and that I cannot resist reproducing, is a humorous piece of chemical doggerel not unlike those that my own high school chemistry teacher would pass out from time to time, though there is no indication of whether Hopkins or one of his students was its author or whether it was simply copied from a popular publication for science teachers:

Ode to Nitrogen Triiodide

Billy in his childish glee
Mixed I with NH_3.
When the stuff was dry and thick
He promptly hit it with a brick.
Billy's now in heaven they say
At least he seemed to go that way.

Also of note is an incident that occurred during Hopkin's first year of teaching at Amherst, as described in a letter that he received on the occasion of his retirement from a member of the class of 1898 (13):

You have probably forgotten the time in my freshman year when Quintard Johnson and I, desiring more fireworks than you furnished over the lecture table, sneaked into the storeroom and got an enormous beaker of large value (there was something about the glassware of a laboratory that always appealed to me and still does; it is so thin and so clean). You were away that day. We rummaged around the storeroom until we found the sodium jar containing long sticks, fascinating in appearance, and, as I remember it, six or eight inches long. You had put a crumb in a small beaker as we watched it burst into flame, but, believe me, you should have seen our piece burst. Nothing ever went around the inside of anything else faster than that sodium went around that beaker. The beaker broke the moment the sodium touched it, which was in the early hours of the experiment. The water poured all over the floor and that damn sodium, still flaming, shot around the floor in the resulting puddle

Carelessly, as I now look at it, you did not tell us how to put out sodium when it was burning in water. The material acted as though it was possessed of the devil. We hit it, we stamped on it, we tried to smother it, we did everything known to freshman minds, but it still flamed and shot about merrily. The old laboratory thereby became a scene of contest and eventual conquest.

We learned from the experiment far more than the chemistry of it. We learned that "your sins will find you out." We learned that one Hopkins was not only a chemist, but a detective. But, best of all, that he was a genial human being with forgiveness in his soul and a sense of humor.

Work on the Origins of Alchemy

If Hopkins' chemical work was unremarkable, the same cannot be said of his work in history of chemistry. This began in 1902 with the publication of an article in the 31 January issue of *The Chemical News* entitled "Bronzing Methods in the Alchemistic Leyden Papyri" (14). Apparently unsure of himself, Hopkins had corresponded with the American chemical historian and bibliographer, Henry Carrington Bolton, over the details of the manuscript in September and October of 1901. Bolton, who was the best known American authority on alchemy at the time, apparently approved of what he read and in November Hopkins received a letter of acceptance from William Crookes, the editor of *The Chemical News* (15):

I have read [your paper] with great interest. If my old friend Dr. Carrington Bolton highly commends the paper, no further recommendation is needed.

It may well be that Crookes himself needed some reassurance from a recognized authority in the history of chemistry before publishing an article on alchemy as he himself had only recently emerged from an embarrassing encounter with a modern alchemist by the name of Stephen H. Emmens during the years 1897-1898 (16).

In this paper Hopkins noted the similarity between various recipes in the 3rd-4th century Leyden X papyrus and the recipes for coloring metal surfaces found in a recently published book on the bronzing of metals by the British chemist Alexander Hiorne (17). This led him to propose two working assumptions (14):

1st. The Egyptian worker in metal was a bronzer.

2nd. Gold or silver was identified, not only by the color of the metal, but also by the bronze that could be produced upon it.

He then used these assumptions to clarify several ambiguous recipes in Marcellin Berthelot's (figure 3) French translation of the Greek in which the Leyden papyrus was originally written (18).

The term bronzing, when used in conjunction with metals, is a rather loose term for any process used to impart color to the metal's surface, including dipping

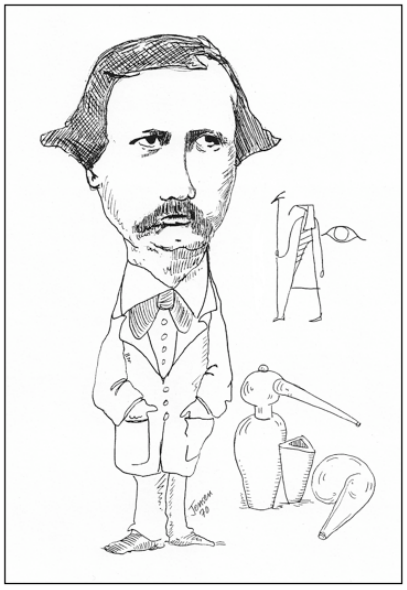

Figure 3. A caricature of the French chemist and historian of early alchemy, Marcellin Berthelot (1827-1907).

