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.

Mark M. Green, Professor of Chemistry, New York University Polytechnic School of Engineering, mgreen@nyu.edu

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 of Alan Rocke's *Quiet Revolution* (1993), and my own articles on the meaning of the urea synthesis (2).

Linus Pauling and the Nobel Prize

Green notes correctly that the Nobel Prize citation about Pauling refers to both his work on the nature of the chemical bond and his successful application of that theory to various complex molecules. It is possible that the elucidation of the α-helix was the tipping point that resulted in finally awarding Pauling the 1954 Nobel Prize in chemistry, and I would not disagree with that claim. But I do disagree with what Green actually writes about Pauling's work and the Nobel Prize on pages 9 and 10. On page 9, Green describes the theoretical problem of tetrahedral bonding in the carbon atom that Pauling solved in the 1930s with the concept of hybridization. Green then writes that Pauling received two Nobel Prizes, one in 1954 and 1962, "neither of which was for his solution" to this problem. Green then mentions Pauling's equally important work on electronegativity, and then, finally (page 10), "And we still are not mentioning Pauling's contribution that won him his first Nobel Prize, for proposing a structural element of proteins, the α-helix." Given this narrative, what is an unknowing reader to conclude about the reason for Pauling's Nobel Prize? Pauling's prize was for lifetime accomplishment, including the α-helix, but not exclusively because of it, as Green explicitly argues in the text. After all, Pauling's general work on the chemical bond (which included hybridization, resonance and electronegativity) is noted first in the Nobel Prize citation.

Kekulé, Couper and Wurtz

In his book, Green recounts a story that Adolphe Wurtz "delayed" Archibald Couper's paper on the self-linking of carbon atoms, allowing Kekulé to publish the idea first and therefore get full credit. Green's book asserts (page 33) that Couper had prepared his paper on the self-linking of carbon atoms in 1857, and that Wurtz "delayed" its publication until 1858. On page 169, Green writes more forcefully that Wurtz "had blocked Couper's paper from appearing so that Kekulé received all the credit." This is, unfortunately, not how Alan Rocke's *Image and Reality* has described the event (3). According to Rocke, Couper had prepared his paper for publication in the spring of *1858*, and, as Rocke recounts *in the very passage quoted above by Green*, Wurtz could not present it to the academy because he was not a member, and the reading of the paper was delayed until Jean-Baptiste Dumas could present it later in the spring. In other words, according to Rocke's account, Wurtz did not actively "block" or "delay" Couper's paper as Green explicitly claims in his text.

Consider also Rocke's analysis of Couper's full paper (3), an analysis which needs to be considered closely together with the historical fact that Kekulé's paper appeared in print before Couper's. Rocke argues clearly that Couper's paper was likely read with very skeptical eyes, and only looks "correct" with hindsight. Chemists *at the time* found Couper's ideas and formulas too speculative and not sufficiently grounded in empirical evidence, no matter how modern they appear to us. This reluctance to accept Couper's paper in part led to giving Kekulé priority.

I should also note here that assigning Kekulé full credit because he published first is also somewhat problematic, because Kekulé himself was somewhat unclear on how to apply his principles and reluctant to present formulas graphically. In 1861, Aleksandr Butlerov argued more forcefully for the consistent application of Kekulé's principles to connect all atoms in the molecule to form a "chemical structure," a term that Butlerov coined. The message to students reading about this episode should be that similar ideas often appear simultaneously in different forms. Ideas and theories in chemistry that we take for granted today do not simply appear fully formed: they are shaped by multiple chemists within a specific historical context full of contingencies (like the factors that delayed publication of Couper's paper).

Green's comments on Kekulé's first publication on the structure of benzene are less problematic, particularly in light of his clarifying the manner in which Wurtz "sponsored" the publication. Still, Green's book says (page 169) that Kekulé's 1865 paper was "again sponsored by Wurtz," naturally leading the reader to assume that Wurtz sponsored Kekulé's work in 1858, which he did not.

I am glad that Green consulted Rocke's book in writing his own, but it is important to summarize accurately what Rocke (and Partington) has written. These examples of misreading or misinterpreting the historical literature are what prompted me to write in the original review that I *suspected* other *possible* historical errors in the book. I did not make a "global condemnation of the historical aspects of the book." I was informing readers that there *may* be misrepresentations of the historical literature, and that they should examine the book with that in mind. This is especially important, as Green does not, except in a few places, indicate his sources for the claims he makes.

