CHARLES JAMES, B. SMITH HOPKINS, AND THE TANGLED WEB OF ELEMENT 61

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Introduction

The history of the search for and discovery of Element 61 is one of the most complex and confused of any of the elements in the periodic table. Certainly no element has been "discovered" and named more times than 61. At least seven claims for discovery were made and 61 has been named at various times illinium, florentium, cyclonium, and promethium. The story of element 61 is also intimately connected with the development of the understanding of atomic structure and of the Periodic Table, and of advances in science and technology in the late 19th and early 20th centuries. The story involves Roentgen's discovery of X-rays and Moseley's use of X-ray spectra to determine atomic numbers. It involves the more than one hundred-year effort to separate the rare earths and to find a place for them in the Periodic Table. Finally it involves the development of ion-exchange chromatography and research on the atomic bomb during World War II. Element 61 was named prometheum in 1946 by its discoverers Coryell, Marinsky, and Glendenin after the Titan Prometheus, who stole fire from the gods and was sentenced to eternal torment for the crime, as a warning that atomic energy could be the savior or the destroyer of humankind. The spelling was later changed to promethium by IUPAC.

The story of Element 61 also involves highly competent, careful investigators who searched for and claimed they found an element that almost certainly does not exist in nature.

James and Hopkins

Two American chemists, Charles James (1) of the University of New Hampshire and B. Smith Hopkins (2) of the University of Illinois were involved in the controversies that surrounded claims of discovery for element 61 in the 1920s.

The conventional wisdom on Professor James's contributions is probably best summarized by a quote from an article on Element 61 by Gould, which appeared in *Chemical and Engineering News* in 1949 (3):

> When Hopkins made his announcement in March 1926, James and Fogg of the University of New Hampshire had just completed their fractionation of ytterspar and had sent the 61-rich concentrate to Cork at the University of Michigan for X-ray analysis. The results were reported in December, but by this time the controversies over the other three claims were in full swing, and the fourth entry went almost unnoticed in spite of the fact that the evidence was perhaps better than that of any other claimant. Probably contributing to this neglect was the fact that the announcement was published in a relatively obscure journal (*Proceedings of the National Academy of Sciences)*…Seven lines of the L series, falling between the corresponding lines of elements 60 and 62, were observed in the X-ray spectrogram, which accompanied the announcement.

> To date, no other X-ray spectrogram of element 61 has been published, and while James' work has never been successfully repeated, neither has it been denied or repudiated.

Gould's view of James's role in the element 61 controversy has been repeated by other authors (4), or James has been ignored entirely (5).

The situation is actually much more complex as revealed by scrapbooks kept by Marion E. James, wife of Charles James (6) and recently catalogued by the University of New Hampshire Archives. They contain letters, which together with others in the University of Illinois Archives (7), shed light on the origins of James' search for element 61 and his relationships with B. S. Hopkins and W. A. Noyes.

The instigation of James' search for Element 61 is most probably a letter from Sir William Ramsay to Charles James dated February 26, 1912 (8). In this letter he points out that there are a number of wide gaps in atomic weights between adjacent known elements which may indicate a missing element. Among these gaps is one between neodymium and samarium.

Letters from Sir William Crookes (9) show that from early 1908 Crookes was analyzing rare earth samples spectroscopically in his private laboratory for James. One of these letters, written in 1913 indicates that James was searching for a new element in a sample of ytterbium. Crookes writes, referring to a letter of April 2 from James (10):

> I shall be glad to photograph its (an ytterbium sample) spectrum and send you the results. I can point out to you what impurities it contains, but the actual measurement of the lines in any new element is a very tedious job. I am afraid I cannot undertake to give more than an approximate measurement (say to five figures) of any new lines.

A letter from H. E. G. Moseley to James, dated May 27, 1914, in which he requests a sample of thulia to replace the one which had been lost in the mail contains the lines (11):

> I am most interested to hear of your systematic search for the missing Nd-Sm element. I have been unsuccessful in the few, rather rough, attempts to find the lines corresponding to it in the X-ray spectrum of a Nd-Sm mixture.

This indicates that James had a systematic search for element 61 underway before the publication of Moseley's second paper on atomic numbers (12), which showed that element 61 was missing. Incidentally, Moseley apparently never received the sample of thulia or received it after his paper was submitted because the space for thulium (69), in his list of atomic numbers is vacant.

