THE 2005 EDELSTEIN AWARD PAPER

TEXTBOOKS AND THE FUTURE OF THE HISTORY OF CHEMISTRY AS AN ACADEMIC DISCIPLINE*

William B. Jensen, University of Cincinnati

In order for history of chemistry to remain academically viable it must be teachable and, in order to teach it, one must have suitable textbooks. By history of chemistry, I am, of course, referring to a specialty course directed specifically at chemistry majors, such as was traditionally taught within chemistry departments, rather than to a general history of science course taught within a history department and directed primarily at undergraduate nonscience majors, in which the chemistry component, if any, deals with its industrial and social impact rather than with its internal conceptual and experimental development. Though specialty courses in the history of chemistry were relatively common before the Second World War, they have become increasingly rare in recent years, due in no small measure to the indifference, if not active opposition, of the American Chemical Society (1). Indeed, if current trends continue, we may soon face the irony of having a Division within the American Chemical Society which deals with a subject that has neither an industrial nor an academic presence.

Having received my training in a chemistry department which still taught a traditional history of chemistry course and holding one of the few chairs in chemistry which explicitly requires that I teach such a course, I have long been preoccupied with the problem of finding suitable textbook material. Indeed, this preoccupation has finally driven me to the extreme of writing a textbook of my own and I thought that it might be of interest to share with you, on the occasion of receiving the Edelstein Award, some of the considerations that lay behind the decision to take this step, as well as some of the problems and lessons which resulted (2).

The Nature and Function of a Textbook

Before describing the chemical issues involved, I should say something about what I consider the nature and function of a textbook to be, as well as a little about the audience it is intended to serve. Though the history of chemistry course which I took as an undergraduate at the University of Wisconsin was spread over two semesters, I am required to cover the same material in a single quarter or in roughly a third of the time (3). My clientele consists largely of chemistry majors in their senior year, with an occasional audit by a graduate student. In addition, the course attracts a few seniors and/or graduate students from the school of education, chemical engineering, the philosophy department, and the medical campus.

Since essentially none of these students has even a rudimentary background in the history of science, let alone in the history of chemistry, the course is intended to serve as an introductory overview or survey. Consequently, the first requirement of a suitable textbook is that it must sketch the evolution of modern chemistry in the broadest possible terms, a requirement that automatically limits the space that can be devoted to discussing the detailed development of individual theories and experimental techniques, the larger political and social context of these discoveries, or the various philosophical issues that were involved. Rather the book must provide a skeleton framework of significant names, dates, and key historical transitions on which this detail can be arranged at a later date, whether acquired through the independent reading of specialist monographs in the history of chemistry or the taking of more advanced courses.

Like a reference work, a good textbook should be tightly organized in order to facilitate rapid access to significant names, events, and dates. Unlike a reference work, however, it must be selective, rather than comprehensive, in its coverage. This selectivity is constrained not only by the comparative importance of the various topics, but also by the fact that few chemistry departments are willing to devote more than a single quarter or semester to a history of chemistry course.

Like a specialist monograph, a good textbook should also provide some context for these names, events, and dates by pointing out significant trends and summarizing interpretive conclusions. Unlike a specialist monograph, however, it cannot present the detailed arguments supporting these trends and conclusions nor indulge in nuanced discussions of subtle distinctions or qualifications. In the interests of clarity and brevity, these summarized, albeit oversimplified, conclusions and characterizations must stand on their own.

Like a popular history intended for the lay public, a good textbook should be readable. Unlike a popular history, however, it does not shy away from using technical terminology, equations, and formulas, or from employing various organizational and scholarly devices, such as sectional headers, summary tables, graphs, and footnotes.

In short, what I wanted was neither a reference book nor an interpretive essay, but rather a survey which took a traditional internalist approach to the history of chemistry and was explicitly targeted at readers having a basic understanding of the principles and techniques of modern chemistry, rather than at nonscience majors or the lay public.

I point out these obvious distinctions between a textbook, on the one hand, and a reference book, specialist monograph, or popular history, on the other, because the textbook appears to be a literary form that has disappeared from the repertoire of the modern historian of science. This neglect is undoubtedly connected with the kinds of questions that are of most interest to professional historians. The type of introductory textbook which I have been describing deals with only the most rudimentary of these: namely with the questions of when certain concepts and techniques became dominant in science and which scientists played a prominent role in that rise to dominance. However, if one asks the further question of how these concepts and techniques were actually discovered, then the situation rapidly becomes more complex and topics, which in an introductory survey consume only a paragraph and which mention only two or three names, suddenly expand to the size of chapters or entire books.

