As a biologist, I must shudder that Wells left out such vital functions as assimilation, excretion, and egestion, but the description also smacks of an authoritarian intellectual soup that is not appealing to me. The impression of ultimate relaxation into a sort of final truth is heightened by Garfield's (*2*) statement that the *"Science Citation Index* is a harbinger of things to come—a forerunner of the World Brain" and by his devotion to the word "nirvana." Nirvana, of course, derives from the Sanskrit word meaning "to blow out." Webster (*4*) lists the usage of the word as "extinction of the flame of life," "the dying out of the threefold fire," and "oblivion to care and pain"—hardly an attractive prospect for a creative scientist.

There are other faint auras of authoritarianism in some discussions of information handling systems. As Wells puts it, "It is *Science* and not *men of Science* that we want to enlighten and animate our politics and rule the world" (in *World Brain*). The report of the President's Science Advisory Committee (*5*) draws an analogy that illustrates the point.

> Lawyers have used a citation index, *Shepard's Citations*, for more than 100 years. Each year Shepard's lists all appellate decisions that have cited any previous cases. Since the law is unified in somewhat the same way as is science in that the rule of precedent connects what happens later with what happened earlier, it is not surprising that a bibliographic tool so useful to the lawyer could also be useful to the scientist.

My understanding is that a precedent in law is an authoritative example, a judicial decision, and I doubt that most references cited in science would be so regarded.

Properly speaking, the major use of *Science Citation Index* would be to contribute to science. However, the advertising blurb (*6*) "What is the *Science Citation Index*" lists "other applications" that I wish I could sweep under the rug, but they should be reported in the interests of a complete discussion and as examples of "uses," many of which will be easier to put into effect than retrieval of scientific information. (The "other applications" are quoted in full.)

> Although the *Science Citation Index* was originally designed to be used for simple, direct retrieval of scientific information, it can facilitate utilization of the literature for sociological and historical evaluations and applications including:
>
> The evaluation of the impact of a paper, a man's total works, a journal, material published during specific time intervals, the works of students of specific teachers, works coming out of a university or department, work financially sponsored by a specific agency.
>
> The *Science Citation Index* may be used to good advantage in writing historical reviews and descriptions of the evolution of specific subjects.
>
> It may be used to study journal utilizations, measuring literature habits of scientists, effectiveness of specific journals in reaching specific audiences, purchasing requirements of specific libraries, library's need for maintaining files of most-frequently requested reprints, utilization of literature of one country by another, impact of scientific discoveries and inventions on technological development.

Misused, some of these "other applications" could cause some important practical and political difficulties, tending to foster the idea that what has been good in the past is best for the future. (A high "impact factor" means more support?) Garfield (*1*), who I assume is the author of these statements, discusses possible misuses of the *Index*.

Science Citation Index at the very least provides an easy way of looking at the complexity of modern scientific literature in an orderly fashion. It should facilitate a search for, and an answer to, the question (*7*) "Is the literature worth keeping?" posed by John Maddox:

> By its meek acceptance of the ponderous accumulation of the current literature, indeed, the scientific community has lent support to the somewhat Freudian view that scientists, collectively as well as separately, have come to regard this mountain of printed paper as their primary product. . . . Clearly, there is good reason for asking that some institution, and preferably one of the learned societies, should take the lead in a search for vastly improved methods of presenting scientific information to a scientific readership. Certainly it is ingenuous almost to the point of dishonesty that the scientific community should so persistently badger the librarians for more and more elaborate methods of cataloguing the scientific literature. . . .

References and Notes

1. E. Garfield, *Am. Documentation* **14** (4); 289-291 (1963).
2. ———, *Science* **144**, 649 (1964).
3. H. G. Wells, *World Brain* (Doubleday, Garden City, N.Y., 1938).
4. Webster's *New International Dictionary*, second unabridged edition (Merriam, Springfield, Mass., 1961).
5. A. M. Weinberg *et al.* (President's Science Advisory Committee) *Government and Information* (GPO, Washington, D.C., 1963).
6. What is the *Science Citation Index?* (Inst. for Scientific Information, Philadelphia, 1964).
7. J. Maddox, *Bull. Atom. Scient.* **19** (9), 14 (1963). I regret closing this discussion with a citation and quotation. Thinking about citation indexes has put me in a citation frame of mind, and, besides, Maddox says things with a nice flair. My use of citations in this discussion represents a habit I deplore—that is, the use of citations to express opinions. This seems to me legalistic and unjustified, and the presumption is that repetition of opinion leads to certainty. Garfield's article in *Science*, cited above is a case in point. Some 50-odd citations are made, many of them expressing opinion, but the largest single group comes from the author of the source article or from his associates. I suggest that this sort of reliance on authority does not give proper recognition to individual thinking—and after all the thinking of an individual is a very important component of the *advancement* of science.

Cabot Foundation Symposium

The Formation of Wood in Forest Trees. A symposium. Martin H. Zimmerman, Ed. Academic Press, New York, 1964. vxi + 562 pp. Illus. $16.

The second Cabot Foundation Symposium was held at the Harvard Forest (Petersham, Massachusetts) in April 1963. The purpose of the symposium was to bring together, for a few days in a quiet and peaceful place, a small but internationally constituted group of invited scholars whose work has had some bearing on anatomical, physiological, or biochemical aspects of wood formation.

This book is the record of proceedings of that symposium. It includes 29 papers, as well as transcripts of the interesting and enlightening open discussions that followed presentation of the papers. These discussions should assist the reader in orienting his own thinking and in evaluating some of the ideas presented.

