CRITERIA FOR GENEALOGICAL ROOTS [1]

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It was almost five decades ago that I revived a then defunct course in history of chemistry at the University of Wisconsin. At that time I was aware of the genealogy prepared by Virginia Bartow (2), who dealt with the chemistry department at the University of Illinois. When I finished the semester I wrote lineages on the blackboard to show my students the genealogy of the Wisconsin department. The students became aware of how their professor fit into the chemical heritage we had been exploring during the semester. The device was a success, and I continued to finish the course with a period devoted to their own heritage. As new faculty joined the department, I inquired about their own graduate education and fitted them into the Wisconsin genealogy. I found that even when they had studied under someone not in the present Wisconsin genealogy, it was generally easy to trace them back in a step or two to a chemist already there.

In 1949 I presented the then current genealogy at the annual meeting of the Wisconsin Academy of Science, Arts, and letters. Handouts I prepared included the professors of satellite chemistry departments in agriculture, engineering, medicine, and pharmacy. It raised a good deal of discussion and suggested that genealogies of other academic disciplines might be prepared. I am not aware that any such lineages were ever prepared at Wisconsin.

Updating the Wisconsin lineage became critical after World War II on two accounts: the death or retirement of several faculty leaders and the rapid expansion of enrollment in the post-war era. Sudden increases in faculty between 1946 and 1955 were common, and I then saw that incorporation of new men (there were no women) into the chart had best await their becoming tenured. During this period the classification of "instructor" almost became obsolete because universities were competing for newly minted doctors of philosophy. It is my criterion, therefore, that nontenured faculty not be incorporated in a departmental lineage until the person becomes tenured, particularly if the genealogy is to be published.

Although I continued to use the Wisconsin lineage annually when the history course drew to a close, I became somewhat derelict in keeping the succession up to date as new faculty became part of the department or moved elsewhere. I continued to think of publication but that always raised questions about updating and resolution of uncertainties. Finally, in the mid 1970s, Alan Rocke had finished his doctorate but was without a job. Teaching positions were not readily available in the 1970s, particularly in the history of science. However, I was beginning to work on what ultimately became a history of the Wisconsin Chemistry Department (3) and was able to obtain a grant out of the bequest which Professor S. M. McElvain had willed the department a few years earlier to hire Alan to do some archival work for me.

Since I considered the faculty intellectual heritage a part of the departmental history, and since I had never found time to update the departmental lineage, I suggested that he become a co-author of the version we finally published in 1979 (4). Alan was largely responsible for the final layout of the genealogy and assembling the necessary background. I remained active in overseeing the project and am responsible for all final decisions, including those that turned out to be questionable. I remember that we were unsure of the Woodward lineage. Alan resolved this by calling Woodward

and was told that Avery A. Ashdown was the correct person, although he was out of the country when Woodward finished his degree. As a result his signature fails to appear on the principal documents connected with the degree awarded to Woodward at MIT in 1937.