Why James published his paper on element 61 in the *Proceedings of the National Academy of Science* has been puzzling, since almost all of his 60 papers were published in the *Journal of the American Chemical Society* (13). The answer is provided in a letter to James from Arthur B. Lamb, editor of the *Journal of the American Chemical Society*. This letter dated April 26, 1926 reads (14):

> Dear Professor James:
> The enclosed manuscripts from Dr. Hopkins will, I am sure, interest you. Dr. Hopkins is naturally very desirous of getting them published promptly, indeed in the June number if possible. If you could give me your verdict on them promptly, I would be grateful.
> Yours truly,
> Arthur B. Lamb
> Dictated.
> Manuscripts by Drs. Hopkins and Yntema, enclosed.

James received this letter at the time he was waiting for the X-ray spectrum of his sample to be determined by Cork at the University of Michigan. He was now being asked to referee papers on the very subject he had been working on for probably fourteen years. The papers claimed discovery and proposed the name illinium for element 61 on the basis of evidence that seemed no better than that which he had declined to publish several years earlier. James apparently quickly gave a positive opinion because the two papers were published with the notations: "Received April 26, 1925; Published June 5, 1925."

Evidently James then submitted his paper on Element 61 to the *Proceedings of the National Academy of Science*, to avoid any conflict of interest. Since none of the authors was a member of the Academy, the identity of the transmitter has been a mystery. A letter dated October 25, 1925 to James from Karl T. Compton in Zürich, Switzerland supplies the answer. The letter reads in part (15):

> I have transmitted your very interesting paper on Element 61 to Professor E. B. Wilson, editor of the *Proc. of the Nat. Acad.* I am sorry that the forwarding of your letter has caused some delay.

The paper was published without further delay in the December, 1926 issue of the *Proceedings*.

The Papers on Element 61 of Hopkins and James

It is perhaps useful to examine Hopkins' and James' publications to determine their experimental procedures and the reasoning which led to the conclusion that they had discovered element 61.

Hopkins' publication consists of two papers, "Observations on the Rare Earths XXII. Element No. 61 Part One. Concentration and Isolation in Impure State," by J. Allen Harris with B. Smith Hopkins (16) and "Observations on The Rare Earths XXII. Element No. 61 Part Two. X-Ray Analysis," by J. Allen Harris with L. F. Yntema and B. S. Hopkins (17). The two papers bound together under the title "Element Number 61 (Illinium)" by Joseph Allen Harris constituted Harris's Ph.D. thesis (18).

B. S. Hopkins

In a historical section of the first paper it is stated that since the time of Moseley's work, which definitely showed that an element should exist between neodymium and samarium, a number of attempts had been made to isolate the missing element. The authors then mention several unsuccessful attempts to isolate the unknown element from rare earth minerals. As confirmation of the existence of the element among the rare earths, they cite the paper by Brinton and James, who showed that, when the rates of hydrolysis of the rare earth carbonates were plotted against time, they were (19):

> …generally spaced uniformly from praseodymium on, except after neodymium. At this point there was a distinct gap between the curves of that element and samarium.

In the Introduction, it is stated that in "early 1919 an agreement was entered into between the National Bureau of Standards and the University of Illinois for a comparative study of the arc spectra, especially in the red and infra-red regions of certain of the rarer elements and particularly members of the rare earth group." The results were published in a series of articles in the Bureau of Standards Scientific Papers between 1921 and 1923. In one of these articles Kiess is quoted by Harris and Hopkins as stating (20):

> A third table contains 130 lines of unknown origin which are common to both spectra (neodymium and samarium). These lines are of unknown origin and may belong to the missing element of order No. 61…

On the basis of this evidence L. F. Yntema conducted an extensive fractionation of neodymium and samarium materials using double magnesium nitrate salts. Because the solubilities of the double magnesium salts increase with atomic number, element 61 should concentrate in the fractions between neodymium and samarium. Examination of X-ray spectra, however, failed to show any evidence of element 61; but ultraviolet arc spectra of the purest samples of both neodymium and samarium gave lines common to both elements, which were somewhat stronger in intermediate samples.

Harris took over the project in 1923 and states (18):

> …in view of the foregoing results it was considered that the logical place to search for Element 61 would be among the rare earths…Since the most extensive researchers in attempts to isolate this element had been using fractional crystallization of the double magnesium salts as a means of separation, in which case it is natural to expect to obtain a concentration of No. 61 in those fractions intermediate between the neodymium and samarium and, since all had resulted in failure, three reasons as to the case presented themselves to us.