in various chemical baths (often followed by heating), exposure to chemical vapors, use of lacquers or other coatings containing powdered metals, or electroplating (19). Obviously Hopkins was interested in the first two procedures. These can alter the color of the metal surface by leaving a colored chemical deposit of some sort, or by leaving a thin film of the metal oxide or sulfide (figure 4). Depending on the thickness of this film, an entire spectrum of colors can be produced (Table 1) due to interference between the reflected and transmitted light rays. The same phenomenon is responsible for the colors observed when a thin film of oil floats on water or when two glass plates are pressed together. Known as Newton's rings, calculation of the wavelengths of the resulting colors as a function of the thickness of the oxide, sulfide or oil film, or of the air space between the glass plates, is an exercise in most introductory college physics courses.

Figure 4. Examples of bronzing effects on an alloy of 90% copper metal and 10% zinc metal (19)

Table 1. Observed colors of oxide and sulfide films on different metals as a function of their thickness (20).

	Colours produced by Air-films between Glass ("Newton's Rings")		Colours produced by Films on Metals		
Order	Viewed by Reflected Light	Viewed by Transmitted Light	Oxide-films on Lead or Nickel. Iodide-films on Silver. Hydroxide-films on Aluminium	Oxide-films on Iron	Oxide- or Sulphide-films on Copper
"Invisible Range."	Colourless (black).	Colourless (white).	Colour of metal unchanged.	Colour of iron unchanged.	Colour of Copper unchanged.
First order.	Blue. Faint green. Yellow. Red.	Yellow. Red to mauve. Blue. Green.	Yellow to brown Rose to mauve. Blue. Silvery (greenish, if film is imperfect).	Yellow to brown. Mauve. Blue. Silver grey.	Brown. Rose to mauve Blue. Brilliant silver (occasionally greenish).
Second order.	Blue. Green. Yellow. Red.	Yellow. Red. Blue. Green.	Yellow to brown Red. Blue. Green.	— Pinkish blue. Blue. Greenish blue.	Yellow-brown* Red. Blue. Green.
Third order.	Blue. Green. Yellow. Red.	Yellow. Red. Greyish blue. Green.	Yellow. Red. (Trace of Lavender blue.) Green.	— Bluish grey (with trace of pink). Bluish grey (specific colour). ,,	Brown. Red. (Trace of Lavender blue). Green.
Fourth order.	— Green. Red.	Dull yellow. Red. Green.	— Red. Green.	,, ,,† ,,	— Dirty red. Dirty green.
Fifth order.	Greenish.	Red.‡	Faint red passing into specific colour of film-substance.	,,	Grey (sometimes trace of red).

* To obtain the higher-order colours on copper, mildly oxidizing conditions are needed, otherwise the colour (due to cuprous oxide) is hidden by sooty cupric oxide.
† If the films are produced carefully, the fourth-order red can be seen on oxidized iron.
‡ Further alternations of red and green can usually be made out, representing sixth- and seventh-order colours.

Hopkins also proposed two further hypotheses that he would expand upon in his later writings. The first of these was that the ancient Egyptians were, because of their drab desert environment, "color hungry" (13):

The Egyptian longed for the golden sun color with which to adorn his temples, and metallic colors were both brilliant and permanent. Brilliant colors, and, if possible, permanent colors, the child nations of the

world have always delighted in. Gold, silver, purple, and (in Egypt) black are the delights of kings and peoples.

The second was that the coloring of metal surfaces through the use of bronzing or pickling baths was based on an analogy with the dyeing of cloth and the use of mordants (13):

Historically it is evident that the original artisan of our our papyri was a dyer in purple, for Papyrus X ends with recipes for dying, and the writings of the pseudo -Democritus are prefaced by two careful recipes for dyeing in purple. Also the dyer evidently transferred his dyeing methods to his new business as a worker in metals, for many of the recipes for producing metals of a golden color strikingly resemble the methods of dyeing.

