ior—did not really emerge until around 1900 in the work of Boas and his students. One might reasonably expect a "history of theories of culture" to confront this issue. Harris does so in two ways: on the one hand, by defining culture so loosely that, in his own words, "in this sense, a *de facto* concept of culture is probably universal"; and on the other hand, by ignoring completely the existence of my articles. The issue, of course, is not one of personal pique, but of historiographical consequences. In this case, they include, among others: a failure to appreciate fully the positive theoretical significance for cultural anthropology of Boas's critique of racial determinism; certain misunderstandings of 19th-century racial thought (which are compounded by Harris's need to condemn Spencer, Morgan, Tylor, and even Theodor Waitz as "racists" while exonerating Marx from the same charge); and a virtually complete neglect of the German roots of the culture concept (Herder is mentioned only once in the whole book).

Anthropology's Milieu

The issues I have discussed so far are related to the first of Harris's two instrumental purposes. Let us now turn to the second: the *reasons* for the failure to give cultural materialism a fair hearing—which Harris finds in the "covert pressures of the sociocultural milieu." The crucial passage on this issue comes at the conclusion of Harris's discussion of Marx and Engels: "With Morgan's scheme incorporated into Communist doctrine, the struggling science of anthropology crossed the threshold of the twentieth century with a clear mandate for its own survival and well-being: expose Morgan's scheme and destroy the method on which it was based."

In developing his argument, Harris rejects Leslie White's attempt to identify antievolutionary anthropologists directly with "reactionary and regressive" political currents. He acknowledges Boas's liberalism, and the radicalism of many others. The impact of reaction is a bit more indirect. It is manifested in the fact that Kroeber's salary during his first five years at the University of California came from the mother of William Randolph Hearst: "Under these circumstances, it is difficult to imagine how someone from the left end of the intellectual spectrum could personally have achieved a strong institutional base, much less how he could have advanced a whole new branch of learning against its many competitors." If cultural anthropology developed "in *reaction* to, instead of independently of, Marxism," it was, apparently, because the likes of Mrs. Hearst kept a close watch lest traces of cultural materialism express themselves in the University of California Publications in American Archaeology and Ethnology, and because left-liberal anthropologists, realizing which side their bread was buttered on, attacked Morgan so that anthropology would not succumb from lack of Mrs. Hearst's largess.

I do not mean to suggest that the political economy of anthropology, or the nature of its institutional base, or the political ideology of its personnel is irrelevant to understanding the development of anthropological theory—or to deny that anthropology has, like the social sciences generally, developed in relation (if not always in reaction) to Marxism. Indeed, I am inclined to agree with Harris that various factors have operated to prevent the open-minded consideration of Marxist hypotheses in anthropology. But it is one thing to believe this and another to "demonstrate" it. Anyone who consults the manuscript sources of American anthropology in the period Harris is referring to will see that its relation either to American capitalism or to Marxism cannot be dealt with in simple terms. Far from "demonstrating" in any systematic way that "covert pressures" have affected anthropology, what Harris has in fact done is to offer a number of asides to a basically "internal" intellectual history—some of them suggestive, some of them simplistic, but all of them of a rather *ad hoc* character, based on a textbook knowledge of general history, or on anthropological gossip, or on Marxist preconception.

This carries us to the last point of my criticism: the contradiction between Harris's anthropological theory and his historiographical practice. One might reasonably expect that someone who was trying to establish a "science of history" in anthropology would approach the intellectual history of his discipline in a fairly rigorous manner. Now I am not exactly sure what an "etic" or "nomothetic" approach to this problem might be. But if "etic statements depend upon phenomenal distinctions judged appropriate by the community of scientific observers," there are many of Harris's historical statements which would seem to me not to measure up. On the contrary, to paraphrase Harris's own comments on "the threat of politics" to scientific objectivity, "it is clear that a history which is explicitly bound to a polemical program is dangerously exposed to the possibility that the values of that program will gain the ascendency over the values of history."

GEORGE W. STOCKING, JR.
Departments of History and
Anthropology, University of Chicago, Chicago, Illinois

Classics of Chemistry

Source Book in Chemistry, 1900–1950. HENRY M. LEICESTER, Ed. Harvard University Press, Cambridge, Mass., 1968. xx + 408 pp., illus. $11.95. Source Books in the History of the Sciences.

Classical Scientific Papers—Chemistry. Arranged and introduced by DAVID M. KNIGHT. Elsevier, New York, 1968. xxiv + 391 pp., illus. $11.75.

Discovery of the Elements. MARY ELVIRA WEEKS. Seventh edition, revised and with new material added by HENRY M. LEICESTER. Illustrations collected by F. B. Dains. Journal of Chemical Education, Easton, Pa., 1968. x + 896 pp. $12.50.

Today the classics of science, like the Bible and Shakespeare, are more often quoted than read, but this has not always been true. Goethe recognized that "the history of science is science itself," and Kekulé spent much time reading the classics of chemistry before making any scientific contributions of his own. Of late, there has been an upsurge in the publication of classic scientific papers. Two of the volumes under review are cases in point.

The first collection, *Source Book in Chemistry, 1900–1950*, translated, edited, and provided with commentary by Henry M. Leicester, includes, either in their entirety or in part, 91 classic papers by 123 authors "in all branches of chemistry—papers upon which contemporary research and practices are based." A continuation of and companion volume to Leicester and Klickstein's *A Source Book in Chemistry, 1400–1900* (Harvard University Press, 1952), an indispensable, standard work now in its fourth printing, this latest effort of Leicester's will undoubtedly be greeted with the same acclaim met with by its predecessor, for it possesses all of the earlier volume's advantages and none of its shortcomings.

