Book Reviews

Peter J. Ramberg: *Chemical Structure, Spatial Arrangement: The Early History of Stereochemistry, 1874-1914*, Ashgate, Aldershot, 2003, 399 pp. [ISBN: 0-7546-0397-0]

One of the most extraordinary accomplishments of modern science is the ability of chemists to discern increasingly subtle details of molecular structure, an ability that had become extraordinarily sophisticated even before the end of the nineteenth century. The degree to which the magnitude of this feat is still poorly appreciated by most historians of science is a measure of how little its history has been studied. Long before physical methods of analysis, such as mass spectrometry or nuclear magnetic resonance, were invented, chemists had only traditional 'wet' laboratory procedures in their armamentarium, such as heating reaction flasks, distillations, recrystallizations, melting point and boiling point determinations, and so on. From such macroscopic manipulations and measurements, chemists gradually learned to deduce how atoms were connected in the molecules of a substance, how the pieces of a molecule were spatially situated with respect to each other, and even sometimes what their bonding angles must be. In short, the development of structural theory in three dimensions has been one of the great untold stories of the history of science.

Now it is no longer untold. Peter Ramberg provides here a full and expert recounting of the origins and early development of stereochemistry, the branch of chemistry that explores the spatial structures of molecules. The conventional birth date of stereochemistry is 1874, when Jacobus Henricus van 't Hoff in the Netherlands and Joseph Achille LeBel in France independently published similar papers hypothesizing a kind of molecular asymmetry that is created in any molecule possessing a carbon atom that is bonded to four different atoms or atomic groups. According to the theory, this asymmetry in structure correlates to asymmetric behavior of the molecules, especially the previously known fact that a solution of such a substance rotates polarized light passing through it. Van 't Hoff and LeBel pointed out that the theory provided explanations for various cases of isomerism that could be explained in no other way. The theory of chemical structure was already mature by this time, but there was considerable skepticism about the physical referents of the letters that chemists were writing in their formulas, and the precise relationship between paper formulas and the real, invisibly small molecules being manipulated in both the mind and the reaction flask of the chemist. Consequently, the first attempts to suggest that three-dimensional depictions of these semi-hypothetical entities were legitimate conclusions from empirical evidence were met with caution in many minds, and downright scorn in some. However, the new theory was defended strongly by the respected mid-career organic chemist Johannes Wislicenus at the University of Würzburg, and within fifteen years it had gained many adherents.

After bringing the reader up to speed regarding the development of the theory of chemical structure and two-dimensional explanations for isomerism up to the early 1870s, Ramberg explores the contributions of van 't Hoff and LeBel, then discusses the reception of the new theory of the 'asymmetric carbon atom'. A chapter then deals with the important subject of Wislicenus's work

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on unsaturated organic diacids, followed by two chapters dealing with crucial contributions by Victor Meyer, Arthur Hantzsch, and Alfred Werner, including the early history of the stereochemistry of nitrogen in organic compounds. Ramberg then discusses the work of Emil Fischer on the stereochemistry of sugars, before returning to Werner and his coordination theory in inorganic chemistry. Ramberg's narrative ends in the year 1914, when Werner announced the synthesis of an optically active inorganic substance. By this time it could be said that stereochemistry was mature, and few chemists troubled themselves any longer about philosophical questions of the reality of atoms and molecules, or the utility of three-dimensional theoretical images of molecular structure.

Along the way, we gain a new and deeper understanding of this important history in many areas. One example from early in the book is the reception of van 't Hoff's theory. Most organic chemists, and all historians of chemistry, are familiar with Hermann Kolbe's furious (and hilarious) tirade of 1877, which attempted to relegate the work of van 't Hoff and his defender Wislicenus to the category of mystical nonsense. The fame of this harangue, and the absence of early favorable notice other than that of Wislicenus, led historians to believe that the early reception of the asymmetric carbon atom was ignorance, indifference, and hostility. Ramberg shows that this impression is incorrect. Van 't Hoff's theory was received with cautious optimism by many chemists, although few came out for it in public early on. Among those who commented favorably in private, and later embraced the theory enthusiastically, were Adolf Baeyer, Adolphe Wurtz, Emil Fischer, Hans Landolt, Theodor Zincke, and Victor Meyer. Those who offered substantive public criticisms, such as Adolf Claus and Wilhelm Lossen, were careful to specify the limited range of their critiques, and did not enter the lists against the fundamental idea of the asymmetric carbon. Indeed, Ramberg's evidence supports his unexpected conclusion that the tetrahedral carbon "seems to have been accepted without any controversy at all, as there was little or no opposition to it" – other than Kolbe's critique, which was written off in professional circles as a madman's rant (p. 330).