The papers are grouped into sections dealing with (i) the evolution, anatomy, and activity of cambium and the anatomy and ultrastructure of its derivatives; (ii) the biochemistry of cambial derivatives, including biosynthesis of cellulose, lignin, and related compounds; (iii) the translocation of photosynthate to the cambium and the relation of translocation, storage, and mobilization of reserves to growth; and (iv) the internal and external control of wood formation. This last section is the most voluminous and includes discussion of hormonal regulators, mechanical pressure, water supply, and various light and temperature conditions as possible controlling factors. It, perhaps more than the first three sections, leaves one with the impression that the vast mass of available information is in continu-

ing need of ordered analysis and evaluation.

Although its title might suggest that the book is of interest primarily to tree physiologists, wood specialists, and forest botanists, it deserves a wider audience because of its broad approach and the depth of its treatment of the specific subjects covered. General botanists, plant anatomists, plant physiologists, and plant biochemists should find many of the papers more readable and more interesting than papers on similar subjects in technical journals.

The book is well printed, bound, and illustrated and has author and subject indices. Many significant literature references, including those to papers mentioned in the discussions, are provided. It seems unfortunate that the references to papers generally lack titles and sometimes lack inclusive page numbers. Such omissions are an annoyance to scholars who do not have direct access to the few great biological libraries. Complete bibliographic information is a great aid in deciding which works are worth borrowing on interlibrary loan.

The editor and publisher deserve praise for getting this well-produced book, quite free of errors, into print while the factual information and discussions that it contains are still relatively fresh.

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Chemical Technology

Methods of Quantitative Inorganic Analysis. An encyclopedia of gravimetric, titrimetric, and colorimetric methods. Kazunobu Kodama. Interscience (Wiley), New York, 1963. xiv + 507 pp. Illus. $22.

This book contains a comprehensive summary, complete through 1957, of published methods in the fields of inorganic gravimetric, titrimetric, and colorimetric analyses. The book is divided into three parts. Part 1, General Considerations (41 pages), contains brief information of a general nature on sampling, the solution of samples, separations, and determinations; part 2 (56 pages) is devoted to organic reagents used in inorganic analysis, classified by their reactions. Part 3 consists of more than 300 pages in which are outlined the determination of the elements, listed according to the classical order used in Hillebrand and Lundell's book. Each element is considered according to methods of attack, separation, and determination. The selection of methods appears to be good, and extensive use is made of tables and flowcharts. Sufficient information is given, in most cases, to permit selection of the method most suitable for a given problem.

The method of presentation and the good general index add to the usefulness of the book and the extensive index of organic reagents is especially valuable. Some 4000 references make this volume a virtual key to the literature of classical chemical analysis.

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Mathematics

Fundamentals of Scientific Mathematics. George E. Owen. Harper, New York, 1964 (© 1961, Johns Hopkins Press, Baltimore, Md.). xii + 274 pp. Illus. Paper. $1.75.

Although this book is very attractive and most ambitious in its purpose, it fails, on the whole, by trying to do too much. Its five chapters cover the topics of geometry and matrices, vector algebra, analytic geometry, functions, and differential and integral calculus. The audience is claimed to be high school seniors and college undergraduates, together with high school teachers of mathematics and science; few of them will have the background or the patience to follow the condensed treatment of such a book as this. It may serve as a review or as a new viewpoint on old ideas for one who knows most of the material, but it will teach little. For example, there are no exercises in the book, and who ever heard of learning mathematics without doing some? There should be a special bow, however, to the attractiveness of the illustrations and their general excellence. Only occasionally are they confusing or inappropriate (examples to illustrate the comment will be found on pp. 6, 7, 8, 11, 26, and 29). The first chapter is one of the poorest; the quality improves in the others, with the chapter on analytic geometry one of the best.

Mathematics is treated completely as a tool, with little evidence of an understanding of the logic of mathematics, its motivation, or its structure. This is a common failing with engineers, but not so common with physicists, and it leads to a cookbook approach to procedures which stress rules without reasons (p. 21). Although there is a great deal to be gained by using physical and geometric introductions to aid intuition, the clarity of the mathematics often suffers. Another influence of this mechanical approach to mathematics is found in such careless statements as the following: "Q is very close to P" (p. 20), and in an apparent confusion between the uses of approximate differentiation and integration and the exact uses of these processes. It is unusual to try to explain the approximate methods before explaining the exact ones which they approximate.

There are mathematical deficiencies and mistakes. The fuzziness of the language sometimes gives the impression that the book has been translated from a foreign language. Examples of awkwardness are found in the introduction of right and left handed coordinate systems (pp. 12, 13); the general discussion of coordinates (p. 16); cylindrical and spherical coordinates (p. 17); scalar multiplication (p. 65); intervals (p. 167); and derivatives (p. 207). Simple mathematical ideas are muddled—for example, the associative law, the distributive law, and the law of cosines (p. 68). Inexact expressions creep in: the "length element" (p. 18); and two vectors "form a plane" (p. 75). The idea of limit is poorly introduced (pp. 203, 207), and the concept of *function* is hopelessly out of date (p. 167). Basic words such as *definition* and *proof*, fundamental in mathematics, are not properly used (pp. 68, 219). Finally, a symptom of the lack of contact of the author with mathematics is his misspelling of such common words as chord (spelled cord) and paraboloid (spelled paraboid).

It is not clear what background is assumed for this book, or whether that background is appropriate for this type of book. There are places where trigonometry is assumed (pp. 19, 220), but also the summation notation (p. 28), determinants (pp. 33, 45), and the power series expansions for trigonometric functions (pp. 20, 211).

Some terms are used before they are explained in the book, or they are not explained at all. Will Coriolis ac-