The new work serves as a veritable mirror reflecting the trends character-

istic of 20th-century science—the increasing pace of research, the growth of multiple authorship, the support of foundations, the classified status of much research, the rapid diffusion of knowledge and application of discoveries, and the increasing role of nuclear science. The growing interdependence of the different branches of chemistry and even of different sciences that was one of the outstanding characteristics of the first half of the 20th century is admirably illustrated. Not only the better-known classics of the period are included but also many lesser-known works worthy of note. Inasmuch as Leicester "hoped that these selections will be of value in years to come to the general historian of chemistry or, more widely, of science," he avoided selections that are almost entirely mathematical, such as the original papers of Debye and Hückel or Heitler and London. Instead, he wisely chose later works of these authors which "explain the ideas behind the mathematical symbolism." This attractive, readable volume will not only interest the historian of chemistry but will also be a valuable supplement for today's texts, most of which unfortunately have little space available for the historical dimension of chemistry.

The second collection, *Classical Scientific Papers—Chemistry*, is an oversized, luxurious volume containing 31 facsimile reproductions of famous scientific papers by 24 authors on the development of 19th-century chemistry, specifically on theories of matter. It chronicles, through the words of the participants in the controversy, the vicissitudes undergone by the atomic theory in the course of its development from John Dalton's initial statement of 1808 to Jean Perrin's demonstration of the existence of molecules by his 1910 interpretation of the Brownian motion.

The general reader may be surprised at the extent to which leading chemists of the 19th century expressed doubt not only as to the existence of atoms but also as to the elementary nature of the elements. Reading these polemical papers will give him a deeper appreciation and understanding of the nature of scientific progress, which in this book is portrayed not as a spiral ascending steadily upward but as a series of ups and downs with more than one cul-de-sac—for example, Sir Benjamin Brodie's Chemical Calculus. Among the authors of the selections, which include not only journal articles but also lectures, excerpts from books, and one encyclopedia article, are such "greats" of chemistry as Dalton, Thomson, Wollaston, Berzelius, Davy, Liebig, Faraday, Dumas, Graham, Kekulé, Williamson, and Ostwald, as well as some lesser luminaries. This is not just another anthology of discontinuous selections. The papers are intimately linked, and several of them are discussions or criticisms of other papers in the collection. Thus a continuous story with drama and excitement emerges.

A few admittedly minor but annoying shortcomings mar this otherwise handsome volume. Although the authors, titles, and complete references for each selection are cited in the table of contents, this information is not repeated at the heads of the individual papers, and the result is ambiguity in several cases. In many cases additional, unrelated material has been included in the facsimile reproductions; but more seriously, though in a lesser number of cases, pertinent material such as footnotes (for example, on pp. 83, 319, and 332) has been inadvertently omitted.

A new edition of an old favorite, *Discovery of the Elements*, for the sixth edition of which Mary Elvira Weeks received the 1967 Dexter Award, can paradoxically enough be reviewed in one paragraph, not because of its unimportance but because its value is well established and recognized by all. This reviewer can do little to add to the praise which other reviewers have accorded to earlier editions. This definitive and unique work originated from a series of articles in the *Journal of Chemical Education* and thus suffered from episodic and irregular organization. In this latest edition, Henry M. Leicester has corrected this shortcoming by rearranging the material and reducing the number of chapters from 31 to 21. In so doing, he has made the work a true book, in spirit as well as in format. Virtually every page bears signs of his revision; he has missed no opportunity to update this standard work even in the smallest of details. Although he has added new material, he has succeeded in reducing the number of pages from 910 to 896, a feat that other revisers might do well to emulate. A veritable treasury of documentation and citation from original sources, the book contains a fantastic number of references (2688), and its 373 well-chosen illustrations add to its utility.

GEORGE B. KAUFFMAN
Department of Chemistry,
California State College, Fresno

Mesophase for Biologists

Liquid Crystals. Proceedings of an international conference, Kent, Ohio, Aug. 1965. Coordinated by GLENN H. BROWN, G. J. DIENES, and M. M. LABES. Gordon and Breach, New York, 1967. viii + 486 pp., illus. $30.

The 29 papers in this volume have been republished unchanged from five numbers of the journal *Molecular Crystals* for 1966 and 1967. The reason for collecting and reissuing these papers, which were originally presented in 1965, would seem to be that of reaching a wider audience than the regular readers of *Molecular Crystals*. Certainly a large part of this potential audience is biological. Biologists have often been fascinated by what might be called the "meso-phenomena"—such as mesomorphism and semiconductivity—of physics, sensing in them a relevance to biological phenomena which is, perhaps, intense in proportion to the inscrutability of the latter. There are indications that the role of the liquid crystalline state in biology—once described as chaotic—is becoming narrower but more secure. Biologists are no longer excited by the mesomorphic behavior of viruses in water or polypeptides in dioxane. Muscle has outgrown its mesomorphism for at least the reason that a classification is not an explanation. Finean has deleted the description of the myelin sheath as a liquid crystalline structure from the second edition of *Biological Ultrastructure,* perhaps to signify that not all ordered noncrystalline materials are liquid crystals.

But whether membranes should be called mesomorphic is secondary to the considerable convergence of the physical chemistry of lyotropic mesophases and membrane biophysics, to which the Mueller-Rudin-Tien bilayers bear witness. Although the Luzzati phase transitions are currently overshadowed by the transport-mediating antibiotics and the membrane proteins remain a mystery of ever-growing importance, the relevance of the lyotropic mesophase as a macrohomologue continues to be impressive.

Liquid crystals, then, have their place in biology, and so does *Liquid Crystals.* Not meant to supplant Gray's monograph on thermotropic systems or to provide a complementary treatment of lyotropic systems, the book presupposes some familiarity with the mesophase and the techniques for studying it. Nevertheless, most of the contributors