COMMENT AND RESPONSE

Review of *Organic Chemistry Principles in Context: A Story Telling Historical Approach*

The last issue of the *Bulletin* included a review of *Organic Chemistry Principles in Context: A Story Telling Historical Approach*. The book is unusual among those routinely reviewed in this journal because it is not primarily a work of history but a textbook of organic chemistry or a supplement to such a textbook. In its own field the book is also noteworthy in its pedagogical approach—not least in its abundance of historical content, which is what makes it relevant to readers of the *Bulletin*.

The following pages contain, at my invitation, a comment on the review from the book's author, Mark M. Green, and a response by the reviewer, Peter J. Ramberg.

—Editor

Comment by Prof. Green

Dear Editor,

There are several corrections necessary in the review of *Organic Chemistry Principles in Context: A Story Telling Historical Approach*, concerning the historical material, which follows below. The review appeared on page 99 of the latest issue of the *Bulletin for the History of Chemistry*, volume 39, number 1. I've reproduced the contested remarks in the review in the order they appeared. Each is followed by the proposed corrections taken from the quoted historical texts and also, when appropriate, notes to the editor concerning the particular points made.

In a second section of this note, I have made further comments, following this section, about, what I consider to be unjustified more general criticisms of the book by the reviewer. Thank you for the opportunity of responding to the review in this manner.

Historical Accuracy

From the review:

> There are also some errors in the history. It repeats, for example, the myth that Friedrich Wöhler sounded the "death knell" for vitalism when he made urea in 1828…

In *A History of Chemistry* volume IV by J. R. Partington is found a description of Wöhler's work on the synthesis of urea (1):

> Dumas (1830) said: "all chemists have applauded Wöhler's brilliant discovery of the artificial production of urea, …." … Liebig (1831) regarded the discovery of Wöhler and the work of Berzelius on racemic acid as "the first beginning of a truly scientific organic chemistry". Liebig (1843) spoke of urea as "composed in a so-called artificial way almost immediately from its elements", and thus "the natural barrier (die natürliche Scheidewand)

> which until then separated the organic from the inorganic compounds had fallen, and a classification of chemical compounds into organic and inorganic in the earlier sense had no natural basis". Of later writers, Hofmann (1888) spoke of "the synthesis of urea" as "an epoch-making discovery" and it was so regarded by others (1900 and onward).

I am aware that there is controversy about the loss of belief in vitalism over the 19th century. Does the reviewer suggest that Wöhler's work was not a critical input into this process? What Partington wrote is certainly evidence that it was a critical input, a death knell, (allowing use of metaphor), perhaps not heard by all immediately.

From the review:

> ….Archibald Couper was "scooped" by August Kekulé about the tetravalence of carbon and the self-linking of carbon atoms (page 33), because Adolphe Wurtz kept Couper from publishing his paper for a year until 1858, three months after Kekulé's paper, by which time Kekulé had "gained all the credit for the tetravalence of carbon." It's unclear where Green found this story, as it is not in the standard historical literature.

In *Image and Reality*, by A. J. Rocke, appears the following (2):

> His (Couper) new chemical theory announced both the tetravalence and self-linking of carbon atoms, the second statement appearing, as he thought, for the first time. Unfortunately, Kekulé's "theory of aromaticity of the elements" paper defending the same proposal had already appeared in print, in May 1858.
>
> The most unhappy aspect of the matter is that earlier that spring (probably in March or April) Couper asked Wurtz to present this paper to the Académie, but Wurtz was not yet a member of the Académie and so had to request the favor of a colleague. Eventually it was Dumas who presented Couper's paper, but too late to procure priority for the thesis of carbon self-linking. Couper was distraught at the disappointment, and he angrily confronted Wurtz. Wurtz then asked him to leave the laboratory.

From the review

> Green also claims that Kekulé published his benzene theory in 1865, "sponsored by Wurtz," (page 169) when in 1865, Kekulé had been a professor in Ghent since 1858 and had left Wurtz's laboratory long ago in 1852.