The paper goes on to detail the three reasons:

> 1) Element No. 61 might be extremely scarce, perhaps the most rare of the rare earths, and so only infinite fractionation of tremendous amounts of materials would result in separation. 2) Very little difference in solubility might exist between the double magnesium nitrates of either 60 and 61 or of 61 and 62, with the result that the element concentrates with one of its more plentiful neighbors. 3) The solubility of the double magnesium nitrate of element No. 61 might

> be entirely unique in falling out of sequence with those of its congeners, and hence would not be found in the fractions intermediate between neodymium and samarium.

They conclude that 1 and 2 are more probable because of the extremely sharp separation between neodymium and samarium when the double magnesium nitrates are fractionated; "the lack of continuity in passing from neodymium and samarium in such a study as that made by Brinton and James; and the presence of new lines in the arc spectra in intermediate fractions." They then assume that 61 is concentrated with neodymium and 61 cannot be detected because its absorption bands are masked by the extensive absorption bands of neodymium. They also conclude that the concentration of 61 remains practically constant throughout the series of fractions and if the ratio is less than 1:1000, the detection by X-ray analysis cannot be relied upon.

At this point they refer to James's paper on the use of bromates to separate the rare earth elements (21). They note that James had shown that, when the solubilities of the rare earths are plotted against atomic number, there is a gradual decrease to europium followed by a gradual increase, as shown in a figure from James's paper. They further state:

> Experience in this Laboratory has shown that the elements arrange themselves in the approximate order of solubility as follows: europium, samarium, gadolinium, No. 61, terbium and neodymium.

Thus it should be much easier to separate 61 from neodymium. Also 61 would probably concentrate with terbium, which has no interfering absorption bands, thus making it possible to detect absorption bands due to the presence of 61. They state that absorption spectroscopy is probably sensitive to one part per one hundred thousand, compared to X-ray spectroscopy that is sensitive to one part per thousand; thus it should be possible to detect the presence of 61 by absorption spectroscopy in amounts too small to be detected by X-ray spectroscopy.

In the experimental section that follows, the separation of a monazite residue donated by the Lindsay Light Company by more than 150 fractional crystallizations is described in great detail. The concluding step was the conversion of a fraction to bromates by the "James Method" (22). After more than 70 fractional crystallizations of the bromates, a band at 5816Å was becoming stronger in some fractions while at the same time characteristic absorptions assigned to neodymium were becoming weaker. In addition, an absorption band at 5123Å, which had previously been assigned to neodymium, was much stronger than other neodymium bands.

At this point Harris and Hopkins make the following conclusion (16):

> The detection of absorption bands at 5816Å and 5123Å confirmed our belief that we were dealing with a new element and increased intensity of these bands led us to hope that the new element had been concentrated sufficiently to enable us to identify it by means of X-ray analysis.

The second paper by Harris, Hopkins, and Yntema describes the construction of an X-ray spectrograph and the collection and interpretation of X-ray spectra. Of special interest is the statement (17):

> A tube was also constructed on which many helpful suggestions were received from Professor Manne Siegbahn who was at the University of Illinois at that time, and to whom the investigators are indeed grateful.

Karl M. G. Siegbahn, at the time Professor of Physics at the University of Uppsala, won the Nobel Prize in Physics in 1924 for his contributions to X-ray spectroscopy. In 1924-25 he traveled extensively in the United States and Canada, delivering lectures at the invitation of the Rockefeller Foundation (23). Evidently one of his stops on his lecture tour was the University of Illinois, probably in 1925. Siegbahn designed and built vastly improved X-ray spectrographs, which allowed a large number of new series of X-radiations to be discovered.