Further complications arise from the fact that modern historians are seldom content to base their accounts of scientific discovery on the published record, but rather seek to discover unpublished correspondence, journals, and laboratory notes which might shed further light on these questions. These unpublished documents are frequently fragmentary, lacking a proper context, and chronologically ambiguous, thus tempting the historian to unrestrained speculation. In addition, they often contradict the published accounts. Though common sense would dictate that these published accounts represent the author's final and considered opinion on the subjects in question, whereas the unpublished documents represent preliminary drafts or paths subsequently rejected, recent historians have tended to invert this view and to claim that the unpublished documents represent the true picture, whereas the published accounts are little more than official misrepresentations. As a result, the recent literature in the history of science has been deluged with highly speculative, and often quite questionable, revisionist accounts of major scientific discoveries (4).

If one moves beyond these questions to the further question of why a given concept triumphed over its competitors and became dominant at a particular time and place, rather than earlier, later, or elsewhere, then things move from the realm of speculation into the realm of acrimony. Attempts to answer such questions range from those who believe in the scientific method and that certain concepts triumph because of their superior explanatory powers, to those who advocate a strict social constructionist approach and maintain that the dominance of one concept versus another is merely a matter of intellectual fads dictated by the larger cultural milieu. Though common sense would suggest that the true reasons probably involve a mixture of these various factors, the proponents of these extremes have again generated a vast and problematic literature which often tells us more about the individual political and philosophical biases of the authors than about the nature of science itself.

Though indulging in speculation and controversy certainly makes for a more stimulating approach to the history of chemistry than does plodding through the introductory basics, I seriously question their use with students who lack the necessary factual background to evaluate independently the cogency of the arguments being offered and feel that they are out of place in an introductory textbook.

The Problem of Selectivity

There are, of course, still numerous older histories of chemistry in print which were written by chemists rather than historians and which essentially conform to the constraints outlined above. In particular, the histories by Leicester, Ihde, and the shorter history by Partington immediately come to mind, all of which are currently available as high quality Dover paperback reprints, as well as the recent history by Fruton (5-8). Indeed, I have at one time or another used all of these as textbooks, but uniformly found them to be unsatisfactory for a variety of reasons. In common with most other older histories of chemistry, they tend to suffer from one or more of the following defects:

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1. They are often heavily biographical in their emphasis, thereby sacrificing conceptual, technical, and sociological insights for anecdotal trivia.

2. They seldom provide any substantive coverage of events after 1925, thus ignoring most of the history of 20th-century chemistry.

3. They often fail to provide overview summaries of significant trends that would allow the student to put names, events, and dates into proper perspective.

4. They often focus exclusively on the historical development of concepts related to the composition and structure of the discrete, stoichiometric, molecular species typical of organic chemistry, thereby ignoring or trivializing the equally important advances made by such fields as solid-state chemistry, phase science, quantum chemistry, chemical thermodynamics, and chemical kinetics – advances which clearly reveal that the traditional molecular mind-set of the organic chemist and the introductory chemistry textbook actually correspond to special cases of a far more general set of chemical and physical concepts.

Anyone who has attempted to write a short overview history of chemistry or has glanced through James Partington's comprehensive, multivolume reference work (weighing in at four volumes and over 3,000 pages) soon becomes painfully aware of the reasons for the first and second of these defects (9). By the second half of the 19th century, the cast of characters and topics becomes overwhelming and, by the 20th century, almost impossible to deal with. Consequently the level of coverage becomes increasingly abbreviated, especially if the author attempts to provide biographical information, however brief, on the chemists and physicists that are mentioned.

Ruthless selectivity becomes essential, though this process automatically produces an historical distortion by associating experimental and conceptual advances with only one or two selected names or dates, when in fact they were really the result of a long evolutionary discovery process and an equally long post-discovery refinement process, each of which involved the cooperative efforts of many chemists and physicists. A closely related consequence of this selectivity is that it also produces a distorted impression of the day-to-day activities of the average chemist. Only a small fraction of the chemical community is privileged to have made a significant conceptual or methodological contribution to chemistry. The vast majority spend their careers applying and refining the concepts and methods discovered by others – a characterization that even includes many activities that were later honored by a Nobel Prize. Work of this sort is absolutely essential to the progress of science and often involves great skill, persistence, and brilliance, though, in the end, the big picture often condemns it to historical anonymity.

