we test specific models. Inasmuch as things do not work, then we worry more about our adaptationist background as well as our model."

I do not agree with Ruse on this point, but his position is undoubtedly defensible. The only place I had a deep disagreement was in the history of ideas, with what Ruse has to say about Motoo Kimura and the neutral theory of molecular evolution — the idea that most molecular evolution proceeds by random genetic drift. Kimura's ideas receive only a brief mention before being dismissed.

I favour the view that Kimura's idea led to something of a paradigm shift in the late 1980s. Most biologists who work on molecular evolution nowadays assume that they are mainly studying the effects of random drift. This contrasts with earlier biologists who worked with non-molecular characters. Kimura's ideas seriously challenged, or at least restricted the scope of, adaptationism. If adaptationism could carry on much as before, it was for reasons that needed further attention. So Kimura deserves more than a passing gesture in a survey as admirably thorough and sensible as *Darwin and Design*.

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István Hargittai has interviewed many of the most influential chemists of the past sixty years, including Glenn Seaborg (top left), Ad Bax (second row, right) and Jean-Marie Lehn (third row, second left).

Chemical conversations

Candid Science III: More Conversations with Famous Chemists
by István Hargittai, edited by Magdolna Hargittai
World Scientific/Imperial College Press: 2003. 520 pp. £48, $78 (hbk); £21, $34 (pbk)

Arthur Greenberg

This book is the latest in a continuing series of interviews with prominent scientists by István and Magdolna Hargittai. The Hargittais are highly regarded structural chemists, located in Budapest, Hungary, whose wide-ranging interests and energy have produced a 'cottage industry' of books.

Candid Science III provides snapshots of the culture and progress of chemistry over the final 60 years of the twentieth century. Its oldest interviewee, Glenn Seaborg (1912–1999), was a nuclear chemist who added a new row — the actinides — to the periodic table. Seaborg was one of the team that discovered plutonium, and worked on the extraction of this metal for the Manhattan Project. His frustration with the naming of the trans-uranium elements is apparent in this interview from 1995. But in a postscript that was first published in 1998, Seaborg happily describes the history of the discovery of element 106, which was named in his honour in 1997.

The youngest interviewee, born in 1956, is Ad Bax, who runs the US National Institutes of Health's section of NMR (nuclear magnetic resonance) spectroscopy. From 1981 to 1997, the ISI logged 21,655 citations of his work, 50% more than the second-most-cited chemist, Nobel laureate John A. Pople. This emphasizes the amazing evolution over the past half-century of NMR spectroscopy from a physics experiment to a routine technique for structural proof, and finally to a method for solving protein structures in solution and for magnetic imaging. Bax, who is at the cutting edge of this work, briefly describes the integration of NMR experiment and computation that took off during this 50-year period, due, in part, to unimaginable advances in computer technology. These applications of NMR, along with advances in X-ray crystallography (also referred to in the interviews with Johann Deisenhofer and Robert Huber), contributed mightily to the emerging field of structural biology, which arguably began with the discovery of the structure of DNA in 1953.

Reading this book reveals the growing sophistication with time of the interviewer's technique. For example, Hargittai began his 1996 interview with Nobel laureate Jean-Marie Lehn as follows: "You started as an organic chemist", to which Lehn replied: "Yes, an organic chemist and, in fact, a natural products chemist." The interviewer's follow-up was: "Now you are also a very conceptual chemist. Not every organic chemist develops general concepts the way you do." Well, *bien sur*. Not many chemists of any stripe develop concepts like Lehn does.

In contrast, an interview two years later with another Nobel laureate, Bruce Merrifield, is more thoughtful and leads to some interesting insights. In discussing his early life in California during the Depression, Merrifield, who was seven years old when the stock market crashed, describes his family's economic condition: "So we scraped along, and that affected me all my life. I can't bear to waste things, I'm not extravagant, never buy anything on credit, and that came directly from the Depression." Two decades later, Merrifield developed solid-phase protein synthesis, a technique that essentially wastes none of the synthetic amino acids and peptides.

Most of the interviews are fairly straightforward affairs, but once in a while some sparks fly. For example, Hargittai starts the interview with Paul von Ragué Schleyer with a provocative implied question: "I recently heard you say that experiments are no longer necessary in chemistry. We can compute everything." Schleyer's response is forceful and there is a fascinating thrust and parry during the interview. The Schleyer interview tracks the career of this experimental chemist, whose early computations

Science in culture

The arsenic green

William Morris seemed indifferent to the fact that his wallpapers contained arsenical pigments.