TABLE I

WAVE LENGTHS OF LINES OBSERVED

Date	Fraction	Time of exposure	Observed wave lengths in Å. $L\alpha_1$	$L\beta_1$	Deviation from calculated values
Dec. 16	S_{5-6}	16¹/₄ hrs.	2.2800		+0.0023
Jan. 2	S_{5-6}	24¹/₄	2.2800*		+ .0023
Jan. 24	S_{5-6}	8³/₄	2.2786		+ .0009
Feb. 10	BT_{14-18}	12¹/₂	2.2803		+ .0026
Feb. 22	BT_{14-18}	16³/₄	2.2786		+ .0009
Mar. 6	BT_{14-18}	15	2.2730		− .0047
	Av. value		2.2781		
	Value calcd. from Siegbahn		2.2777		
	Deviation		0.0004+		
Mar. 13	CT_{11}	10¹/₂ hrs.		2.0770	+0.0040
Value calcd. from Siegbahn				2.0730	

* Plate rejected. Loose plate holder.

The new precision technique developed by Siegbahn led to a practically complete knowledge of the energy and radiation conditions of the electron shells of the atoms and created a solid empirical foundation for the quantum-mechanical interpretation.

X-ray spectra of a number of fractions expected to contain element 61 gave results which were summarized in the table reproduced here (24).

Note the average of the observed wavelengths of the $L_{\alpha 1}$ lines is very close to the value "calculated from Siegbahn." Likewise the wavelength of the $L_{\beta 1}$ line is relatively close to the "value calculated by Siegbahn."

The authors also give a table of all the lines possible in the region other than 61 and offer reasons why all can be eliminated. The paper concluded with a summary in which they claim the discovery of a new element on the basis of: 1) The presence of 130 lines in the red and infrared and 5 lines toward the violet in the arc spectra which are common to both samarium and neodymium and which are stronger in intermediate fractions; 2) The presence in the intermediate fractions of absorption bands which become stronger as the characteristic bands of neodymium become weaker; 3) The presence of lines in the X-ray spectrum corresponding to the theoretical positions for $L_{\alpha 1 \text{ and}}$ $L_{\beta 1}$ of Element 61. On this basis they proposed:

> …the name of Illinium with the symbol Il for this element in honor of the state of Illinois and of our university.

James's paper, with James. M. Cork of the Department of Physics at the University of Michigan as first author and Heman C. Fogg, at the time a graduate student and later a chemistry professor at New Hampshire as third author, is entitled "The Concentration and Identification of the Element of Atomic Number 61 (25). The paper opens with the lines:

> In making measurements of the wave-lengths of the X-ray K emission lines for the rare-earth elements, very faint traces of lines corresponding to the K series of the element of atomic number 61 appeared on the plate with certain specimens of samarium (62) and neodymium (60).

With this line is a reference to a paper entitled "A Short Wave X-Ray Spectrograph and Some K Series Emission Wave-Lengths" (26). In this paper Cork describes the design of an X-ray spectrograph similar to one used by Rutherford and Andrade for gamma rays. Cork states that among the advantages of this spectrograph is the fact that all lines of the K series are obtained simultaneously without crystal rotation and that the time of exposure may be greatly reduced by placing the crystal close to the source of the rays. Cork then describes the preparation of samples and the determination of the X-ray K spectra of the elements Ba (56) through Er (68). The source of the rare earth samples is not given, but they most likely came from James. A table gives the wave-lengths in X units of the K_{α}', K_{α}, K_{β} and K_{γ} lines. The lines of each series have an almost linear relationship with a break between Nd (60) and Sm (62). Included is a photograph of a plate containing the spectrum of praseodymium that contains sharp clear spectral lines of Pr as well as tin and tungsten which were used as calibration points.

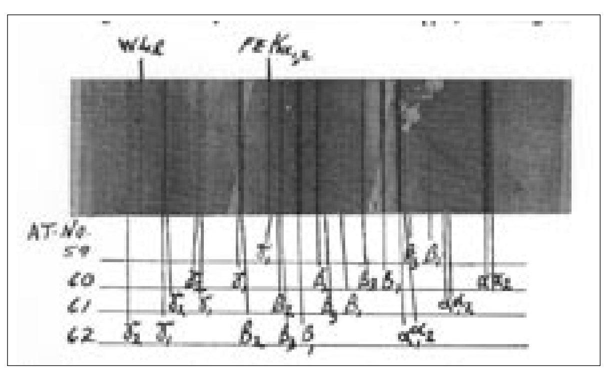
James's paper states that:

> Many of the samples obtained have shown definite traces of lines where they should be expected (for element 61) in both the K and L X-ray regions but the photographic spectra have until now never been strong enough to permit photographic reproduction in a printed article and this was deemed necessary before the announcement of the discovery of the element was made.

