of glycogen and other carbohydrate structures, is discussed in chapter 58. The synthetic function of glycogen synthase as opposed to the degradative role of phosphorylase is chosen as one of the best examples of the diversity of enzymatic systems in anabolic and catabolic processes.

Chapter 59 reviews the succession of experiments that proved that lipid synthesis was not a simple reversal of β-oxidation of fatty acids. The participation of a whole different set of enzymes, including a novel carboxylation of acetyl CoA by ATP to yield malonyl CoA as the active participant in fatty acid chain elongation, was indeed one of the most noteworthy surprises of biochemical research. Here the decarboxylation of the reactant is the driving force of the biosynthetic direction of the reaction. The complex reactions of triglyceride and phospholipid synthesis are also reviewed.

Chapter 61 continues the narrative of acetate utilization for the synthesis of the sterols and related natural products. Here, as was often the case, the initial experiments originated in the Columbia laboratories under the guidance of Rudolph Schoenheimer and his exceptional group of collaborators. The analysis in cholesterol of the carbon atoms derived from the carboxyl and methyl carbons of acetate implicated an isoprenoid unit and squalene in sterol biosynthesis and helped in the prediction of the correct pattern of the folding of squalene in these reactions.

The account of extensions of the isoprenoid pathways includes reviews on the biosynthesis of terpenes, carotenoids, tocopherols, ubiquinones, dolichols, rubber, steroid hormones, bile acids and alcohols, arthropod hormones, and insect pheromones.

The remaining chapters of volume 33A deal with the heterocyclic compounds, the porphyrins, the purine nucleotides, and the vitamins. Chapter 60 contains a detailed summary of how the complex pyrrole and corrinoids are formed. Again deductions from the origins of the carbon and nitrogen atoms of the pyrrole ring from labeled acetate and glycine provided the clues for demonstrating the central role of δ-amino levulinic acid in the synthesis of the pyrrole ring.

Volume 33B is devoted entirely to reactions of amino acid synthesis. In the 11 chapters of this volume the amino acids are classified into families according to metabolic origin. For example, ornithine, arginine, and proline are treated as part of the glutamate family, and urea and the pyrimidines are treated as extensions of this family because their ureido groups are formed by a pathway common to arginine. Thus, although pyrimidine biosynthesis is placed out of context with purine nucleotide synthesis, the vitamins folic acid and riboflavin are considered as extensions of the latter.

These two volumes cover such recent work that it is hard for this reviewer to relegate it to history. However, the period in question is rapidly passing as the emphasis in biochemistry is clearly shifting from clarification of metabolic pathways to investigations of complicated interactions at the macromolecular and cellular level.

Florkin's history does not include the biosynthesis of the macromolecules other than glycogen. Surprisingly, there is no mention of the synthesis of the deoxynucleotides, nor is appropriate space devoted to control mechanisms. This material may be taken up in a further volume projected before Florkin's death in the summer of 1979.

Florkin's history is undoubtedly the most complete treatise of its kind. It is detailed almost in the manner of a textbook, including valuable references and above all photographs of leading investigators. It avoids the personal and sometimes gossipy treatment of Judson's account of molecular biology, *The Eighth Day of Creation*. Yet Florkin's chapter on carbon dioxide fixation by heterotrophic organisms is outstanding because of the inclusion of letters and assorted information arising from previous historical research. Although documentation of the history of a discipline by the participating scientists themselves may yield at times a biased version of the story, it is indeed important that such personal histories be written, including anecdotes together with the investigators' accounts of their motivations and insights into their research. In many instances there is a certain urgency that these autobiographies be written to complement the straightforward approach that distinguishes Florkin's remarkable effort.

JOHN M. BUCHANAN
Department of Biology,
Massachusetts Institute of Technology,
Cambridge 02139

DNA Studies Brought Up to Date

DNA Replication. ARTHUR KORNBERG. Freeman, San Francisco, 1980. xii, 724 pp., illus. \$32.50. Revision of *DNA Synthesis* (1974).

