best of the lot. It deserves a wide readership, both among historians of science and technology and among computer professionals, for whom the breathtaking pace of innovation and impact of computing have overshadowed the remarkable personal story of its beginnings.

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Software

History of Programming Languages. Papers from a conference, June 1978. RICHARD L. WEXELBLAT, Ed. Academic Press, New York, 1981. xxiv, 760 pp., illus. $45. ACM Monograph Series.

Modern computing systems are as much a product of software as of hardware. One of the most important parts of such software is the set of compilers and interpreters that enable a computer to be programmed in a so-called high-level language. The first such programming languages date from the early 1950's, and their history is as interesting as that of the computers whose usability they so greatly increased. In 1977, when the conference of which the present book is a record was held, comparatively little had been done to record or study this history. The conference was in fact a deliberate attempt to remedy this situation, at least with regard to a selected set of programming languages, namely ALGOL 60, APL, APT, BASIC, COBOL, FORTRAN, GPSS, JOSS, JOVIAL, LISP, PL/1, SIMULA, and SNOBOL. The reasoning behind this choice from among the literally hundreds of programming languages that have been designed and implemented in the last 30 years is quite understandable. Though each language had been introduced at least ten years earlier, all were still in active use and had undoubtedly had a major influence on the field of computing. Indeed, most present-day computer users will be familiar with, and have had their view of how computers should be programmed colored (for better or worse) by, one or another of the chosen languages. Very rarely, however, will they have any clear idea of the circumstances surrounding the development of the language or of the motivations and intentions of its developers. Any such user with even a modicum of interest in the past should find the present book fascinating, consisting, as it does, largely of accounts given by the original developers themselves. For example, FORTRAN programmers will learn that one of the major spurs to the development in 1954 of their language by a team at IBM led by John Backus was the advent of hardware for performing floating-point arithmetic. COBOL programmers will learn that its designers thought they were developing in 1959 just "a short-range composite approach (good for at least the next year or two) to a common business language for programming digital computers." And ALGOL programmers will get, all too clearly, an impression of the intensity of the debates that occurred in the international committee whose deliberations led to Peter Naur's magnificent ALGOL 60 Report.

This book is not, however, a mere collection of personal reminiscences by pioneers. Rather it is the outcome of a carefully organized process that sought to maximize the historical value of the conference. Thus for each language one, or in some cases two, of the leading figures in its original conception and development were invited to prepare a detailed written account of the origins of and rationale behind the design of the language. Each was given guidance as to the information it was hoped his or her paper would contain. This guidance, in the form of a lengthy questionnaire, covered both general matters and specific technical questions relative to the particular language and was complemented by a careful and constructive reviewing process.

Each author was, as a result, motivated to supplement his or her personal recollections by undertaking extensive historical research. The resulting accounts are all excellent and full of fascinating and often surprising information, though they are far from uniform in style and differ greatly in emphasis. For example, the accounts of languages designed by committees, such as COBOL and ALGOL, tend to stress the often painful process by which agreement on the various major features of the languages was reached. In contrast, the papers on languages that were essentially, at least initially, the product of a single individual typically concentrate more on the reasoning behind the various detailed technical decisions that were made; a prime example of such a paper is that by Kenneth Iverson on APL.

The papers were made available in draft form before the conference, at which ample time was provided for discussion and questions. The published proceedings include, in addition to the final versions of the papers, transcripts of the actual conference presentations and of the discussion sessions and technical summaries of each language. These last play an important role in helping readers to appreciate the accounts of programming languages with which they are unfamiliar. The book, therefore, provides, as was hoped, a very valuable and readable source of historical information on the development of some of the most important and influential programming languages. Yet for all its serious intent, the conference was obviously an enjoyable and entertaining affair, and this is well reflected in the present volume. Thus this is a book that should appeal not just to people with a serious interest in the history of computing but to anybody who has experienced the delights and frustrations of computer programming and who has an appropriate curiosity as to the origins of the language or languages that provide the arena for his or her programming exertions.