A program, beginning in 1923 to examine large quantities of gadolinite, ytterspar, and monazite for element 61 is then described. It is stated that:

> …the minerals were decomposed and separated in the usual methods of fractionation. In the case of gadolinite and monazite this Nd-Sa (sic) portion had to be put through an exhaustive fractionation before even the faintest line of 61 could be discerned. On the other hand, the ytterspar material gave faint lines after just a few crystallizations.

A reference is then made to James's 1914 article on terbium, where faint absorption lines were observed in fractions coming between neodymium bromate and less soluble gadolinium bromate, which were assigned to neodymium (27). This was shown to be a false as-



X-ray diagram from Ref. 25

sumption since further work has shown that neodymium bromate is more soluble than terbium bromate. A careful reexamination of the fractions of bromates more soluble than terbium showed a complete absence of neodymium absorption bands. From this the authors conclude that the faint absorption bands in addition to terbium in the terbium gadolinium fractions were due to the presence of element 61.

In an attempt to confirm the presence of element 61, a large quantity of Brazilian monazite sand was fractionated and the small amount of neodymium which accompanied the yttrium earths was concentrated by fractionally crystallizing the bromate and then the double magnesium nitrate salts. The neodymium fraction was expected to carry the major portion of element 61. This sample was sent to Cork for determination of the X-ray spectrum. The X-ray L spectrum was then obtained using a Siegbahn vacuum spectrograph. A photograph of the X-ray plate is shown with the various lines of elements 59-62 identified (28). They conclude that the sample contained the elements samarium (62), neodymium (60), praseodymium (59), a slight amount of cerium (58), and about 1 to 1.5% element 61. They state that, while there are more than twenty L series lines for each element, only about seven are fairly strong: the α_1, α_2, β_1, β_2, γ_1, γ_2 lines (Siegbahn notation). All seven of the lines for element 61 lie approximately midway between the corresponding lines for elements 60 and 62, with the α_1 line being the strongest. By using the Siegbahn values for 60 and 62, the L wavelengths of the lines of element 61 are calculated as follows:

$$\alpha_1\ 2.289 \quad \alpha_2\ 2.279 \quad \beta_1\ 2.078 \quad \beta_2\ 2.038$$
$$\beta_3\ 1.952 \quad \gamma_1\ 1.799 \quad \gamma_2\ 1.725$$

They then proceeded to eliminate all the possibilities of impurities that might give the observed lines which are attributed to element 61.

Cork had apparently obtained a Siegbahn vacuum spectrograph since he had previously determined the X-ray spectra for James in 1924 on a spectrograph similar to one designed by Rutherford. This Siegbahn instrument that could determine the entire X-ray spectrum without rotating the sample was clearly superior to the Uhler spectrograph used by Hopkins that could only determine one X-ray line at a time by rotating the sample at the appropriate angle.

X-ray Diagram

Line	This Laboratory	Cork, James and Fogg	Harris, Yntema, and Hopkins
$L_{\alpha 1}$	2287.9 ± 0.4 xμ	2289	
$L_{\alpha 2}$	2277.5 ± 0.3	2279	2278.1 ± 3.0
$L_{\beta 1}$	2075.4 ± 0.4	2078	2077
$L_{\beta 2}$	2037.9 ± 0.4	2038	
$L_{\beta 3}$	1951.8 ±0.6	1952	
$L_{\gamma 1}$	1795.2 ± 0.9	1799	

Table from Ref. 29

In 1949 W. F. Peed, E. J. Pitzer, and L. E. Burkhart, working at Oak Ridge National Laboratories, published the L spectrum of Element 61 in *Physical Reviews* (29). In this paper they compared the L spectrum of a sample of Element 61 isolated at Oak Ridge with the spectra obtained by James and Hopkins, as shown in the table:

It is striking that the six spectral lines reported by James and the two by Hopkins are remarkably close to those determined from an authentic sample of element 61.

Personal Relationships of James and Hopkins

Documents recently found in the archives of the University of New Hampshire and the University of Illinois reveal a previously unknown relationship between the careers of James and Hopkins. The revelation of this relationship began when a letter from W. A. Noyes, Chairman of the Chemistry Department at Illinois, to James dated March 23, 1916 ,was found among James's papers. In this letter Noyes offered James a faculty position at Illinois to supervise the General Chemistry program and to carry on the rare earth research at Illinois started by C. W. Balke (30).