He also briefly hinted that the order of surface colors obtained in the bronzing process for metals was later perverted by European alchemists into the color sequence required to produce the so-called philosopher's stone:

black → white → yellow → red or violet *(Ios)*

The only items in Hopkins' correspondence that indicate a favorable response to his paper, aside from the preliminary correspondence with Carrington Bolton, came from one William Ward (21):

Let me say that it appears to me that such work as yours is very creditable and it is unusual and peculiar. I am very sorry that you did not have access to the originals in Greek and had to depend upon the French translation of Berthelot.

The issue raised here was the widespread criticism among historians of Berthelot's French translation of the Leyden papyrus (18). Though he would later publish transcriptions of the Greek originals, these were not available to Hopkins when he wrote his paper (22).

Hopkins' correspondence indicates that he later attempted, with the help of a Professor of Greek at Amherst, to publish his own English translation of the papyrus. However, the project fell through when the professor in question lost his eye sight (23). More curious is the fact that Hopkins seemed oblivious to the fact that Earl Caley had not only published a corrected English translation of the Leyden papyrus in 1926, but also one of the closely related Stockholm papyrus the following year (24).

Sixteen years would pass before Hopkins would once again return to the subject of alchemy, this time with a popular article in the *Scientific Monthly* (25). Less encumbered by the detailed discussions of imperfectly translated recipes from the Leyden papyrus, this paper contained much clearer statements of Hopkins' evolving assumptions concerning the nature of Greco-Egyptian alchemy. The most important of these was that he now believed that Greco-Egyptian alchemy had never concerned itself with a literal material transmutation of base metals into actual gold, but rather only with a "color transmutation," leading to the bronzing of various metals and alloys with a golden color or, better yet, a violet color known in Greek as *Ios*.

He now also believed that philosophers had become interested in Egyptian bronzing techniques because they viewed them as a literal experimental test of Aristotle's doctrine of form and matter, i.e. of the ability to strip a piece of matter of its previous qualities and to impose new ones – the quality in question being, of course, its color. In this article he further expanded upon his earlier passing suggestion that, by the time alchemy reached Europe in the 12th century, these doctrines were so muddled that European alchemists would incorrectly interpret them as a literal transmutation of base metals into true gold, rather than as the imposition of a gold color. In other words, they would pass over that thin line that separates, in the later terminology of Needham, the practice of *aurifiction* from that of *aurifaction*. For this reason, Hopkins now decided to call these later confused alchemists "pseudo-alchemists" – an unfortunate choice of terminology that would eventually come back to haunt him.

This article further revealed that Hopkins, at this point, still continued to uncritically accept several of Berthelot's ideas now considered incorrect, including his characterization of the Leyden papyrus as alchemical (26), and his theory, given in his 1885 *Les origines de l'alchimie* (27), that alchemy was originated in the temples of Egypt by priests attempting to con superstitious worshipers via the manufacture and sale of cheap religious amulets made of imitation gold and silver. In addition, there were problems of chronology due to Hopkins' uncritical acceptance of the story that the Roman Emperor Diocletian had issued a decree in 292 AD banning the practice of alchemy for fear that alchemical gold might be used to finance rebellions against Rome, and that this decree had led to a mass exodus of Egyptian alchemists to Persia and other points east and west.

In 1925 Hopkins would publish yet a second, more comprehensive, article on alchemy in *Isis* —George Sarton's recently founded journal for the history of science (28). As we will see below, this was probably a resume of a book that he had been writing since 1920 and which would ultimately be published in 1934.