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Vertebrate Morphology

Basic Structure and Evolution of Vertebrates. ERIK JARVIK. Academic Press, New York, 1980. In two volumes. Vol. 1, xvi, 576 pp., illus. $94.50. Vol. 2, xiv, 338 pp., illus. $56.50.

Vertebrate morphology is not a fashionable subject. The textbook most commonly cited is still E. S. Goodrich's *Studies on the Structure and Development of Vertebrates* (1930), now more than 50 years old. The heyday of the subject was over by about 1920, when it seemed that the comparative anatomy and embryology of vertebrates were well enough understood for the framework of morphology to be permanent, so that attention could be turned to newer fields. Then in 1921 Erik Stensiö published the first of a series of brilliant monographs, analyzing fossil lower vertebrates in an entirely novel way. Before Stensiö fossil fishes had been treated much like fossil invertebrates—as shells, whose external features were sufficient for diagnosis. Stensiö's innovation was to treat fossil anatomy in the same detail as is found in classical morphologists' work on Recent fishes. By new and painstaking methods of preparing fossils and by close comparison with Recent adults and embryos, Stensiö reconstructed not only bone but cartilage, nerves, vessels, and muscles in group after group of Paleozoic and early Mesozoic fishes. Goodrich viewed Sten-

siö's early monographs with skepticism, believing that such detail was surely inaccessible in fossils. But others adopted Stensiö's methods, particularly those who gathered round him in the Swedish Museum of Natural History—the "Stockholm school," as A. S. Romer and his colleagues called them. Stensiö and his students applied the embryologist's technique of serial sectioning to fossils, making serial grindings from which the fossil was reconstructed in enlarged wax models.

Erik Jarvik came to the Stockholm Museum in the 1930's and took over from Stensiö the serial grinding of a skull of *Eusthenopteron*, begun in the late 1920's. That grinding series was completed in 1952, almost 25 years later. In 1959, Jarvik succeeded Stensiö as head of paleozoology in the Museum, and since retiring in 1973 has continued to work there daily.

Jarvik's book is in no sense a replacement for standard textbooks like Goodrich's. Rather it is an account of Jarvik's own work and that of his colleagues in Stockholm. The book shows hallmarks of Stensiö's approach—copious professional illustration (527 figures in the two volumes, mostly elaborate drawings made for the book) and scholarship, particularly in the older German literature, where so much fine work lies half-forgotten; there is a 58-page bibliography, repeated in each of the two volumes.

Jarvik's aim is "to summarize certain results which have been gained by studying the early fossil vertebrates in comparison with extant ones." As an introduction he presents a comparative study of one Recent fish, *Amia calva*, and one Paleozoic fossil, *Eusthenopteron foordi*, the Devonian fish on which Jarvik has spent so much of his working life. These accounts take up nearly 200 pages and 130 figures. The remainder of volume 1 is a survey of lower vertebrates, mainly the groups represented by Devonian and older fossils. In accordance with that principle, the only tetrapods included are the late Devonian ichthyostegids. These fossils, the sole Devonian tetrapods known, have been in Jarvik's hands since 1948, when Säve-Söderbergh, who discovered them in east Greenland, died. The book includes new information on the postcranial skeleton of *Ichthyostega*.