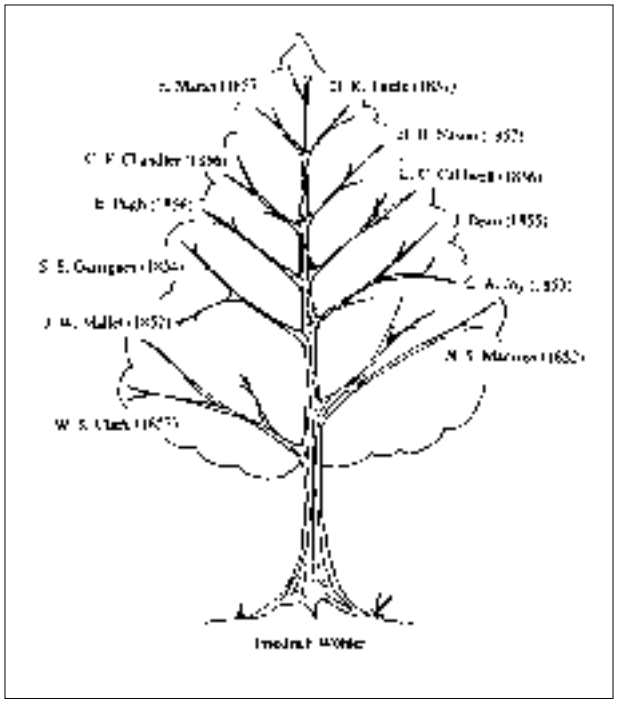
I would argue that, if the person involved is still living, he or she is the most reliable individual regarding the major professor. That person is also the best one to ask if there is any suspicion that the designated major professor was a fraud and someone else was truly the major influence. I do believe also, that one should be skeptical of those who insist that their post-doctoral guide is the most important, particularly when the name of the post-doctoral professor is much better known (i.e., Adams, Woodward, Seaborg) than the Ph.D. sponsor. Very often the post-doctoral professor is merely a busy person who provides an income and a laboratory while he is away from the department attending conferences, giving lectures, and fulfilling consulting obligations.

When Alan and I had resolved our problems, completed the chart, and written the accompanying manuscript, we sent the finished product to William Lippincott, then editor of the *Journal of Chemical Education*. In due time he informed us that he had recently established a policy not to accept any more departmental genealogies for publication, but in this case he would break his policy. Then, never again! Despite the failure of the *Journal* to publish subsequent genealogies, I know that the pursuit of chemical heredity has not been abandoned. I have received frequent questions and have been challenged about some of our decisions as revealed in our published version in 1979. When visiting other chemistry departments, I have also been shown charts of their genealogies, some of them being displayed permanently on a wall of the chemistry building.

The Roger Adams niche has received the greatest number of challenges. Virginia Bartow traced him to Torrey and Richards jointly and connected Torrey to C. L. Jackson (2). I knew that Henry A. Torrey was a professor in the Harvard department and that Adams had begun graduate studies with him before he died in 1910. Although I was aware that Adams did not receive the doctorate until 1912, and that Adams was brilliant in organic chemistry, I persuaded Alan that Torrey deserved recognition and should not be lost to history. Very soon after publication I received letters from other chemists challenging my decision. While I agreed that C. Loring Jackson as the major professor certainly had merit because he helped guide Adams to the Ph.D. after Torrey's death, I failed to make a public effort to change the published version. After D. Stanley Tarbell and Ann Tarbell published their biography of Roger Adams in 1981 (5), I studied Adams' days at Harvard very carefully and felt somewhat content to leave Torrey as the principal influence on him. As a sophomore, Adams completed two half course in organic chemistry with Torrey (no laboratory), historical and elementary physical chemistry under C. R. Sanger and G. P. Baxter, respectively (D in historical, C in physical). In his junior year Adams took a year long course in organic chemistry with Torrey (grade A). By now he had completed enough courses to graduate, but he chose to combine the senior year with graduate studies and included a course in organic reactions given by Torrey (grade A) and a class with T. W. Richards in physical measurements. After Torrey's death in March, 1910 Adams was obviously moving ahead in studies of organic reactions. His work now came under the guidance of C. L. Jackson, as far as his studies on alkali-insoluble phenols were concerned. He received the A.M. degree that spring. The next year he completed his work on physical measurements with Richards, who agreed to let him take his examinations in analytical and inorganic chemistry in June, 1911. Richards is sometimes mentioned as being a joint major professor of Adams, but available information appears to suggest that he merely supervised a minor study. The Ph.D. dissertation, submitted in May, 1912, dealt with three separate investigations, all organic. In the light of the information in Tarbell's book, I am inclined to correct one error and change my position on Torrey as follows: 1) eliminate the solid line between Richards and Torrey since Torrey took no degree under Richards; 2) because Torrey received his Ph.D. in 1897 under Professor Henry B. Hill, an A.B. student of Josiah Parsons Cooke, insert the name of Hill under Torrey and run a solid line down from Torrey to Hill and another solid line from Hill over to Cooke; 3) insert the name of C. L. Jackson in the space under Richards and Hill, another solid line up to R. Adams, and another down to Cooke, since Jackson took his A.B. in 1867 and his A. M. in 1870 under Cooke. Jackson also went to Europe, where he heard Bunsens' lectures at Heidelberg and Hofmann's lectures at Berlin but took no degree under either (6).