A comparison of Arthur Kornberg's *DNA Replication* and the 1974 edition, *DNA Synthesis*, amply substantiates the author's prefatory statement that extensive new information warranted a new book. The first edition reflected Kornberg's focus on DNA polymerases and nucleases of *Escherichia coli*, and although it was an excellent treatment of nucleic acid biochemistry it suffered from some difficulties inherent in the presentation of up-to-date research in a rapidly moving field. The volume also appeared prior to the harvest reaped from nearly 20 years of research on DNA molecular biology. This bumper crop was presented at the Cold Spring Harbor symposium on DNA replication in 1978 (Watson-Crick model + 20 years). As a participant in that meeting, I recall my excitement and awe as new discoveries were presented that transformed problems hitherto unapproachable into those that had been solved, those that were being solved, and those that could be solved. Kornberg's decision to revamp the previous edition clearly reflects this enormous progress in DNA research.

The new edition is excellent. It is divided into 17 chapters, each of which covers a particular subject relevant to DNA synthesis. The organization of the book is logical; the first two chapters deal with the structure and function of DNA and the biosynthesis of DNA precursors (including the various pathologies resulting from deficiencies in some of these enzymes). The chapters dealing with DNA synthesis, DNA polymerase I of *E. coli*, and other prokaryotic DNA polymerases are similar to those in the 1974 edition. The only other chapter resembling one in the previous edition is the chapter on deoxyribonucleases. Even here, new nucleases are discussed and their modes of action presented and tabulated. The rest of the chapters are either new or so filled with up-to-date information that they are virtually new. The chapters on binding and unwinding proteins, replication mechanisms, and operations are clearly written, as are the chapters on eukaryotic replication. Because nature has devised different ways of initiating DNA replication, each system—T4, T7, and so on—is presented individually. Though our knowledge of the mechanism of eukaryotic DNA replication is limited, the new edition has an excellent summary of recent studies of

DNA synthesis in animal cells as well as in animal viruses.

One of the most valuable features of the book is its illustrations elucidating different concepts. The diagrams of DNA superhelicity (both negative and positive) and replication mechanisms are clear and for the most part self-explanatory. Wherever feasible, the book contains tables summarizing and comparing properties of nucleases, polymerases, DNA's, phage genes, bacterial genes, animal viruses, and so on. The illustrations and tables reflect the author's clarity of thought and ability to organize huge amounts of information into a coherent story. Areas of research now under intensive investigation are clearly identified and discussed with timely suggestions. This edition overcomes the limitations of the 1974 rendition. So much solid information on DNA metabolism has accumulated since 1974 that new results now represent a smaller fraction of our knowledge of DNA replication.

The book is enormously important for those of us who teach and carry out research. Research on DNA synthesis, replication, and repair has grown beyond the scope of any standard biochemistry textbook. *DNA Replication* is a "must" for all students of nucleic acid biochemistry. The book is a testimony to the importance (and ultimate relevance) of good basic research. It is of interest to note that the 1974 edition ended with a plea for genetic engineering and its potential for new discoveries; the new edition ends with the realization of this potential.

JERARD HURWITZ
Department of Developmental Biology and Cancer, Albert Einstein College of Medicine, Yeshiva University, Bronx, New York 10461

Letters Between Physicists

Wolfgang Pauli: Wissenschaftlicher Briefwechsel mit Bohr, Einstein, Heisenberg, u.a. Vol. 1, 1919-1929. A. HERMANN, K. v. MEYENN, and V. F. WEISSKOPF, Eds. Springer-Verlag, New York, 1979. lii, 578 pp. $80. Sources in the History of Mathematics and Physical Sciences 2.

In January 1925 Heisenberg, acknowledging receipt of Pauli's manuscript on the exclusion principle, wrote:

> Today I read your paper and it is certain that I take greater joy in it than anyone, not only because you have driven the swindle to an unsuspected swindling height . . . by introducing individual electrons with four degrees of freedom; but especially I am elated that you (et tu, Brute!) have returned to the land of the Philistine formalists.