In volume 2 Jarvik turns to general topics. The first 100 pages develop the theory of vertebrate head segmentation generated over the last decade by Jarvik and his embryologist colleague Hans Bjerring. Then come a section on the origin of limbs and girdles and one on the origin of tetrapods, mostly summarizing Jarvik's previous publications but including a new theory of mammalian ear ossicles. Jarvik rejects the accepted homologies, which are widely regarded as the greatest triumph of comparative anatomy, and proposes that all three ossicles are part of the hyoid arch, the incus homologous with the fish interhyal and the malleus with the proximal ceratohyal. The book ends with a section of "recapitulation and comment." The comment is on principles for assessing relationship. Jarvik recommends workaday definitions of terms like "monophyly" and "polyphyly"; to him they depend on identifying one or more fossil ancestral groups. He does not find recent theoretical discussions of such terms "useful for my purpose." He has some hard words for cladists, who think they can "safely solve intricate problems of relationship and phylogeny, often without the burden of much knowledge." Jarvik plainly carries that burden, and it is germane to ask what he makes of it. He writes, for example, "Because it is impossible to say what the 'sister group' of coelacanthiforms is, it is impossible to include that group in a cladogram." Since others have had no trouble including coelacanths in a variety of cladograms, Jarvik uses "impossible" in a special sense here, and the sense evidently invokes final, unassailable knowledge. Indeed, he writes frequently of "safe opinion," "reliable conclusions," "safe and detailed knowledge," and so on. Among the safe conclusions that Jarvik summarizes are: The various groups of vertebrates show remarkably little change during their recorded history—they appear fully fledged in the Paleozoic and have hardly altered since. Cyclostomes are diphyletic, as Stensiö proposed in 1927, because lampreys can be derived from cephalaspids whereas hagfishes cannot. Recent work (Løvtrup, Janvier) arguing that cyclostomes are paraphyletic is not mentioned, though Løvtrup's book is in the bibliography. Acanthodians are elasmobranchs, related to sharks; lungfishes are plagiostomes, related to sharks, rays, and chimaeroids; and as for teleostomes, the interrelationships of the major groups such as actinopterygians, polypterids, coelacanths, and tetrapods (Jarvik recognizes seven distinct groups) are simply unknown. However, two things are known about these animals—frogs and amniotes are descendants of osteolepiform fishes whereas urodeles are descendants of porolepiforms, as Jarvik first suggested in 1942. These ideas, and these alone, Jarvik regards as firmly established. One gets the impression that safe knowledge is hard to find outside Stockholm.

Such knowledge depends, like all knowledge in comparative biology, on homologies, or, in Jarvik's terms, "safe homologies." But homologies are abstractions, or theories, and there is no known way of making theories safe. And there seems to be a paradox in Jarvik's conclusions: study of Paleozoic fossils has totally reorganized ideas of vertebrate interrelationships, yet those fossils differ hardly at all from their living relatives. Then how was the scheme of relationships built up by classical morphologists overthrown? In order to follow Jarvik's assessments, it is necessary to accept his interpretations of serial homologies in the head. These involve identification of serially homologous elements in terminal, premandibular, mandibular, hyoid, and branchial segments, including cranial vertebrae, and gill arches each furnished with supra- and infrapharyngeal portions, series of toothplates, and opercular bones. Through these interpretations, Jarvik arrives at an internally consistent theory, in which every hypothetical element can be identified. The analysis reminds me of Richard Owen's 1848 book on the homologies of the vertebrate skeleton, in which the head was interpreted by serial homology with vertebrae, ribs, and sternum. Owen also built up an internally consistent theory, whose essentials were expressed in a diagrammatic archetype, an ideal, hypothetical animal. In Jarvik's theory, *Eusthenopteron* seems to me to come close to that archetype.

Over the last 40 years, Jarvik has produced some of the most interesting and provocative work in morphology, summarized and extended in this book. The interest of his work lies in novel viewpoints (one Scandinavian said to me "Jarvik sees problems where no one else does") and in his way of following intricate ideas as far as they can be driven. In the past, Jarvik's ideas have certainly acted as a stimulus. Many a graduate student's teeth have been cut analyzing one of his arguments. That, I think, will be one of the main uses of this book: not as textbook, but as a reference work, an atlas of superb illustrations, and a source of epistemological or critical analysis for graduate students and professionals. Since the book weighs in at over five pounds, the price is believable, but as usual graduate students will have to borrow it.

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