In *Image and Reality*, in the section on Aromatic Apparitions, appears the following (3):

> Considering the events that immediately followed Kekulé's trip to Paris, it seems reasonable to believe that he went there specifically to talk to his good friend Adolphe Wurtz about his new theory.
>
> A few days later, Wurtz presented Kekulé's benzene theory to the Société Chimique in Paris.[29] (Kekulé, Substances aromatiques (1865)) Kekulé began by pointing out that no one, "as far as I am aware," had attempted to apply the theory of atomicity of the elements to aromatic compounds. He stated that he had had a "fully formed idea" on this question since 1858, having published hints in that direction in his major paper of that year, but he had not regarded it as appropriate to unveil it publicly and in detail until now.

In the reviewed text the following appears on page 169: "……he wrote his now famous paper in French, because he was a professor in Belgium." Criticism of the book by the reviewer by noting that Kekulé was a professor in Ghent (Belgium) is surprising considering that the book took note of his position in Belgium. In addition to the unjustified criticism concerning Kekulé's position in Belgium, noting Rocke's quoted material above, "sponsored by Wurtz" is certainly justified.

From the review:

> Linus Pauling did not win his Nobel Prize for proposing the structure of the alpha helix (p 10), but for his work on the nature of the chemical bond during the 1930s, a fact that is easily checked on the internet.

On the Nobel Prize web site is found the following statement (4):

> The Nobel Prize in Chemistry 1954 was awarded to Linus Pauling "*for his research into the nature of the chemical bond and its application to the elucidation of the structure of complex substances*".

In the presentation speech is found:

> On this basis Pauling deduced some possible structures of the fundamental units in proteins, and the problem was then to examine whether these could explain the X-ray data obtained. It has thus become apparent that one of these structures, the so-called alpha-helix, probably exists in several proteins.

The alpha-helix, deduced by Pauling from his effort on the chemical bond, was certainly a very important part of the body of work for which Pauling won the Nobel Prize as seen from the web site quoted above although I agree with the criticism that I should have stated more clearly the central role of the chemical bond. There is an

extensive discussion on page 9 of the book of Pauling's role (and his picture) in development of understanding of the chemical bond.

These sentences are in the review: "These fundamental errors are reason enough to suspect others throughout the book." "The strength of Green's approach is therefore not in his use of history, but in his extensive use of specific real-world problems in organic chemistry, ….."

These so-called historical errors are especially unfair in the review, considering that the reviewer himself may be reasonably questioned about his own historical accuracy. There is no basis to make a global condemnation of the historical aspects of the book.

General Criticism

The following sentence appears in the review:

> The general assumption throughout the text is that the first publication of theories resembling our own are unproblematic and were immediately accepted by chemists as correct.

Only reading the reviewed text can convince one that this statement in the review is entirely unjustified, but for just a few of many examples one turns to the discussion in the book of Kekulé's proposal for the structure of benzene. Section 6.6 discusses the objections to this theory and the manner in which these objections were overcome. The section heading is: "Objections to Kekulé's hexagonal ring structure for benzene required an explanation that was the equivalent to the concept of resonance." (p 173)

Here is another example, section 3.3 (p 74) with the heading

> It took many years for chemical science to accept the idea that rings did not have to be flat and further that acceptance of this idea could explain many aspects of the chemical behavior of cyclic molecules. An important advance, as is the situation in science, was the use of a new kind of instrument applied to the problem.

Here is another example. Section 4.3 on page 107 with the section heading

> It took a great deal of time before chemists accepted the possibility that the carbon skeleton of a molecule could change, and then even longer to realize that the agent of change was a chemical intermediate with a positively charged carbon, a carbocation.

Here is another example. Section 8.8 on page 267 with the section heading: "Stereochemistry: Why Krebs' proposal was thought to be impossible."

The criticism in the review is really quite astonishing considering anyone who has read the book. The examples above are just exemplary of the way the book is written with regard to how theories were dealt with.

The criticism in the review of the beginning of Chapter 1 about showing the line structures of cellulose and starch without an introductory explanation does not take account of the approach of the book, which, as pointed out in the Introduction, is presented as a top-down approach or as some say, backwards learning. These structures, drawn in the manner used by organic chemists, which are incomprehensible to the student at this stage in their learning, as intended by the author, form the basis of the student using the structures to learn about the meaning of these lines and as well the missing carbon and hydrogen atoms to make up the formula of glucose. This explanatory material occurs in the following pages. The rest of the book follows this philosophy. The book is not only about context with a historical background but also about pulling the principles of the science out of complex phenomena arising from application of these principles. This approach is one of the original aspects of the book, which is not only concerned with context and history but also with top-down learning in which the principles of the science are discovered by the student in the complex phenomena arising by application of these principles.