The Trip to Egypt

As stated in the introduction, in 1922 Hopkins and his wife made a trip to Egypt via Portugal. For Hopkins, at least, the purpose of this trip was two-fold: first, to see if he could discover whether alchemy was still being practiced in these countries and, second, to see if any ancient examples of Greco-Egyptian metal bronzing were to be found among the artifacts in the museums of Cairo. As preparation for the trip, Hopkins contacted the American Egyptologist, James Breasted, of the Oriental Institute of the University of Chicago for advice, to which the latter replied (29):

You are delving into an almost unworked field and into an age much later than that with which we orientalists are chiefly occupied. The practical arts were earlier and more highly developed in Egypt than anywhere else and when the Greeks inherited these, especially in the Hellenistic Age, after Alexander the Great, the Hellenes living in Egypt made amazing progress in such matters. Unfortunately the data for studying them are very scanty and no one has as yet carefully investigated even the limited sources surviving.

As a chemist you could do a great deal of valuable work in the study of color in Egypt, which is almost an untouched subject. The pigments used in painting, the dyes employed in the textile arts, and the colors used in making glass and glazes, all await careful and exhaustive analysis ... I have no doubt you would find the museums quite ready to cooperate with you in furnishing ancient specimens of the various pigments, colors, and dyes, mostly, of course, in the finished product rather than in the raw materials, although we can furnish you with masses of glass and sometimes, also, of painter's pigments. I do not know of any source for dyes other than the dyed textiles themselves and these may be insufficient for your purposes.

Breasted then concluded by recommending several hotels in Cairo and enclosing a letter of introduction to his friend, "J. E. Quibell, who is curator of the great Egyptian museum, commonly called *Musée Egyptian*," though once again cautioning that Quibell's "field is a much older Egypt than the one in which you are interested." Unfortunately, the color sources mentioned by Breasted did not include the bronzed metals of most interest to Hopkins.

The various unreliable newspaper accounts of the trip published after his return imply that Hopkins did indeed discover several modern alchemists during his six-month stay in Cairo, though they were all of the kind that he called "pseudo" and were intent on making real gold (30). Several letters sent in 1923 from one Robert W. Wilson of the Government Assay Office in Cairo, with whom he had apparently made contact with during his trip, reveal that the two of them also attempted to explore more modern remnants of ancient bronzing techniques, including a handwritten book of recipes owned by a local Egyptian, which, however, proved on examination to contain only recipes copied from a 19th-century French volume on the subject (31). In short, the trip failed to uncover the kinds of information Hopkins was hoping to find.

The Book

A surviving outline found in the Hopkins' papers shows that, as early as 1920, he had written the first draft of an integrated history of western alchemy from its Greco-Egyptian beginnings in the 1st century AD to its final demise at the hands of Lavoisier in the last quarter of the 18th century. He sent a copy of this to the Harvard biochemist, Lawrence J. Henderson, for comments and criticism, but, as Henderson admitted in his reply, he was "not competent to criticize the thing in detail," though he did grant that it was "both interesting and valuable" (32). Yet other readers were not so kind, declaring that it was "dull and should be lightened and made more popular" (33). Nevertheless, in 1928 Hopkins finally submitted the manuscript to Macmillan and Company. Though they expressed interest in publishing it, they would do so only on the condition that Hopkins would agree to personally purchase 500 copies on his own at a cost of $900 to cover printing and binding costs (34). For obvious reasons, Hopkins declined.

Then, starting around 1931, a correspondence began between Hopkins and Frederich Barry, an historian of science in the Department of History at Columbia University, in which the latter made suggestions for changes in Hopkins' original outline and encouraged him, in contrast to his earlier reviewers, to make the book more scholarly (23, 33). Apparently Barry was also an editorial advisor for Columbia University Press, which finally published the revised 262-page book (figure 5) in 1934, using – in the words of one of Hopkins' other correspondents – a "lovely purple cover," symbolic no doubt of the ultimate alchemical color of *Ios* (35).

Sixty-five copies of the book were sent out for review to various journals and newspapers and 20 more to various individuals. The resulting reviews, most of which were favorable, appeared in a bewildering range of publications, including the *Journal of the American Chemical Society*, *Industrial and Engineering Chemistry, Nature*, *Isis*, *Scientia*, the *Deutsche Literaturzeitung*, the *Journal of Hellenic Studie*s, and