Another example of an important new insight is Ramberg's treatment of Emil Fischer's work on monosaccharides. Contrary to uniform impressions of both chemists and historians of chemistry, Ramberg shows that Fischer's monumental accomplishment was not a preplanned assault to determine the stereochemical configuration of the glucose molecule, but more of a gradual affair that included many unplanned and unexpected turns. In this sense Fischer now resembles one of Frederic Holmes's protagonists following an unpredictable investigative pathway – an analogy that Ramberg notes. Ramberg's treatment of Fischer, like that of Wislicenus, Baeyer, Meyer, Werner, and Hantzsch, does not provide the final word on these important developments, but constitute an important step forward. His considerable accomplishment only reminds us of how much work remains before we can say that we truly understand, both in detail and in broad philosophical lines, how these extraordinary scientists achieved what they did.

There are some limitations of Ramberg's study that need to be noted. The discussion is almost exclusively limited to Germany, which is understandable since nearly all the late 19th-century stereochemical action occurred within the German-speaking chemical community. Ramberg notes that the reasons for this curious fact remain obscure, but he does not attempt to explain or even address it. A second issue is the technical character of the discussion. Ramberg writes that he has done his best "to introduce these details on a level suitable for non-chemists to understand" (p. 9), but I doubt that anyone without college-level organic chemistry will be fully able to follow the arguments, and I wish that Ramberg had done even more to help non-chemists. A third point is that the material is almost exclusively confined to the cognitive discourse itself – the chemical evidence and arguments, and philosophical points that emerge from it. It would be good to know more about other, more sociological, aspects of the story, especially how the various research schools and styles interacted with the cognitive content. For example, there is peripheral discussion of the more pragmatic, taxonomic, and non-theoretical style of such men as Baeyer and Fischer, but I would have appreciated a more careful study of the rise and development of this style. Among all these men, who knew whom and how well, how did the protagonists interact personally and in groups, what were the power structures, and so on? Such questions occasionally come into view, but are not dealt with systematically.

But the heart and soul of Ramberg's important project is the actual scientific content of the historical material, and Ramberg clearly knows what he is doing. He has fully mined the existing secondary literature, and has integrated it well into his analysis. He has also used appropriate archival sources in Germany, the United States, and Switzerland. You can trust his chemistry, as well as his sharp eye for philosophical implications of the science. In sum, this is an extraordinary accomplishment on a technically demanding subject. Peter Ramberg has uncovered a broad and deep current of important chemical science and philosophy that deserves careful study, and future workers in this field will be heavily in his debt.

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Jerome A. Berson: *Chemical Discovery and the Logicians' Program. A Problematic Pairing*, Wiley-VCH, Weinheim, 2003, xiii + 194 pp. [ISBN 3-527-30797-4]

The question posed in this book is of obvious interest to the readership of *Hyle*: Do philosophers of science provide useful roadmaps to professional chemists? While the author answers in the negative, arguably for wrong reasons (the function of philosophy is not jurisdiction over science), this essay nevertheless repays close study. The two attractions for philosophers of chemistry, and for chemists with an interest in philosophy, are the conflicting forces having fashioned this book and the case studies Professor Berson provides.

Let me start with the latter, in the sequence in which they appear. Kekulé's benzene theory is dealt with in chapter 4. Thank God, Berson spares us the probably *post hoc* dream of the snakes.

In 1865-66, Kekulé proposed for benzene the 1,3,5-cyclohexatriene structure. By 1869, Adolf Baeyer (1835-1917), Wilhelm Körner (1839-1925), and Albert Ladenburg (1842-1911) – all former students of Kekulé – criticized his formulation. It lacked self-consistency. As Ladenburg pointed out, Kekulé's argument hinged on a count of isomers, which ignored the lack of six-fold symmetry in cyclohexatriene. Kekulé salvaged his formulation for benzene with an ingenious *ad hoc* hypothesis, published in 1872. He animated the cyclohexatriene structure with the oscillations (exchanging single and double bonds, in modern parlance) that a regular hexagonal structure demanded.

Examined in detail, Kekulé's proposal is a fantasy: sequential, instead of random collisions of any atom with its bonded nearest neighbors. And yet it is a milestone in chemical thought. At the very time when the structural theory of organic chemistry was being put together, one of its leading progenitors took