The most natural way of applying the requisite selectivity is to let time itself act as the ultimate arbiter of what is to be included and what is to be ignored. In other words, one focuses on the origins of only those advances which still have significance to modern chemistry. This criterion has long been an anathema to professional historians, who claim that all events – even those which qualify as crank science – should be given equal and impartial treatment. To do otherwise is to commit the deadly historical sin of "Whig history." I need hardly point out the incredible naiveté of such a position when it comes to the reasons that motivate most people to either write or read history, let alone the overwhelming impracticality of putting such a plan into practice when dealing with a broad range of topics and time periods—an impracticality which no doubt accounts for the failure of said historians to provide any comprehensive histories of chemistry themselves.

Using the present to select the past also has the shortcoming that the selection process is highly dependent on the author's understanding of the present. What I or some other chemist might deem as historically important will vary with our current understanding of chemistry and with what we consider to be its most fundamental achievements. Indeed, it has been my personal experience that it is virtually impossible to get any two chemists to agree on just what constitutes the most important and most basic principles of chemistry and that they often mistake their areas of specialization or practical industrial applications for fundamental generalizations. This lack of consensus is largely responsible for the third and fourth of the above defects. It is difficult to formulate broad perspectives if one cannot agree on what is important, and most past histories of chemistry have been written by organic chemists with a limited appreciation of the achievements of phase science and solid-state inorganic chemistry.

In my teaching I have found that I not only have to repeatedly commit the historical sin of using the present to select the past, I also have to violate the injunction of said historical theorists against using our current knowledge of chemistry to help clarify and evaluate older theories. If possible, this taboo is even more unrealistic than the first. As even a rudimentary knowledge of educational psychology shows, people do not assimilate new information in a vacuum, but rather seek to integrate that information with their previous knowledge. Historians may have the luxury of assuming that their readers or students are blissfully ignorant of modern chemical theory, but I do not. It is simply impossible to present outdated chemical theories and terminology to an audience of trained chemists without them automatically asking themselves "but what is really going on here?" and attempting to evaluate that theory or terminology in terms of their current knowledge of modern chemistry. Either the teacher or author can attempt to control this integration process by explicitly pointing out the differences and similarities with our current views or allow each individual to do so on their own—a process which can lead to some very bizarre distortions and misinterpretations, as repeated studies by science educators have shown (10).

In the end, it all boils down to the question of just how seriously one should take historians who claim that there is only one legitimate set of historical interests (by which they usually mean the political and sociological context of scientific discovery) and only one legitimate method of writing history, and who furthermore base these claims on the highly dubious proposition that the writing of political history can serve as a legitimate model for the writing of history of science (11). Despite their strident claims to the contrary, science, unlike politics, does progress and we really do know more about the nature of the physical universe in the 20th century than we did in the 15th century, even if we are no wiser when it comes to the motives of the human heart. Historical hindsight is simply not the culprit it is made out to be. Indeed, it can be plausibly argued that hindsight is the only thing that differentiates history from mere chronology.

Making Some Choices

Having also experimented with various organizational approaches, I eventually concluded that a simple century by century chronological approach was best for the type of survey course I had envisioned. Of course, every historian knows that the start and finish of significant historical eras seldom coincide with the turn of a decade, a century, or even a millennium. Yet there is something in the human psyche which endows these arbitrary dates with a special significance and which makes us want to pause and evaluate where we have been and where we are going. More importantly, however, I found that use of these purely conventional time divisions seemed to facilitate mastery of the names and dates required to construct our basic historical framework, whereas students found more sophisticated approaches based on significant eras or themes chronologically confusing.

In the interests of brevity I also decided to restrict my coverage per century to three themes or historical indicators:

1. Professional Development
2. Experimental Techniques
3. Conceptual Content

The category of professional development was intended to subsume the state of chemical training or education, the development of scientific societies and other professional organizations, and the evolution of a distinct chemical literature, including textbooks, monographs of various sorts, journals, abstracting services, etc.

