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

the daring step of endowing atoms in molecules with motion.

This raises questions for the philosopher, such as: Why did Kekulé, with the restricted amount of data at hand, regarding the numbers of substituted benzenes of the various types, make the two intuitive leaps of an underlying invariant benzene structure, which moreover amounted to a regular hexagon?

The information he had did not fully warrant these conclusions: an assertion typical of some philosophers of science who chide a scientist for conclusions that go beyond the evidence. The attitude is all too familiar to scientists. Referees of their peer-reviewed papers take them to task likewise for over-interpreting the data. Having been the occasional butt of such criticism, I have had opportunity to consider this very issue.

Transgressions of the rules of evidence, *i.e.*, of the limits posed by inductive inferences, demarcate run-of-the-mill and imitative from innovative scientists. The latter mix in an element of imaginative, deductive projection. Theorizing from the data, they construct a model, more often than not implicit, from which they deduce properties to which the data ought to conform. This tricky topic of epistemological breaks, through creative intuitions, is worthy of the attention of philosophers, as pointed out long ago by Bachelard and by Kuhn. Chemistry provides them with a wealth of concrete examples.

Camphene racemizes partly when it is formed under acidic conditions from precursors such as borneol or isoborneol. Berson brings up this problem in the following chapter. Meerwein and van Emster made the observation in the early 1920s. They knew it to bear mechanistic significance. However, they failed to measure the optical rotations of several compounds involved in the reactions they dealt with. It was left to Houben and Pfankuch to clear up the whole matter, which they achieved most admirably in 1931-33. In so doing, they dealt a deathblow to Meerwein's 2,6-shift theory, which he had devised to rationalize his results.

The fascinating point here, which Berson makes from the strength of his in-depth familiarity with molecular rearrangements, is that both camps are in the right. Both mechanisms, Houben's and Meerwein's, operate in camphene racemization. The 2,6-shift also plays a role, a prominent role too, in other reactions. As Berson writes, "apparent refutations of a theory cannot logically guarantee that no confirmation eventually will be found" (p. 77).

To a large extent, this is flogging a dead horse. By this time, we all know that chemistry routinely goes beyond Popperian norms. Chemical science is far from being reducible to a game of conjectures and refutations.

In chapter 6, Berson returns to the axiom that underscored the benzene structure problem. Kekulé explicitly espoused a principle of minimal structural change, which he stated in his seminal paper of 1858. I do not have the space to develop the argument, which I will do elsewhere. There is a deep-seated analogy between a molecular radical and the root of a word. I hold such a linguistic analogy to be the basis of Kekulé's proposal, consonant with the invariance of the benzene nucleus in the aromatic derivatives that he studied. And this principle, of course one more avatar of Ockham's Razor, became sacrosanct; it opened up the whole field of structural elucidation of organic molecules through stepwise degradative analysis or, conversely, stepwise synthesis.

The challenge came from molecular rearrangements. Berson covers in detail the two examples of the pinacol rearrangement and of the benzilic acid rearrangement. In both, reactants and products differ in their carbon skeletons – which the protagonists of these two stories had great trouble coming to terms with. Their mental make-up prevented organic chemists, for a long time, from seeing the truth of the matter. There was no way Rudolf Fittig (1835-1910) could have known what he was doing when he

inadvertently stumbled upon the pinacol rearrangement in 1859-60. Charles Friedel (1832-1899) made a remarkable contribution. But it was left to Alexander Butlerov (1828-1886) to clear up the whole issue in 1873-74. The benzilic acid rearrangement had a similar history, in which the original 'discoverer', Auguste Laurent (1807-1853), had no inkling of what he had stumbled across in 1838, and which was definitively elucidated by Baeyer in 1877. Why such a length of time? Because, as Berson felicitously puts it, "it is as though the developments of chemistry took place in orthogonal domains of intellectual space" (p. 108).

Chapter 7 takes up "non refutative motivations in science": synthesis of organic molecules has an "inescapably confirmative purpose". And chapter 9, devoted to the Woodward hypothesis for alkaloid biosynthesis, which was fecund in spite of being totally erroneous, concludes these case studies.

As these examples show, the book could have been turned most profitably into an investigation of cognitive skills, using chemical pattern recognition as its guideline. Jerry Berson, who obviously started reading in the 1960s the then classics of epistemology (the dissonant quartet of Popper, Kuhn, Lakatos, and Feyerabend), chose to line up a few examples from chemical history in order to demolish the Popperian edifice. Is there such a need any longer? Professor Berson could have used his pulpit more effectively, had he addressed the epistemological findings by Herbert Simon (1916-2001); or had he chosen to launch a discussion with philosophers who talk to the concerns of our times, and whose subtlety ridicules Sir Karl (it was not his forte), such as Gilles-Gaston Granger, Ian Hacking, Hilary Putnam, or Susan Haack, to mention but these few names.

Which brings me to the goal pursued by Professor Berson: Whom does he write for? From the evidence of his highly technical if eloquent language, he addresses fellow chemists, which is perhaps not the best way to initiate a dialog with professional philosophers. One of the two conflicting forces at work here is an apologetic role. Berson sees himself as a go-between. He has the noble ambition of making chemists become aware of the importance of philosophical questioning. Yet, at the same time, Berson fictionalizes philosophical issues by treating them as so many hypotheses testable through factual evidence from chemical science. That he finds them lacking is unsurprising: Are the two fields commensurable?

To sum up, this book is both a small gem and seriously misconceived.

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