Systematic Analysis of Surface-Active Agents. M. J. Rosen and H. A. Goldsmith. Interscience, New York–London, 1960. xvii + 422 pp. $13.50.

As the preface states, the text, *Systematic Analysis of Surface-Active Agents*, is "an attempt to fill a need which has long existed for a comprehensive treatise on the analysis of surface-active agents." The authors have attempted to classify all the existing literature on the analysis of surface-active agents into sections such as Detection, Isolation and Estimation; Qualitative Analysis; Quantitative Analysis; and Separation. This they have done quite well, and the text provides a convenient catalog of existing literature.

A reviewer must find a weak point, and in my opinion, there is a lack of sufficient emphasis on instrumental methods of analysis, especially in the area of infrared absorption. The authors state on page 4; "Among the newer tools, spectrographic methods offer special promise, but much remains to be done in the field." Actually, infrared has been in use over 10 years in this field and is the most useful single tool one can apply. Ultraviolet analysis has been in use over 15 years. Both fields are quite fully developed. In my 12 or so years in surfactant analysis, I have found that over 90% of the unknowns received were identified by infrared analysis, separation techniques being applied to mixtures and then IR used on the isolated components. The chapter on qualitative analysis, spends about six pages on physical methods ($2^{1}/_{5}$ on infrared) and 74 pages on the "wet" tests. The main advantage of infrared is speed of analysis and conclusiveness. Not only can the surface-active agent be quickly classified, but it can also be actually specified exactly.

Ultraviolet analysis and x-ray diffraction are given but a brief mention (one or two pages each), and these also are valuable methods, though not so generally useful as infrared.

The reviewer gets the distinct impression that the authors are wet analysts or chemists, since the text leans so heavily on chemical tests.

I am sure the field will welcome this text, since it does serve to catalog the existing literature, especially the "wet" techniques. It may be possible in future editions to expand the instrumental portion of the text to best express the field in its true perspective.

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Epoxydverbindungen und Epoxydharze. A. M. Paquin. Springer-Verlag, Berlin, 1958. xx + 833 pp. DM 88.

The epoxy resins have been among the most rapidly maturing fruits of polymer science. In the mid-thirties, Pierre Castan discovered the curing of diepoxides with anhydrides or amines—condensation reactions proceeding with only small shrinkage and producing no volatile by-products. During the forties, intensive research in laboratories on both sides of the Atlantic led to outstanding new coatings, electrical potting compounds, resins for reinforced plastics, adhesives, and plasticizer-stabilizers. Commercial exploitation began in earnest during the last decade; and now, in the United States alone, half a dozen resin manufacturers vie with a hundred formulators and a thousand end-users to brighten the pages of the trade press with sagas of technological conquest.

Dr. Paquin has prepared a scholarly compendium of the science of epoxides and epoxy resins, with references going into 1957. Beginning with ethylene oxide, he presents a comprehensive summary of the preparations and reactions of epoxides which should make the book valuable to workers in many fields. Chapters on epichlorohydrin, polyphenols such as bisphenol A, epoxy resin intermediates, hardening and hardeners, and applications make up the main portion of the book. Discussions of testing and analysis, trade literature, patent lists, and indexes complete the volume.

Castan himself has provided a thoughtful and philosophical introduction.

Paquin's work differs in approach from two books entitled *Epoxy Resins* which have been published in the United States, one by Lee and Neville (McGraw-Hill, 1957), the other by Skeist and Somerville (Reinhold, 1958). Paquin has sought to be complete; the American authors have preferred to be selective and critical, with greater emphasis on formulation and end-use technology.

This book is a most convenient reference source for the chemist bent on making or breaking an epoxide ring.

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The Sequestration of Metals: Theoretical Considerations and Practical Applications. R. L. Smith. Macmillan, New York, 1959. vii + 256 pp. $8.50.

This book will be of interest to persons engaged in developmental research in agricultural, biochemical, pharmaceutical, photographic and analytical research, since the author has compiled a great deal of data relating to the sequestration of metals in these fields. His approach is simple, straightforward, and on a level that can be comprehended by anyone with an elemental knowledge of chemistry.

While reading the book, however, the reviewer was appalled by the number of typographical errors and little inconsistencies which occurred in the text. It appeared that a competent proofreader was needed rather than a reviewer. Many of the errors are inexcusable. A few of the more pertinent errata are as follows:

The author states, "A benzene group instead of a methyl group slightly *weakens* the acid properties." He cites the following dissociation constants to prove his point:

$$\begin{array}{ll} \text{Acetic} & k = 1.75 \times 10^{-5} \\ \text{Benzoic} & k = 6.3 \times 10^{-5} \end{array}$$

It is quite obvious that benzoic acid is actually a *stronger* acid than acetic, since its dissociation constant is larger. On the same page the author gives the value for the second ionization constant of succinic acid as $k_2 = 2.8 \times 10^{-4}$, whereas, the value should be 2.8×10^{-6}. The statement that, "The case of malonic acid is intermediate between oxalic and succinic," is true with respect to k_1 but is not borne out by the data in the case of k_2.