Andy Meharg

William Morris (1834–1896) was a utopian idealist whose life was full of contradictions. He was a progenitor of the green movement and decried the environmental and human degradation caused by industrial activity. But he was also a successful capitalist who supplied the bourgeoisie with expensive interior décor. This paradox is most disturbingly evident in his intimate associations with arsenic.

His father, William Morris senior, helped set up the mining company Devon Great Consols (DGC), in its day the largest producer of arsenic in the world. William Morris used his income from shares in DGC to finance his design company Morris, Marshall, Faulkner & Co. (later Morris & Co). He also served as a director of DCG between 1871 and 1875, when arsenic production was at its height. But he was increasingly influenced by the growing socialist movement and resigned his directorship in 1875.

The environmental pollution caused by DGC was vast and persists to this day. Working conditions were atrocious. Workers suffered widely from skin lesions known as arsenic 'pock', which caused great discomfort, and many died from arsenic-related lung disease.

From 1867 onwards, DGC was the major supplier of arsenic for the production of green pigments following the synthesis in the late eighteenth century of copper arsenite, named Scheele's green after its discoverer. These pigments were widely used in wallpapers. In damp rooms, fungi living on the wallpaper paste turned the arsenic salts into highly toxic trimethylarsine. Arsenic pigments, which were also used extensively in paints and to dye clothes, paper, cardboard, food, soap, and artificial and dried flowers, were responsible for untold numbers of cases of chronic illness and many deaths.

Was Morris using the arsenic from DGC in his own products? Evidence to suggest that he was comes from correspondence with Thomas Wardle, his dye manufacturer. In one letter, dated 3 October 1885, responding to an enquiry from a concerned customer (Mr Nicholson), Morris dismissed the concerns of the medical professions and popular press about arsenic poisoning wallpapers coloured with arsenic pigments. "As to the arsenic scare, a greater folly is hardly possible to imagine: the doctors were being bitten by witch fever."

A second letter three days later stated: "Of course it is proving too much to prove that the Nicholsons were poisoned by wall-papers; for if they were a great number of people would be in the same plight and we should be sure to hear of it." This blasé response to the harmful effect that his wallpapers might have been having on his customers' health is remarkable, given the documented occurrence of arsenic poisoning at DGC.

To help me investigate the possible use of arsenic pigments in William Morris wallpapers, the William Morris Gallery in London kindly sent me a small piece of an early example of the 'Trellis' pattern wallpaper. The Trellis pattern is believed to be Morris's first wallpaper and was produced from 1864 onwards. I analysed the green pigment by energy-dispersive analysis and showed unequivocally that the coloration was caused by a copper arsenic salt. The beauty that William Morris wallpapers brought to a room must have had a health cost, at least in damp houses.

V&A PICTURE LIBRARY

Hidden danger: in damp houses, the green pigment in William Morris's wallpaper could have released toxic arsenic compounds.

It is easy to be harsh on William Morris for his double standards with respect to industry and pollution. In his defence, he was a product of his age, when environmentalism was in its infancy. He was actually a positive force in this movement. His political creed developed over several decades, and by the end of his life, when he was most revolutionary, his links with industry were in the past.

However, as a writer who demonized the industrial practices of his time as dehumanizing, he is almost as silent as a stone on his own role in the most polluting of industries, arsenic production. The Wardle letters — all that survive of his thoughts on his connections with arsenic — show only indifference, not regret.

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helped to rationalize interesting results, and whose later work using much faster computers predicted utterly non-classical organometallics, some of which were later verified experimentally.

Some interviewees offer insights that are worthy of further discussion. Jacqueline K. Barton is self-reflective and generous to students and colleagues in tracing her career path. Starting on tenure track at Hunter College in New York, she moved to Columbia University, where she was promoted to full professor, and then moved to the California Institute of Technology, where she holds an endowed professorship. She notes in passing that "the paths of woman professors tend to be non-traditional". Barton concludes the interview: "You can be a woman scientist in a major research institution and also be a person with a family and be happy." It is not a self-satisfied remark but rather active encouragement to young women pondering futures in science.

This book makes interesting light reading, especially for chemists who have watched the field develop over the past 30 to 60 years. One might wish for a preface that provides some integration and perspective, for example in considering the career paths of the three women interviewed (Barton, Mildred Cohn and Reiko Kuroda). Nonetheless, the book makes a worthwhile contribution to the oral history of science, and I recommend it for both libraries and individuals. ■

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