A search of the Illinois Archives uncovered a number of interesting documents in the papers of B. S. Hopkins, W. A. Noyes, and the Departmental and Subject File of the College of Liberal Arts. One is a letter from Noyes to Charles Parsons, Secretary of the ACS and James' predecessor as Chemistry Department Head at New Hampshire, requesting a reference for James (31). In this letter Noyes states that James is "almost the only man in the country who could take up this work of Professor Balke and carry it on successfully without a break." There is also a letter from Noyes to Dean K. C. Babcock of the College of Liberal Arts and Sciences requesting permission to invite James for an interview (32). Unfortunately, there is no evidence what reply, if any, James made to this offer. Also among Noyes correspondence concerning a search for chemistry faculty in 1916 are letters which indicate that among those approached after James were Joel Hildebrand, Moses Gomberg, and A. B. Lamb (33). Interestingly, B. S. Hopkins was working for Balke at the time, and Noyes was writing to Babcock favorably about Hopkins in December 1916 (34). Apparently, Hopkins was appointed to the vacancy after the search outside the department had failed.

Charles James

James and Hopkins were in fact friendly rivals, well aware of the other's work in rare earth chemistry, and carried on an extensive correspondence. James reviewed Hopkins's book *Chemistry of the Rarer Elements*, and Hopkins wrote James a letter stating that he appreciated the suggestions for improvements (35), to which James replied that he had liked the book very much and had recommended it to several people (36).

Hopkins visited James in New Hampshire in the spring of 1925. During the visit James took Hopkins to a small storage area where he kept samples of the materials he had prepared. James took down a bottle of one of the rarer earths to show Hopkins, who exclaimed, "Goodness, you are holding in your hand more of that material than exists anywhere in the world." James replied, "Oh, that's just an overflow bottle. I have others here of much larger size" (37). Related to this visit is a letter from James to Hopkins (38), in which James expresses his pleasure about Hopkins' visit and the hope of meeting again.

Apparently, Hopkins was not aware that James was searching for Element 61 before James published his paper because he does not list James in the groups who were "hot on its trail" in an interview that was published in "Eminent Albionia" in 1945 (39). James was also probably not aware of Hopkins' work until he received Hopkins' papers to referee.

That they acknowledged each other's work after publication is shown by two letters:

To be noted is the reference to Element 61, Illinium in the second paragraph. This letter was written while James

Letter from James to Hopkins (40):
August 17, 1926
Dear Professor Hopkins:

I am sorry to say that at the present time we have no man available who has specialized on rare earths.

I was interested in your work on Element 61, Illinium. We also have done some work in the past on this element. Our observations show that it occurs much more commonly in titaniferous xenotimes since the neodymium samarium fraction gives the lines immediately. We have a quantity of this material which we are working up.

Although we have examined a very large number of minerals which has been a tedious matter occupying years, we have come across nothing to equal the one mentioned above.

Yours truly,
C. James (sig)

Letter from Hopkins to James (41):
February 3, 1927
Dear Professor James:

I have just today seen a copy of your article in the December *Proceedings of the National Academy of Sciences* describing your work in the location of element No. 61. Permit me to offer my most sincere congratulation on the excellence of the work. I believe the men in our laboratory can appreciate fully how difficult the task has been and your success is very gratifying. Professor Cork's cooperation in X-ray spectrum work is timely and his photograph is splendid.

I rejoice with you in the successful outcome of the work.

With kindest personal regards.

Very sincerely,
B. S. Hopkins (sig)

was waiting for the X-ray spectrum of his sample of 61 to be determined by Cork at Michigan and after he had served as the referee for Hopkins's papers.

Hopkins wrote a letter dated February 3, 1927 to James, commenting on James's paper in the *Proceeding of the National Academy of Sciences*.

Perhaps others overlooked James's paper on Element 61 in the *Proceedings of the National Academy of Sciences*.

It is quite probable, however, that W. A. Noyes was aware of James's paper when he engaged in his polemical exchange of letters with Luigi Rolla in *Nature* in 1927 over priority for the discovery of 61 (42). Despite this James and Hopkins apparently continued to exchange polite letters concerning their research, as evidenced by a letter from James to Hopkins dated March 9, 1927 (43):

> Dear Professor Hopkins:
>
> We are still working on our bromate solubilities and expect to continue this work for some time yet. We are also running solubilities of some other compounds.
>
> We are beginning to believe that the bromate method will not be used much in the future, since we have discovered a very much better process, giving great speed in separation.
>
> Yours very sincerely,
> C. James (sig.)