On page 56 the author has been inconsistent through use of K_2 and K_{11} for the same quantity. Furthermore, $(M^1)(Y^2) = (M^1)(Y)$ should read $(M^1)(Y)^2 = (M^{11})(Y)$.

At the beginning of Chapter Four, the author twice defines "stability constant" as the logarithm of the formation constant. However, he uses stability constant and formation constant synonymously on the following page and frequently throughout the remainder of the chapter. Most writers use the terms interchangeably. At one point he even introduces the term "normal logarithmic stability constants." According to his definition, this could only mean log log K_mY which is rather absurd.

On page 65 several plus signs have been omitted; this will be confusing to readers who are unfamiliar with the expressions. In the table on page 66, the author apparently means log K instead of K. On page 62 the page number is given as 26 and cadmium has been substituted for calcium. On pages 122 and 123, 10^{-15} has been substituted for 10^{15} in equations.

The author has been extremely negligent not only in disdaining to use Geneva nomenclature for organic compounds but also in not being consistent in the nomenclature he does use. In particular I would like to cite the following variations in word grouping, hyphenation, etc.:

amino triacetic acid
nitrilo triacetic acid
nitrilotriacetic acid

1:2:diamino-cyclohexane tetra-acetic acid
1:2-diaminocyclohexane-*N*:*N'*-tetraacetic acid
1:2:diamino-cyclohexane-tetra-acetic acid
1.2-diamino-cyclohexane tetra acetic acid
1–2 diamino cyclohexane tetra acetic acid
1:2:diamino cyclo hexane tetra acetic acid
1.2:diamino-cyclo-hexane-tetra-acetic acid

hydroxyethyl-ethylene-diamine-triacetic acid
hydroxyethyl ethylene diamine triacetic acid
hydroxyethyl-ethylene-diamine-tri-acetic acid
hydroxyethyl-ethylene diamine tri acetic acid
hydroxy ethyl ethylene diamine acetic acid
beta hydroxy ethyl ethylene diamine triacetic acid

1.2.diamino propane tetra acetic acid
1–2 propylene diamine tetra acetic acid
propylene diamine tetra acetic acid

Many other variations of this nature occur which should have been eliminated by careful editing.

The author will find that his conclusions regarding secondary or bimetallic chelates with EDTA are argumentative and will evoke a good deal of criticism from those who have measured the formation constants of such binuclear species as $Zn_2(EDTA)$ and $M_2(DTPA)$. Although the equilibrium constant for equation (2) is less than that for equation (1), it is appreciable, nevertheless, in many cases.

$$M^{+2} + Y^{-n} \rightleftarrows MY^{+2-n} \qquad (1)$$

$$M^{+2} + MY^{2-n} \rightleftarrows M_2Y^{4-n} \qquad (2)$$

One wonders by what reasoning the author can accept complexes of the type MY_2, MYA, MYA_2, $MYAB$, and even MHY while rejecting the possibility of forming M_2Y or NMY.

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Review of Textile Progress, Volume 10, 1958.

M. Tordoff and C. J. W. Hooper, Eds. Textile Book Publishers, New York–London, 1959. 494 pp. $8.00.

This is the tenth volume of *The Review*, a compilation and interpretation of textile work published every year since 1949. It is divided into 11 main chapters and subdivided into approximately 25 subtitles.

The difficulties in compiling the work published in the textile field during one year are enormous, but the contributors and editors have done very well in overcoming them. Obviously, papers of potential interest to the textile technologist include so many borderline cases that to decide whether or not particular items should be covered is often a difficult task. Other problems are presented by the delay with which certain sources, especially exotic ones, become available. Consequently, the 1958 review contains numerous references dated 1957 and earlier.

In the break-down of main disciplines, it is felt that the chapter on "Finishing of Textile Fabrics" should have a counterpart covering the finishing of fibers and yarns. Also, the division of that chapter into "Finishing of Wool Fabrics" and "Finishing of Fabrics other than Wool" indicates a slightly arbitrary handling. With all due respect to the achievements of the wool-finishing industry, one would expect a more detailed treatment of the other natural and the synthetic fiber types.

Some chapters, e.g., "Non-Woven Fabrics" (p. 432) or "Coated Fabrics" (pp. 432–433), are extremely short and can hardly be called exhaustive. It is realized that the contributors, most of whom are well known for the achievements in their particular fields and their thorough knowledge of the literature, were probably subject to space limitations and simply could not compress more data into their article. Nevertheless, a publication like *the Review* would profit by being less restrictive in future editions.

The *Review* serves a dual purpose: To assist the textile technologist who wishes to familiarize himself with the recent developments in his special field, and to help the searcher who attempts to locate a reference known to him only in a cursory way (or not at all). Both will find Volume 10, especially when used in combination with the earlier editions, an excellent work, invaluable for the purpose. The carefully arranged name and subject indexes permit quick location of references. Especially in combination with the Abstracts Section of the *Journal of the Textile Institute*, the *Review* gives to the textile research worker or technologist information comparable, though on a lesser scale, with the *Chemical Abstracts* or the *Zentralblatt.*

The sponsoring organizations and the extremely competent contributors should be thanked for their efforts which have made the *Review* again one of the most important publications in the field of textile documentation.

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