Josiah Parsons Cooke, the mentor of Hill, took no degree beyond the Harvard A.B. in 1848 as a student of John W. Webster, the Erving Professor of Chemistry and Mineralogy, who was hanged in 1850 for the murder of Dr. George Parkman, from whom Webster, a high liver,

had borrowed money he could not repay. Webster does not appear in the Wisconsin genealogy simply because Cooke spent a year in Europe where he attended the lectures of J. B. A. Dumas in Paris. When Cooke returned to Harvard, he became a tutor in mathematics for a year and then was awarded the now vacant Erving Professorship of Chemistry, which he held until his death in 1894. If Cooke had not expanded his studies by going to Europe, we would have brought Webster into the genealogy and would have entered a new lineage into our chart. Webster earned his chemistry from Aaron Dexter, the first professor of chemistry at Harvard Medical School, from 1783 to 1815, and the chemist after whom the Dexter Award was named. Dexter is the first in the Harvard chemical lineage, followed by John Gorman and John Webster. Dexter learned a bit of chemistry from John Winthrop, professor of natural philosophy at Harvard, and studied it more extensively during medical training with Samuel Danforth, Jr., a Boston physician.



Early American Doctoral Students of F. Wöhler, Göttingen

A few thoughts are worth considering in preparation of a chart of transmission. If one examines published genealogies, one finds that the early transmissions include Berthollet, Fourcroy, and Berzelius. The Berthollet transmission moves through Gay-Lussac to Liebig. In truth, Liebig obtained his doctorate, not from Gay-Lussac in Paris but from Wilhelm G. Kastner at Erlangen. Liebig joined Kastner at Bonn in 1820 and then followed him to Erlangen a year later. He completed his doctorate there in 1822 but felt his instruction from Kastner was very inadequate. The Grand Duke of Hesse granted him a fund for further study in Paris. The quality of instruction he received from Gay-Lussac started him on his brilliant career in Gießen, where he attracted many students, including some Americans. In 1852 he transferred to the University of Munich; he accepted no more students in his laboratory, but a number of Americans attended his lectures there.

The Fourcroy lineage follows through Vauquelin, who trained Thenard and Stromeyer; and they in turn passed on their chemistry to Dumas and Bunsen, respectively. Both of the latter started their own groups of students, although neither attracted as many Americans as did Liebig and, especially, Wöhler. The latter spent a year with Berzelius after earning a medical degree at Heidelberg and then held minor teaching positions in Berlin and Kassel before setting up his famous program at Göttingen. Wöhler taught many students who became leaders in chemistry of the next generation.

Any attempt to extend the genealogy to earlier teachers has proven unproductive. Bartow introduced the name of Afzelius as the teacher of Berzelius (2); and Graham, in his McMaster genealogy (7), included Afzelius and T. O. Bergman, a man of varied scientific talents and vastly more competent in analytical chemistry than Afzelius, but hardly in a class with Berzelius. While it is true that, when Berzelius studied medical sciences he attended chemistry classes of Afzelius, the latter offered little useful chemistry. Berzelius later acquired excellent knowledge of chemistry and physics through independent study and experimentation. In later years he added to his knowledge by travels in England, France, and Germany

There has been a temptation to introduce Lavoisier as the intellectual grandfather of our three lines of chemical development. This has been properly resisted, despite the fact that Berthollet and Fourcroy were contemporaries who knew Lavoisier through his contributions in the French Academy of Sciences. Although Berzelius never met Lavoiser, he was profoundly influenced by Lavoisier's chemical publications.