The category of experimental techniques was intended to subsume advances in instrumentation and apparatus, the development of new experimental procedures, and the discovery of new classes of reactions and compounds. Though it might seem odd, at first glance, to include reactions and compounds in this category, the discovery of a new reaction or synthetic procedure can be as productive of new experimental results as the invention of a new instrument, and new classes of compounds may challenge existing theories of bonding and structure as effectively as quantitative data measurements.

Lastly, the category of conceptual content was intended to subsume not only theories proper, but also definitions and nomenclature – in short, all of those aspects that contribute to the organization and interpretation of experimental data.

Of course, these three indicators are not completely independent of one another. New experimental techniques and theories often lead to the development of new specialties at the professional level, whereas professionalization leads to the sharing of experimental data, provides much of the driving force for funding basic research, and streamlines theory by enforcing shared standards of vocabulary and symbolism. Likewise, theory often suggests new instruments and aids in the interpretation of experimental results, whereas experiment, in turn, both confirms and challenges current theory. This interdependence means that, as one approaches the 20th century, it becomes increasingly difficult to avoid some degree of repetition when separately discussing each indicator.

Aaron J. Ihde, 1968 Dexter Award

The necessity of brevity also required that I restrict my survey of experimental techniques and conceptual content to only the most fundamental advances common to all branches of chemistry, whether pure or applied. Consequently it was necessary to largely ignore the specific history of such applied fields as industrial chemistry, geochemistry, biochemistry, etc., most of which are the subject of an historical literature of their own. Similarly, biographical coverage was limited to names and occasionally to nationality and birth and death dates.

A final decision was to begin the survey in the 15th century and to extend it to the end of the 20th century. The choice of the 15th century as the starting point was dictated by the fact that it was essentially the latest date that could still be effectively used as a reference point for a brief overview of the technical heritage of the previous centuries, as it is only in the 16th century that we begin to see the stirrings of a significant change in this otherwise relatively flat chemical landscape. A perceptive reader will note that I used the term "technical heritage" rather than "alchemical heritage." The reason for this is that I believe, despite the recent fad in the history of science, which purports to find the origins of virtually everything from Newton's physics to Boyle's atomism in the alchemical literature, that alchemy proper is not an important progenitor of modern chemistry, which instead clearly evolved out of metallurgy and pharmacy.

As mentioned earlier, much of this revisionist literature is based on so-called "imaginative reconstructions" of manuscripts, personal interactions, and chronologies, as well as on the indiscriminate use of the terms "alchemist" and "alchemy" to describe any chemical activity that suits the thesis at hand. While it is true that, etymologically speaking, the words chemistry and alchemy are one and the same, the restricted use of the Arabic form to describe only those activities dealing with the improvement or transmutation of metals is a tradition which dates back at least to the 17th century and one which I also maintain.

In contrast to the technical and philosophical writings of the Greeks and Romans, the genuine alchemical literature, with its rampant use of allegory, its intentional obfuscation, and its pervasive forgery of dates and names, clearly has more in common with the occult and religious literature than with the literature of secular philosophy, technology, and science. It is alchemy's commitment to these practices, rather than its belief in the transmutation of metals or the elixir of life, which clearly places it outside the history of science proper.

Adoption of this point of view leads to a critical reassessment of the nature and role of certain key protochemical documents. Thus, in keeping with the opinion of Cyril Stanley Smith, I have chosen to view the famous Stockholm and Leyden X papyri as part of a continuous tradition of practical recipe books intended for the use of metal workers and artisans, rather than as proto-alchemical documents, as they have been traditionally portrayed since the work of Berthelot at the end of the 19th century (12).

As for the often repeated claim that the alchemists, despite their obscure writings and questionable theories, developed important pieces of equipment and accidentally stumbled on many new substances, it is, in my opinion, far more likely that they either borrowed or adapted these from the metallurgical and pharmaceutical practice of their day. Though occasional consultation of the alchemical literature is useful for descriptions of common apparatus and chemicals when filling in the gaps in the technical and pharmaceutical literature, the necessity for this increasingly disappears after the 15th century.