Acceptance of Theories

Green cites several passages from his book to show that he understands that chemists did not immediately accept theories. The first passage, a caption from page 173, notes that "Objections to Kekulé's hexagonal ring structure for benzene required an explanation that was the equivalent to the concept of resonance." On page 173, Green describes, correctly, the problem with Kekulé's benzene formula, but then he describes Kekulé's solution to this problem as a proto-resonance formula, and then jumps to Linus Pauling's description of benzene in the 1930s, skipping seventy years of intense discussion by chemists about benzene's structure that did *not* involve resonance (4). The second example, involving the elucidation of the chair forms of cyclohexane, is less problematic, although the passage Green quotes raises the issue about why chemists thought of rings as flat for so long.

Pedagogical Issues

In my review, I noted that the book would come with a steep learning curve. I understand Green's pedagogical approach and described it in the review. My view is not that his approach is *wrong*, as he implies in his letter, but that it is different, and that not all students will benefit from it (5). Green's approach is admirable. I have myself thought about how to incorporate historical material into my organic chemistry lectures that would explain the epistemological foundations of organic chemistry.

Green is correct that there is certainly much more material in his book than in typical textbooks on the chemists who developed organic chemistry. My argument is not with the quantity of the material, but its relevance. For example, as noted by another reviewer, what lessons are students to learn from the story about R. B. Woodward's lack of sleep, his loathing of exercise, or his chain smoking during his marathon lectures (6)? It is useful, as I noted in the review, to present the material as a set of problems that chemists have solved. But how should students use the historical information Green presents to understand organic chemistry? What lessons should they learn about chemistry as a science? In his introduction (page xi), Green himself tells students not to worry about reproducing the historical material. If this is the case, then why is it there? Unless Green integrates this material fully into the chemistry and gives students a clear idea why each historical episode is important for understanding the nature of chemistry, the historical material serves as a distraction or a diversion.

Regarding Green's final point, we agree that his book could be used as a supplement to a traditional text. In neglecting to mention in my review that the author envisioned such a use, I did not intend to suggest that such a use of the book was a bad idea. But I did mean to express doubt, based on my own experience of twenty years of teaching organic chemistry, that the text would work as a stand-alone textbook. Two instructors from my department came to the same conclusion after looking through the book. Nevertheless, I would encourage readers and instructors to decide for themselves whether and how to use this book in the classroom.

Peter J. Ramberg, Truman State University, ramberg@truman.edu

References and Notes

1. J. R. Partington, *A History of Chemistry*, Macmillan, London, 1964, pp 259-60.
2. A. J. Rocke, *The Quiet Revolution: Hermann Kolbe and the Science of Organic Chemistr*y, University of California Press, Berkeley, 1993. J. H. Brooke, "Wöhler's Urea, and its Vital Force: A Verdict from the Chemists," *Ambix*, **1968**, *15*, 84-114. P. J. Ramberg, "Myth #7: That Friedrich Wöhler's Synthesis of Urea in 1828 Destroyed Vitalism and Gave Rise to Organic Chemistry," in K. Kompourakis and R. L. Numbers, Eds., *Historical Myths about Science*, Harvard University Press, Cambridge, MA, 2015 (forthcoming). P. J. Ramberg, "The Death of Vitalism and the Birth of Organic Chemistry: Wöhler's Urea Synthesis in Textbooks of Organic Chemistry," *Ambix*, **2000**, *47*, 170-195.
3. A. J. Rocke, *Image and Reality: Kekulé, Kopp, and the Scientific Imagination*, University of Chicago Press, Chicago, 2010, p 121.
4. See for example, chapter XII in C. A. Russell, *The History of Valency*, Humanities Press, New York, 1971, and S. G. Brush, "Dynamics of Theory Change in Chemistry: Part I. The Benzene Problem, 1865-1845," *Studies in History and Philosophy of Science*, **1999**, *30*, 21-79.
5. My review was not the only one to question Green's pedagogical approach. See H. Hopf, "*Organic Chemistry Principles in Context. A Story Telling Historical Approach*. By Mark M. Green," *Angew. Chem. Int. Ed. Engl.,* **2013**, *52*, 1365-1366.
6. S. R. Pruett, "Review of *Organic Chemistry Principles in Context: A Story-Telling Approach*," *J. Chem. Educ*. **2014**, *91*, 624-625.