The sentence in the review "but what is there is little more than expanded versions of the side boxes found in other texts that are largely unconnected to the chemistry itself," is hard to understand if one looks at large numbers of organic chemistry textbooks with boxed in historical information. There is so much more in this book, in which the historical aspects are interwoven with the text, than any other beginning organic chemistry text in use today. Reading *Organic Chemistry Principles in Context: A Story Telling Historical Approach*, will demonstrate the truth of the claim.

Finally, there are the critical remarks in the review about use of the book and the necessity of other sources of information. It is amazing to make a criticism out of this fact without acknowledging that that book proposes such a use. Is the reviewer suggesting that such a use is not a good idea? Apparently not from what is written in the review. On page x in the introductory part of the book there is the suggestion that the book might serve well as

a supplementary text together with a more conventional textbook. This possibility is the primary reason why the price is so low ($25 for paper and $10 for e-book) to exactly allow such a possibility, which can bring history and context to the study of organic chemistry. To make a criticism out of something the book itself proposes as a use, without acknowledging what is in the book on that precise point, as if that were a problem, is especially unjustified. The low price and the suggestion for use as a supplement to enhance appreciation of historical aspects of the science could have been a point of praise.

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References and Notes

1. J. R. Partington, A History of Chemistry, Macmillan, London, 1964, pp 259-60.
2. A. J. Rocke, Image and Reality: Kekulé, Kopp, and the Scientific Imagination, University of Chicago Press, Chicago, 2010, p 121.
3. Ref. 2, p 199.
4. Nobel Foundation, http://www.nobelprize.org/nobel_prizes/chemistry/laureates/1954/

Response by Prof. Ramberg

Dear Editor,

The purpose of a book review is to tell potential readers of the book about its contents, the author's purpose, and to discuss the strengths and weaknesses of the book as written. As I was reading Prof. Green's book, I had truly mixed feelings about it. I admired very much the approach and the examples, as well as how Green completely reorganized the approach to organic chemistry. But my admiration was tempered by what I perceived as shortcomings, that in an all too brief review I could not present fully. I am glad to explain myself here in greater detail, allowing readers to decide if my review was accurate.

Wöhler and Vitalism

Green cites a passage from volume 4 of Partington's *History of Chemistry* (1) in support of his claim that Wöhler sounded the "death knell" for the vital force. But consider that the citations in the passage refer exclusively to the artificial nature of Wöhler's synthesis, which was cause for excitement among chemists at the time. Nothing in this passage actually refers to the fate of the vital force! Importantly, the pages from Partington cited by Green are in a section labelled "isomerism," and Partington himself does not discuss vitalism at all in this section of the book. Looking at "vital force" in the index, furthermore, shows only three relevant entries, one of which refers to Jakob Berzelius' concept of the vital force developed in his 1827 textbook that remained unaltered until his death, and another on Justus Liebig's concept of vital force developed in the 1840s. Partington's history cannot therefore support the claim about Wöhler's synthesis and vitalism, because he does not discuss the effect of Wöhler's synthesis on the vital force.

In the fifty years since Partington's encyclopedic oeuvre, historians have shown clearly that Wöhler's synthesis could not have been the demise of vitalism, because "vitalism" was not a single, comprehensive theory, but a variety of different theories about *biological systems*, and vitalistic theories continued to appear long after Wöhler's synthesis, as the examples of Berzelius and Liebig show. The idea that organic *compounds* possessed a mysterious vital force began to disappear at least as early as 1814, when Berzelius showed that organic compounds followed laws of constant chemical composition, albeit following different rules than inorganic compounds. By the 1820s, the principal stumbling block for the synthesis of organic compounds was not ignorance of a different kind of chemical force that held organic compounds together, but the greater complexity of the composition of atoms in organic compounds. For a more detailed look at the current understanding of Wöhler's urea synthesis, I would refer readers to John Brooke's 1968 article, Chapter 10