By the mid 1900s Americans began to appear for studies with the leaders in German laboratories, and this

trend grew during the later decades. The trek to Germany began to decline by the beginning of the twentieth century, when chemistry professors with German degrees set up graduate programs in leading American universities.

Tracking of American lineages is seldom without problems. While university records are helpful, there is a lack of consistency between schools as to how graduate records are codified, how they are preserved, and who can gain access to them. Graduate schools vary greatly in the nature of requirements. As a result, investigators of chemical lineages report a variety of problems. One prestigious graduate school denies public access to dissertations for a period of years. Some libraries with space problems have actually destroyed certain kinds of theses.

Before I conclude, I would like to express discontent with the term "genealogy" as applied to chemistry students. I consider the term misleading in spite of having used it myself for a half century. I argue that we are not dealing with a genealogy at all but with a "Chart of Intellectual Succession." The term "genealogy" is related to the words generate, genus, genetics, and gene: that is, the areas of biology and biological succession, including the transmission of family characteristics. In the transmission of hereditary traits in the biological world, two parents of opposite sex are involved. In all of the "Chemical Genealogies" I have noted the almost total absence of the female gender. I would argue that the descendant is an intellectual descendant, not a biological one, because there is no transmission and pairing of genes. To be sure, there is transmission of ideas; but clearly lacking is a paired transmission of genes like that involved in family genealogy.

Perhaps I may be accused of raising a trivial distinction, particularly if we examine the reproduction of single-celled organisms that reproduce by cell division. Yet even here, there is evidence of exchange of genetic material under particular circumstances. In the botanical world, also, many plants can be propagated by slipping a part and placing it in nutrient solution or even in soil. In fruit-bearing trees the practice of grafting is widespread. However, in that part of the biological world we consider most common, propagation is by sexual activity. As an aside, I might call attention to the very highly developed genealogical records in the breeding of domesticated animals, not only in race horses, hunting and racing dogs, but in farm animals raised for production of milk, meat, eggs, or fur.

In what is termed chemical genealogy, the most important criterion is the transmission of chemical knowledge and skills. This suggests a serious partnership between master and apprentice during early maturity of the latter and influence with a major impact on the novice for the remainder of his career. In most cases the source of primary impact is the major professor who guides the work toward the Ph.D. degree. The choice of major professor may be made for a variety of reasons: an undergraduate's fascination with a talented teacher or a particular area of chemistry; interest aroused in the research pursued by a particular professor; opportunity for financial support in a particular department; or the recommendation by an undergraduate teacher. These are perhaps the principal, but not the only, reasons that an apprentice ends up being linked with a particular major professor. There is also the unforeseen instance where a candidate begins with a professor who dies within a year or two, moves to another university, or transfers to an industrial position. In cases where the professor moves, he may keep in touch by periodic visits, correspondence, telephone or FAX and return for the final examination. In other situations the university may have rules against research being directed *in absentia*, or there may be other reasons for selecting a new mentor in the home university. In such cases, the "genealogist" must make a wise decision as to who contributed most to the education of the candidate.

REFERENCES AND NOTES

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3. A. J. Ihde, *Chemistry as Viewed from Bascom's Hill: A History of the Chemistry Department of the University of Wisconsin in Madison*, Department of Chemistry, University of Wisconsin, Madison, WI, 1990.
4. A. J. Ihde and A. J. Rocke, "A Badger Chemist Genealogy," *J. Chem. Educ.*, **1979**, *56*, 93-95.
5. D. S. Tarbell and A. T. Tarbell, *Roger Adams: Scientist and Statesman*, American Chemical Society, Washington, DC, 1981.
6. In a revised and expanded Badger Genealogy, 1995, Paul F. Schatz linked Adams through Torrey to both Jackson and Hill, and thence to Cooke (but not to Richards).
7. R. P. Graham, "The Genealogy of a Chemistry Department," *J. Chem. Educ.*, **1948**, *25*, 632-633.