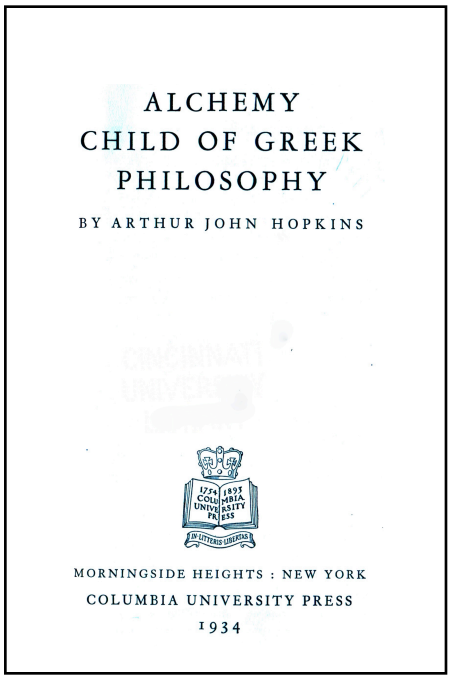
ALCHEMY
CHILD OF GREEK
PHILOSOPHY
BY ARTHUR JOHN HOPKINS
MORNINGSIDE HEIGHTS : NEW YORK
COLUMBIA UNIVERSITY PRESS
1934

Figure 5. The title page of Hopkins' 1934 history of alchemy.

Classical Philology, as well as several more written in Japanese, Chinese, Swedish and French.

In his introduction, Hopkins declared that "no real history of alchemy has yet appeared." At first glance, this may seem absurd, since at least 10 previous 19th- and early 20th-century histories of alchemy are listed in his bibliography (36). What he meant by this remark, however, was that these earlier works, with their extensive lists of alchemists, their dates, and their works, had only provided the requisite raw materials for a proper history, but had fallen short of successfully integrating these laboriously won facts into a synthetic overview of the origins, assumptions, techniques, and gradual transmission of alchemy from one culture to another through time (37).

Despite this claim, Hopkins felt it was necessary to single out at least one of his predecessors for special praise (18, 22, 27, 38, 39):

I distinctly remember ... my thrill of anticipation when a delayed copy of Berthelot's "Les origines de l'alchimie" (1885) was secured; which was followed, with dramatic intervals, by the "Collection des alchimistes grecs" (1888) and "La chimie au moyen age" (1893). It is impossible to estimate the influence which the last six folio volumes exerted as they lay bare the actual writings of real alchemists of ancient and medieval days. Here indeed was a rich fund of material for the history of alchemy.

This, despite the fact that he had by now come to doubt several of Berthelot's conclusions that he had uncritically accepted in his earlier publications, including the latter's characterization of the Leyden papyrus as alchemical and his temple-theory of the origins of alchemy.

In the broadest terms, Hopkins divided his book, in keeping with his original 1920 outline, into successive treatments of Greco-Egyptian, Islamic, and European alchemy, each separated from its successor by a chapter on the transitional period, and the whole followed by concluding chapters on the eventual demise of alchemy in the 18th century. Not surprisingly, his beloved color theory of Greco-Egyptian alchemy was given pride of place, with Islamic and European alchemy receiving lesser, but adequate, coverage. In keeping with the author's decision to restrict the book to a history of western alchemy, virtually no mention was made of either Chinese or Indian alchemy.

Perhaps the most original aspect of Hopkins' presentation was his attempt to provide a rather detailed chemical rationale of three different methods for attaining transmutation via color changes:

1. "The standard method" using bronzing techniques

2. "The primitive method" using sulfur water (calcium polysulfide)

3. "The method of Mary the Jewess" based on exposing metals to the vapors of sulfur, arsenic, mercury, etc. using an apparatus known as the *kerotakis*.

With respect to the latter method he reproduced figures of the various forms of the *kerotakis* as they appeared in several Greco-Egyptian alchemical manuscripts and

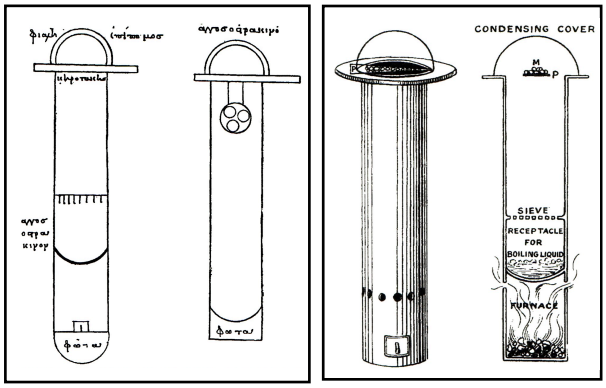

Figure 6. The cylindrical *kerotakis*. (Left) The original figure from the 11th-century MS Marcianus 299. (Right) Taylor's reconstruction of 1930.