Some Historical Lessons

Perhaps the primary historical lesson I learned from writing the book was just how difficult it was, given the necessity of ruthless selectivity, to decide just which 20th-century advances to include and which to ignore. This editing process has long been accomplished for the 18th and 19th centuries and most modern readers, unfamiliar with the chemical literature of these two periods, have no idea of the vast numbers of books and journal articles that this editing process has consigned to permanent historical oblivion. In the case of the 20th century, however, there are many readers still alive who are well aware of the literature extending back at least as far as the 1930s and an author runs the risk of violent disagreements over his unilateral choices of what to include and what to ignore. Indeed, though the book was intended to cover the entire 20th century, I found it almost impossible to pass a reasonable historical judgment on events less than 25 years old, so in effect little is said of developments after 1980.

As already indicated, I found that surveying my fellow chemists was not very helpful in this regard, as most were so focused on their narrow research specialties that they totally lack a basis for making reasonable value judgments about what was or was not fundamental to chemistry as a whole. A more helpful approach was to look at which concepts and techniques had made it into the textbooks, as this was ostensibly an indication that the chemical community had found them important enough to pass along to the next generation. One consequence of this procedure is that the resulting survey places a much heavier emphasis on the publication of significant textbooks and monographs than has been the case with most previous general histories of chemistry.

A second historical lesson was the realization of how closely coordinated the histories of physics and chemistry have been for the last 400 years. It has usually been assumed that this close connection was a development of the late 19th century and has been fully operative only throughout the 20th century. But in fact chemistry and physics have shared the same general assumptions about the nature of matter since at least the 17th century, though chemistry has often exhibited a lag time relative to physics and has often partially modified the shared model to suit its own purposes. The change from hylomorphic models of form and matter to static hylomeric models based on the size, shape, and mechanical entanglement of discrete corpuscles is common to both in the late 17th century; the switch to static dynamical models based on short-range Newtonian interparticle forces and assorted imponderable fluids is common to both in the 18th century; the change to kinetic molecular models and the laws of thermodynamics is common to both in the 19th century; and the switch to electrical models of matter and interaction is the common denominator in the 20th century.

Some Philosophical Lessons

Perhaps the single most important philosophical lesson gleaned from writing the book was the realization of how removed these shared models of matter were from the details of day-to-day experimental work in chemistry. What I mean by this statement is best illustrated by a T-shirt which I saw during a recent visit to Oberlin College. On the front of the shirt was printed the observation:

$\Delta S \geq 0$, All the rest is mere detail

But that is just the point: the devil is in the details. Formulating a nice concise mathematical statement of the second law of thermodynamics as a fundamental principle of nature is fine and good, but it tells you nothing about how to quantitatively apply this law to a specific chemical system. This requires the development of detailed application models which allow one to calculate the entropy changes for specific systems. Contradictory experimental results cannot be taken as a disproof of the second law, but are far more likely to be a disproof of the application model or one of its underlying assumptions. The same is true of the laws of quantum mechanics. The approximations used in applying these laws to chemical calculations are often quite extensive. Again, conflicts with experimental results are always assumed to be a reflection of defects in the application model and not a direct test of the laws of quantum mechanics themselves. Indeed, chemists often have to take the results of quantum chemical calculations with a grain of salt, as presumed general conclusions about the nature of chemical bonding, the details of the electronic structure of molecules, etc. derived from these calculations may not be fundamental at all, but merely artifacts of some nonfundamental assumption of the application model.

James R. Partington, 1961 Dexter Award

The point here is that history shows that chemists are seldom involved in the direct experimental testing of truly fundamental physical laws, but rather are largely occupied with the development and testing of approximate application models, and with questions concerning their accuracy and range of application. The models and theories of science are hierarchical in nature. Those highest in the hierarchy are seldom subject to direct experimental testing. Rather it is at the lower levels of approximate application models that the day-to-day give and take between theory and experiment, much beloved of the philosopher of science, largely takes place.

Some Pedagogical Lessons

Having tested the book in manuscript form for the last three years in my history of chemistry course at the University of Cincinnati, the question naturally arises as to how successful it has been. While I am perfectly happy with the amount of material, which is not excessive for a one quarter survey course, and feel that it provides a much more comprehensive overview of the development of modern chemistry than the previous texts I have tried, I must confess to some disappointments. Experience has shown that most of the chemistry majors taking the course, having survived four years of undergraduate training without being required to memorize any descriptive chemistry, are also extremely resistant to the idea of having to memorize any historical facts. Though I have made a great effort to structure the book and the exams as closely as possible around the types of experiences they have encountered in their chemistry courses, they almost universally lack the ability to assess and master large amounts of verbal information. In this regard, the philosophy majors, though lacking as extensive a chemical background, beat the chemistry majors hands down, and the same is largely true of the engineering students who take the course. Indeed, the only students who do more poorly than the chemistry majors are the education majors.