Despite the confidence shown by Noyes in his defense of Hopkins's claim for discovery of Element 61, apparently by 1928 he was having doubts about the experimental evidence. This resulted in a letter to James asking him to collaborate with Hopkins to achieve "a prompt and complete solution of this extremely difficult problem" (44). There is no evidence that James ever replied to this letter. In any event he was now terminally ill and would die on December 10, 1928. Hopkins had no doubts, however. He believed to the very end that he had discovered illinium (45).

Following James's death in 1928 his widow sold his collection of rare earth element samples to the National Bureau of Standards. Recently these samples have been returned to the University of New Hampshire, together with their inventory cards. Several of these cards indicate that samples were sent to Clement Rodden of the Atomic Energy Commission (a student of James) for examination. Cards for two samarium samples have a notation, "No trace of #61 with X-ray" (46).

What were the compositions of James and Hopkins's samples from which Cork and Yntema, respectively, obtained the published X-ray data? They could not have contained Element 61, for it has been found in nature only in trace amounts in uranium ores, as a product of uranium fission. If the spectral lines were not due to the presence of Element 6l, where is the logical fault in either their reasoning or experimental procedures? There can be no definitive answers to these questions because Cork used James's entire sample for determination of the X-ray spectrum (47), and Hopkins' samples were apparently lost by Argonne National Laboratory (48).

Unfortunately, James's laboratory notebooks and correspondence have apparently been lost, the only examples being those saved by his wife and included in her scrapbook, now in the archives of the University of New Hampshire. No further information about the relationship between James and Hopkins was found in Hopkins's papers deposited in the Albion College Library Archives. However, an article in the *Chicago Tribune* reports that Hopkins sought "an unscheduled place on the program of the 112th national meeting of the American Chemical Society" to defend his claim to be the discoverer of Illinium (49). This was the meeting in 1947 at which Coryell, Marinsky, and Glendenin claimed discovery of element 61 and proposed the name promethium.

Conclusion

James and Hopkins were cordial rivals whose careers intertwined in unlikely ways. Both were considered for the same position in 1916; they were fellow investigators in the chemistry of the rare earths; they carried on a long standing scientific correspondence; were Alpha Chi Sigma fraternity brothers; and were rival claimants for discovery of element 61.

The Northeastern Section of the American Chemical Society, in which James was long active, published a memorial pamphlet, *The Life and Work of Charles James – 1880-1928*. B. Smith Hopkins wrote the essay "Charles James, the Chemist" for the pamphlet. The concluding paragraph probably best sums up James's contributions and Hopkins's evaluation of James (50):

> Professor James was a prolific worker whose contributions to chemistry are both numerous and valuable. But no doubt the greatest professional contribution of his life was his quiet and kindly influence over the lives of his students. A list of his publications reveals the fact that he has been instrumental in the training of many chemists whose names stand high in chemical

circles. To train such men is to make a contribution whose influence is eternal.

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REFERENCES AND NOTES

1. Charles James (1880-1928), born in Earls Barton, England. Associate, 1904, Fellow, 1907, Institute of Chemistry, University College, London (Sir William Ramsay), Dr. Sci. (Hon) University of New Hampshire, 1927. In 1906 he was appointed assistant professor of chemistry at New Hampshire College. In 1911 he was awarded the Nichols Medal for his research in the chemistry of the rare earths. He succeeded Charles Parsons as Professor of Chemistry and department head in 1912. He is considered as a co-discoverer in 1908 of the element lutetium. In recognition of his rare earth research, the American Chemical Society named Conant Hall where his laboratory was located, a National Historic Chemical Landmark in 1999. He was named to the Alpha Chi Sigma Fraternity Hall of Fame in 2000.
2. B. Smith Hopkins (1873-1952), born Owosso, MI. A. B. Albion College, 1896, Ph.D. Johns Hopkins, 1906 (Morse). Professor, Nebraska Wesleyan (1906-1909), Carroll College (1909-1912). University of Illinois from 1912, Professor of Chemistry (1923-1941), Professor emeritus (1941-52). He studied the chemistry and separations of the lanthanides, beryllium, selenium, and tellurium.
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