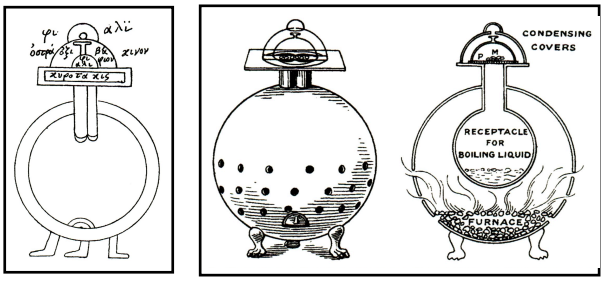

Figure 7. The spherical *kerotakis*. (Left) The original figure from the 11th-century MS Marcianus 299. (Right) Taylor's reconstruction of 1930.

of modern attempts to reconstruct them that had been recently published by a rising young historian of alchemy by the name of Sherwood Taylor (figures 6 and 7) (40). Apparently unsatisfied with the chemistry of this process, as described in his book, Hopkins would later return to the *kerotakis* process in his final published paper in 1938 (41).

Omissions

In his book Hopkins was able to finally correct some of his earlier problems with both chronology and interpretation. Nevertheless, the book still contained some curious omissions. Though, as already indicated, he gave very explicit chemical recipes, both ancient and modern, for the color transmutation process, and he was a professor of chemistry with easy access to both chemicals and laboratory space, it apparently never occurred to him to try and replicate any of these in the laboratory in order to confirm that they did indeed produce the color changes described in the literature or to iron out the confusion between red versus violet as the final stage of the transmutation process (42).

Similarly, though the contents of the Stockholm papyrus were essentially unavailable when he wrote his first paper in 1902, Hopkins was fully aware of this document by the time he wrote his book, where he mentions it in passing in a footnote. Yet he never fully exploited its contents to support his thesis that the recipes for coloring metals at the beginning of the Leyden papyrus were based on an analogy with the few recipes for dyeing cloth found at the end of the papyrus. The Stockholm papyrus is almost equally divided between recipes for the dyeing of cloth and those for the coloring of crystals and stones, thus providing an even more compelling example of the application of Hopkins' presumed mordant-dye analogy to objects other than cloth (43).

Establishing Contacts

Hopkins' collected correspondence, especially after the publication of his book, reads like a *Who's Who* of early 20th-century historians of science, chemistry and alchemy. Including those already mentioned, one finds letters to and from such correspondents as Henry Carrington Bolton, Edgar Fahs Smith, Eric J. Holmyard, John Reid, Tenney L. Davis, the Singers, Sherwood Taylor, Lynn Thorndyke, George Sarton, Frederich Barry, Ernst Darmstaedter, Rudolf Winderlich, and Julius Ruska, as well as such unexpected outliers as the Egyptologist, James Breasted, and the Chaucer specialist, John W. Spargo.

Thus, writing in 1925, Edgar Fahs Smith shared with Hopkins his dream of founding an American journal devoted to the history of chemistry (44):

For years I have dreamed of the "Annals of Historical Chemistry," but thus far have not seen my way clear to launching such a project. I believe a publication of this character is needed.

Alas, Smith's dream would not materialize until long after both his death and that of Hopkins with the publication of the first volume of the journal *Chymia* in 1948. But even this would falter after 12 years, and would not see a successor until 1988 and the publication of the first issue of *The Bulletin for the History of Chemistry*.

