located at the back of the book, indicate references and sometimes offer detailed discussions. An alphabetical list of references and more such discussion would have been useful additions. The book also contains 24 pages of black and white photographs, predominantly of chimpanzees from the language projects and the Gombe River Stream.

Desmond considers man's close genetic relationship to the chimpanzee, citing 99% similarity in DNA nucleotides. Yet he does not note that the 1% difference appears to control timing and sequence of genetic expression, a mechanism by which organisms can develop very differently from each other. Furthermore, overall genetic similarity or dissimilarity need bear no necessary relation to the similarity of *particular* attributes of any two species. A dolphin is physically very different from man, yet *may* have certain cognitive abilities that are similar to man's. Desmond's comparisons between the Gombe River chimpanzee killing and man's act of murder, as well as many anecdotes from the ape language projects, are other examples of dramatic interpretations of data that need further analysis.

Desmond does raise important points. For example, in Desmond's consideration of the ape language work, Wittgenstein's aphorism, "If a lion could speak we could not understand him" (p.33), is quoted in reference to the naive assumption often made by trainers that chimpanzees use signs like 'please' and 'think' with the meanings assigned to them by humans (p.35).

Desmond also describes the recent investigation of Savage-Rumbaugh *et al.* (p.107) into the meaning of an utterance. Their chimpanzees seem to have the ability to use a lexigram or geometric design such as the design for 'key', in a highly specific context, but become bewildered upon its application to a wider array of contexts. That is, the chimpanzees seem to recognize that use of the lexigram for key will initiate a chain of events that eventually leads to their obtaining food with a key, but do not seem to understand that the lexigram is the 'name of' a key. Gradually some kind of understanding closer to that level does seem to be achieved, but the research makes it clear that a chimpanzee's (or young child's?) appropriate use of a 'word' in a limited number of contexts does not imply understanding of the word.

However, as Desmond notes, words *can* 'map on' to thought. Premack conceptualizes his 'ape language' investigation as permitting apes to use words to map out conceptual structures the animal already possesses. The end of the work is thus not to teach an ape language or to wonder how linguistic the apes' abilities are or are not, but to use the ape 'language' as a tool for revealing some of the apes' cognitive capacities.

Throughout, Desmond wisely emphasizes that we cannot measure apes with a human yardstick.

Another of the more positive parts of Desmond's book is his exciting account of the trail of discoveries initiated by analysing soil samples and leading, finally, to the discovery, in the molecules of soil, of flowers at the grave of a Neanderthal man. Here was an indication of some ceremony for the dead, a remnant of culture in so early a hominid.

In all, Desmond does consider fascinating issues and offers some insightful thoughts. But one wishes at least for less wordiness, more dispassionate writing and less uncertainty about the validity of his interpretations. And does the reader need to find phrases like "Oh Darwin, where art thou now?" □

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Coulson's Valence

Linus Pauling

Coulson's Valence. Third edition. By R. McWeeny. Pp.434. (Oxford University Press: Oxford, 1979.) Hardback £17.50; paperback £8.50.

A SIMPLE but very powerful theory of chemical valence and the structure of molecules, especially of compounds of carbon, was developed in the period between 1852 and 1874 by E. Frankland, F. A. Kekulé, A. S. Couper, A. M. Couper, A. M. Butlerov, J. H.van't Hoff and J. A. Le Bel. It was extended in 1893 by A. Werner to include complexes of metals, which he identified correctly from chemical evidence as representing tetrahedral, square planar or octahedral coordination of ligands about a central metal atom. An important contribution to the theory was made in 1916 by Gilbert Newton Lewis, who described the chemical bond as a pair of electrons held jointly by two atoms and who emphasized the stability of molecular structures in which each atom is surrounded by the same number of electrons as in a neutral noble-gas atom. Important contributions, such as the principle of electroneutrality and possible covalence as large as eight (for nickel in nickel tetracarbonyl), were made by Irving Langmuir from 1919 to 1921.

Despite some clarification introduced by the new ideas about the electronic structure of atoms and molecules, the theory of valence and the structure of molecules and crystals clearly remained incomplete. Many questions were asked in the first half of the 1920s that could not be answered; for example, why are the aromatic hydrocarbons more resistant to hydrogenation than other unsaturated hydrocarbons, why do complexes of palladium(II) and platinum(II) have the square planar structure whereas those of zinc(II) and cadmium(II) are tetrahedral, why are most compounds formed from the elements with a decrease in enthalpy, and most basic of all, why does sharing a pair of electrons between two atoms lead to a bond that holds the atoms together? The answers to these and many other questions were found in the period 1927 to 1933 through the application of the newly discovered theory of quantum mechanics to the problems. In 1927 Ø. Burrau published his quantum-mechanical treatment of the one-electron bond in the H_2^+ molecule-ion describing the first molecular orbital, and in the same year E. U. Condon treated the electron-pair bond in H_2 by the molecular-orbital method, and W. Heitler and F. London treated the same problem by the prototype of the valence-bond method. In 1926, and in greater detail in 1931, I introduced the concept of hydridization of bond orbitals, and between 1931 and 1933 I discussed the quantum-mechanical basis of tetrahedral, square planar and octahedral coordination, ionic–covalent resonance energy, the electronegativity scale, magnetic properties of complexes, the structure and properties of aromatic and conjugated molecules, and several other matters relating to valence and molecular structure.