Decoding this message rewards the effort. It expresses in Heisenberg's boyish humor his and Pauli's ambivalent attitude toward the atomic physics they sought simultaneously to improve and to replace. Six months after writing this letter, Heisenberg created matrix mechanics. The extraordinary correspondence under review documents this creation from its proximate beginnings in the late swindles of the old Bohr-Sommerfeld theory through the establishment of what some regard as the new swindles of the Copenhagen interpretation.

As terminus technicus "swindle" (*Schwindel*) signified a mixture of quantum rules and classical physics, in particular an ad hoc adjustment of the conditions defining the stationary states. Heisenberg had been an accomplished swindler from his first published work, in which he introduced half-integral angular momenta in order to account for multiplet splitting (*1*). Half quantum numbers then had no dynamical significance, as they eluded Bohr's correspondence principle (CP), which required numerical agreement between quantities reckoned according to classical procedures and those computed according to Bohr's rules in the limit of large quantum numbers. For example, when $n \gg i$, the classical harmonics $i\omega_n$ of motion in a Kepler ellipse of ground frequency ω_n should asymptotically equal the frequencies $\nu(n, n-i)$ of the radiation emitted in quantum transitions from the nth to the $(n-i)$th orbit. On this scheme a half quantum number made no sense.

"Philistine formalism" referred to proceeding without justification in terms of the physical model, without a grounding in the harmony of interlacing electron orbits, in the "atomic music of the spheres" (*2*). Pauli had once succumbed to this Philistinism, which he deplored as subversive of coherent physical theory. Hence Heisenberg's playful chiding: the exclusion principle seemed to him a Philistine swindle, Philistine in offering no explanation, based upon the model, of the unfriendliness of equivalent electrons, and a swindle in defining equivalence by introducing into the model electrons with four degrees of mechanical freedom.

Heisenberg's chiding was misplaced; it was prompted by his, not by Pauli's, brand of physics. Pauli had not endowed the electron with a fourth degree of mechanical freedom but with a "classically nondescribable ambiguity" that seemed to him to lie beyond the reach of the CP. Also, Pauli did not intend his new principle to be merely formal. He thought it ranked with the postulates regulating the stationary states and pointed the way toward a quantum mechanics without electron orbits. What he had in mind may be gathered from measures for reform that he recommended to his correspondents during the winter and spring of 1924-25.

Pauli urged the Bohr school to concede that the "language of models is not adequate to [describe] the simplicity and beauty of the quantum world." The CP alone could not bring what was needed, for it rested on the applicability of the classical concepts in the appropriate limit. Pauli's new principle indicated the sort of additional ingredient required: whoever succeeds in combining your "nonsense" with mine, he wrote Bohr, will have the solution to the quantum riddle. And how should one approach this odd summation? "Uniting this red and white rose will certainly require a fierce battle with our unconsciously held preconceptions." To begin with, he told Bohr, in a prescient formulation of the difficulty, one should hold to the old dynamics and seek a new kinematics. "I regard the angular momentum and the energy values of the stationary states as more real than the orbits." The Cheshire cat was to be constructed from its smile.

Bohr and Heisenberg at first rejected this radical and scarcely intelligible program. At Copenhagen they clung to what Pauli called the "imperialism of the correspondence principle." Perhaps, Bohr hoped, the four quantum numbers referred to electron orbits in the inner and outer parts of the atom; it was rash and premature to declare the incompetence of the CP. On the contrary, the "swindle" had been so well described that, with the help of the CP and the concept of virtual oscillators, the riddle might soon be solved. Pauli doubted that illumination was nigh and that light could ever come from virtual oscillators, the instruments (he said) of Bohr's "reactionary Copenhagen putsch" against the photon. As for Heisenberg, he continued