In 1919 the Austrian-German historian of alchemy Edmund von Lippmann (figure 8) published the first volume of his massive *Entstehung und Ausbreitung der Alchemie* in which he included an appendix severely criticizing the earlier work of Berthelot (37). In 1923 Hopkins and Holmyard would share their candid opin-

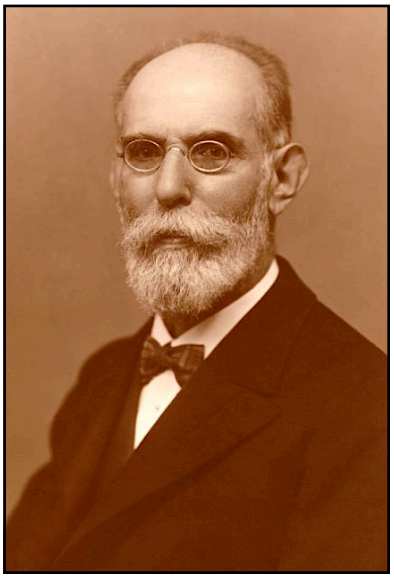

Figure 8. Edmund Oscar von Lippmann (1857-1940).

Figure 9. Julius Ferdinand Ruska (1867-1949).

ions of von Lippmann's critique (45):

In regard to B, I agree that you are right and von Lippmann is right, but I couldn't help getting back at the latter for his evident Gallophobic and postwar exuberance. At the same time, I do not forget that neither you nor I would be working on the history of alchemy if it had not been for this same B. Also be careful not to be led into the same excess.

But perhaps the most extensive and most interesting correspondence was with the German orientalist and specialist on Islamic alchemy – Julius Ruska (figure 9). This went back as early as 1927 when Ruska invited Hopkins to contribute a paper to a *festschrift* in honor of the 70th birthday of von Lippmann (46), and was reignited in 1934 when Hopkins sent him a copy of his book. Initially Ruska was concerned that Hopkins, in his account of Islamic alchemy, had accepted Holmyard's naive belief in the reality of the Persian alchemist Jabir (aka Geber) – a misconception that Hopkins was anxious to correct (47):

Please allow me to explain that I have known Holmyard since 1922; and in my chapters VIII and XI, have given his early views (perhaps too much in length); but then have shown how I, at first intrigued by his estimate of Geber, was compelled to condemn his picture as being without foundation. I first criticized Holmyard for claiming all knowledge of the Egyptians and of Dioscorides as original with Geber; and then gave the arguments of Paul Kraus and the "Ruska school" that Jabir – if he ever existed – was to be reckoned as a "man of straw" set up by the Ishmailist-Schiist sect upon whom to fasten their propaganda ... In a word, I was delighted to have Holmyard's naive defense of the "Existence of Geber" thrown aside and to be able to subscribe to all that the Ruska school has contributed.

That not everyone was as thrilled with the conclusions of the "Ruska school" is apparent from a letter from the Swiss-American medical specialist, Arnold C. Klebs, that Hopkins had received a few weeks earlier, congratulating him on his book (48):

Like an incubus, Ruska's Turba and Pseudo-Jabir and Pseudo-Geber and Pseudo-everything else have been weighing on me, and left everything very dark around. Now you give us light and color and history tastes quite well. Of course, there have probably been others that thought real alchemy was a "Farbenspiel." To me the main thing is that it was a "Spiel" and not a pedantical struggle.

In February of 1935 Hopkins received a detailed critique of his book from Ruska in which the latter stated that "your color theory for the oldest alchemy has thoroughly persuaded me," and then cited detailed evidence that the Arabs were fully aware of the earlier writings of the Greco-Egyptian alchemists (49). However, this apparently secure conclusion was soon to be challenged by an unexpected source.

Defining Alchemy

Starting around 1930, American historian of chemistry, Tenney L. Davis (figure 10), with the help of Chinese collaborators, began publishing articles on the history of Chinese alchemy, as well as translations of early Chinese alchemical texts, thereby anticipating Needham by nearly two decades (50). In late 1936 he caused an upset for both Hopkins and Ruska when he published a provocative article on "The Problem of the Origins of Alchemy," in which he suggested that the major source of influence for Islamic alchemy was probably Chinese rather than Greco-Egyptian alchemy and supported this contention using the contents of Hopkins' recent book (51).