Between 1928 and 1931 the quantum-mechanical theory of the resonance of a system between two or more possible structures and the consequent stabilization of the system by resonance energy had been discussed by John Slater, by me and by E. Hückel, and Slater had, in 1931, formulated his expression for the wave function representing an assigned valence-bond structure for a molecule. Many detailed quantum-mechanical calculations were being made of molecular properties by both the molecular-orbital method and the valence-bond method, and in 1931 both Salter and I published closely similar short papers to point out that, whereas these two methods in their simplest forms gave somewhat different results, when refined they approached each other, ultimately becoming identical.

During the 50 years since these events took place molecular quantum mechanics has made great strides. Many extensive calculations have been carried out, mostly with emphasis on molecular orbitals. I chose, however, to attempt to refine the theory of the chemical bond in a semi-empirical way, incorporating the ideas and principles of quantum mechanics. The results of this effort, in which I adhered as closely as possible to the ideas of classical valence-bond theory, were described in 1939 in my book *The Nature of the Chemical Bond* (third edition Cornell University Press: Ithaca, New York, 1960).

In the first edition of his book *Valence* (Oxford University Press: Oxford, 1952), Professor Charles Coulson emphasized the molecular-orbital description of electrons in molecules, with rather little discussion of empirical information. This policy has been followed by Professor McWeeny in preparing the much enlarged third edition. The result is that the book contains only a small amount of information about valence. There is no significant discussion of oxidation number, the range of values of the valence shown by many elements, and a number of other aspects of valence theory. Instead, as the author states in his preface, the book provides an introduction to quantum mechanics and a discussion of the application of quantum mechanics to the structure of molecules, with emphasis on the molecular-orbital method (which the author describes as having largely superseded the valence-bond method). The level of the discussion is for the most part rather high, but there are some surprising exceptions, such as the failure to derive the wave functions for the hydrogen atom. The book provides a survey of molecular quantum mechanics and of some aspects of structural chemistry. An isolated student without previous knowledge of quantum mechanics might find it difficult, but it could serve well as the textbook in a course, the purpose for which it was written. □

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Soviet innovation

Christopher Freeman

Science and Industrialisation in the USSR: Industrial Research and Development, 1917–1940. By Robert Lewis. Pp.211. (Macmillan: London, UK, 1979.) £12.

OVER the past 20 years the Birmingham Centre for Russian and East European Studies has established a high reputation for carefully documented research on Soviet R&D and industrial innovation. This book will certainly enhance that reputation and in many ways is even more interesting than its predecessors. Although it is entirely concerned with the inter-War years, the great interest of this period is that it was the formative time when the structure and procedures of the Soviet industrial innovation system became firmly established. It is also remarkable how many of the post-War debates in Western Europe were anticipated in the pre-War controversies in the USSR. Who remembered, for example, that the Rothschild "customer-contractor" principle was enthusiastically introduced in the Soviet institutes 50 years before its application here?

The new Soviet state inherited a science-technology system in 1917 which had many of the characteristics of today's under-developed countries — dependence on foreign technology and an almost complete lack of industry-based R&D. The Soviet leaders attempted from the outset to build up their own industrial R&D system, and the expansion in the 1920s and early 1930s was much more rapid than elsewhere. Indeed the Soviet Union has probably spent a higher proportion of its national income on scientific and technical activities than any other country for about half a century now. In absolute terms, this was, of course, still a fairly small total in the 1930s and as Robert Lewis is careful to point out "R&D" is only one part of the total rubric "science and technology". Definitions of R&D differ between Eastern and Western Europe and in any case were only formulated after the Second World War.

The first few chapters of the book document the growth of the Soviet science-technology system during the inter-War years with scrupulous attention to detail. (In a 200-page book, 50 pages are devoted to sources and methods.) The next few chapters review the organization of the system and the various attempts to control, coordinate and plan it. Finally, the last three chapters analyse this experience critically with a particularly interesting discussion in Chapter 10 on "Yesterday and Today". They seek to answer the question: why has the return on the massive Soviet R&D investment been so unsatisfactory?

Robert Lewis concludes that the major structural weakness of the Soviet industrial R&D system was the failure to build up a strong R&D capability at the enterprise level and that this weakness persists to this day despite a succession of attempts to overcome it. Shortages of skilled manpower and equipment as well as the desire to avoid expensive duplication dictated the early decisions to concentrate the industry-related R&D in central institutes for each branch of industry. Ever since then, there has been a continuous theme of complaint that the central institutes were too remote from the real problems of the factories and conversely that industry had neither the capacity nor the incentive to apply the results of the R&D conducted in the central institutes.

This problem is by no means confined to the Soviet Union or to centrally planned economies. Even within large private enterprise, it is a familiar theme of the R&D management literature. In one form or another it is likely to persist even in the best-managed systems as it reflects a real difference in preoccupation and pers-