I will resist commenting on what this says about the defects of the recommended ACS curriculum for chemistry majors or about the quality of the students which it attracts. However, I will note that the ACS has repeatedly proven to be an obstacle in other ways. Since the course covers the development of chemical kinetics, thermodynamics, and quantum mechanics, it has physical chemistry as a prerequisite and is consequently counted as an advanced chemistry credit by the department. This is in fact the primary reason many of our seniors take the course, as they are looking for some relief from the excessive number of laboratory courses they would otherwise have to take their senior year to fulfill the ACS advanced requirements. However, despite this prerequisite and the disturbingly poor performance of our seniors in the course, the ACS has repeatedly refused to support the department's decision to count it as an advanced credit and consequently they have seriously jeopardized its continuing existence. A similar move was taken by the ACS a number of years ago when they rejected Mary

Virgina Orna's attempts to introduce my suggestions for how to structure an introductory chemistry course around the history of chemistry (13). Hence the source of my earlier remarks that the ACS may well prove to be the primary driving force for the demise of history of chemistry as an academic discipline.

REFERENCES AND NOTES

** As part of the Edelstein Symposium held at the 230th Annual ACS Meeting in Washington DC, 30 August, 2005, Dr. Jensen gave the talk, "An Illustrated Tour of the Oesper Collections in the History of Chemistry." Since this involved an informal commentary on more than 80 slides illustrating the Oesper Book, Journal, Print, Photo, and Apparatus Collections at the University of Cincinnati, it was felt that it was impractical to reproduce the flavor of the presentation within the usual format of the Bulletin, and Dr. Jensen has requested that he be allowed to substitute the above remarks instead.*

1. The most recent survey of history of chemistry courses I am aware of is given in K. G. Everett and W. S. DeLoach, "Who is Teaching History of Chemistry?" *J. Chem. Educ.*, **1987**, *64*, 991-993.
2. W. B. Jensen, *Philosophers of Fire: An Illustrated Survey of 600 Years of Chemical History for Students of Chemistry*, Oesper Collections, University of Cincinnati, Cincinnati, OH, 2003. This is the manuscript version tested in class.
3. The course at Wisconsin was taught by Aaron Ihde and formed the basis of both his published text, *The Development of Modern Chemistry* (see below) and the unpublished manuscript, *The Dawn of Chemistry*.
4. For a particularly egregious example, see T. Tucker, *Bolt of Fate: Benjamin Franklin and his Electric Kite Hoax*, Public Affairs, New York, 2003, in which the author attempts to show that Franklin never performed his famous kite experiment, but rather fabricated it in an attempt to indirectly murder his rival, the British electrician, William Watson, whom he suspected of plagiarism.
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8. J. S. Fruton, *Methods and Styles in the Development of Chemistry*, American Philosophical Society, Philadelphia, PA, 2002.
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10. See, for example, M. McCloskey, A. Caramazza, and B. Green, "Curvilinear Motion in the Absence of Forces: Naïve Beliefs about the Motion of Objects," *Science*, **1980**, *210*, 1139-141.
11. For a good critical commentary on the concept of Whig history, see A. R. Hall, "On Whiggism," *Hist. Sci.*, **1983**, *21*, 45-59.
12. C. S. Smith and J. C. Hawthorne, "Mappae Cavicula: A Little Key to the World of Medieval Techniques," *Mem. Am. Philos. Soc.*, **1974**, *64(4)*, 1-128.
13. The suggestions referred to were based on the series of papers, W. B. Jensen, "Logic, History, and the Chemistry Textbook," *J. Chem. Educ.*, **1998**, *75*, 679-687; 817-828; 961-969.

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All photographs courtesy of the Oesper Collection, University of Cincinnati.

ABOUT THE AUTHOR

Dr. William B. Jensen, recipient of the 2005 Edelstein Award of the History of Chemistry Division, is Professor of Chemistry and Director of the Oesper Collections at the University of Cincinnati. A past chair and secretary-treasurer of HIST, Dr. Jensen is the founding editor of the *Bulletin for the History of Chemistry.*