Davis' argument was based on his definition of alchemy. He was tired of the sloppy and indiscriminate use of this term to describe anything both vaguely chemical and vaguely ancient and complained that (51):

Alchemy has been called primitive chemistry, and any chemistry which is ancient enough or sufficiently crude has been called alchemy. Documents of practical chemistry, such as the Leiden and Stockholm papyri, and documents of cosmology, like the Emerald Tablet

of Hermes, which contains no chemistry whatsoever, have been described as documents of alchemy.

What was needed, he argued, was a more restricted and concise definition (51):

A man might conceivably be a chemist, but, if he were not applying his art to the search for the elixir or for transmutation, he would be no alchemist. Another desiring the same results, but without knowledge or practice in chemistry, might perhaps attain them by fasting or prayer, by systematic deep breathing or by magical ceremonials. He also would be no alchemist. The desiring of the object is not enough, nor is the effort to procure them unless it be by chemical means. We therefore conceive alchemy to be the search or the effort, whether successful or not, by chemical means to prepare a medicine of longevity or immortality, or by chemical means to prepare authentic noble metal from base metal or both ... The beginnings of that search are the beginnings of alchemy.

In short, true alchemy had to do with attempts at aurifaction, whereas Hopkins had shown that Greco-Egyptian alchemy had dealt only with a color theory of aurifiction. Since Chinese alchemy, like Islamic alchemy, dealt primarily with aurifaction, Davis argued that it was more logical to consider it as the most likely source for the latter. Ruska was appalled. Apparently confused as to precisely what Davis had proposed, he complained to Hopkins (52):

How can we maintain the connection between Alexandrian and Chinese alchemy without even offering a shadow of a proof? ... Just as well could we make the Aztecs of Mexico or the New Zealanders responsible for Greek alchemy.

For his part, Hopkins was driven to writing a rebuttal, which appeared in the May 1938 issue of *Isis* under the title "A Defense of Egyptian Alchemy" in which he reiterated his claim that Zosimus, by attempting to rationalize Egyptian bronzing techniques using Greek philosophy, was entitled to be considered as the first true alchemist (53).

Close friends, like the British biochemist, Max Nierenstein, were sympathetic to Hopkins' position (54):

I read your paper in the recent issue of Isis, and was very much interested in it. I really never could understand how Davis got his interpretation. However, I think he has imbibed so much Chinese philosophy that he has quite gone Chinese himself.

I thought you might like to hear what outsiders think. Partington was staying with me over the weekend and both he and I agree that Zosimus has a very good claim indeed ... I hate polemics and I sympathize with you heartily.

Figure 10. Tenney Lombard Davis (1890-1949).

However, Davis remained unrepentant and, after thanking Hopkins for a reprint of his rebuttal, challenged him directly in a letter of 14 August 1938 (55):

If you can show that the Alexandrians were trying to make real gold by chemical means from base metal, then I will agree that they were practicing alchemy in accordance with my definition – and I take it that you have no objection to that definition. At present I am not willing to believe that any chemist or alchemist of the third century AD (or very much earlier) didn't know the difference between yellow base metal and authentic gold. I suppose that gold is the one metal which chemists from prehistoric times have been able to identify.

The entire debate died down with Hopkins' passing a year later. Though there is now little doubt that Hopkins and Ruska were correct in believing that Greco-Egyptian, rather than Chinese alchemy, was the most significant source for Islamic alchemy, in a way Davis had highlighted a fundamental weakness in Hopkins' terminology. What Hopkins had dismissed as "pseudo-alchemy," Davis considered to be true alchemy, and what Hopkins believed to be true alchemy, Davis considered not to be alchemy at all. If there was any lesson to be learned in all of this, it is the importance of paying close attention to your opponent's definitions.

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36. Thus he lists the standard works by Figuier (1860), Hoefer (1869), Berthelot (1885, 1888, 1889, 1893,

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37. This claim would seem to be contradicted by the title of the volume by E. O. von Lippmann, *Entstehung und Ausbreitung der Alchimie*, 3 Vols, Springer: Berlin, 1919-1954. However, any integrated overview offered by this work seems to have been eclipsed by von Lippmann's obsession with Germanic thoroughness, as the first volume of 1919 weighs in at 742 pages, as compared with Hopkins' breezy 262-page monograph.

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