Book Reviews

Strategy in the Missile Age. Bernard Brodie. Princeton University Press, Princeton, N.J., 1959. vii + 423 pp. $6.50.

In the nuclear era there is less excuse than ever—if there ever was any—for thinking of strategy as the science of conducting a war that has already started, toward an end that is taken for granted. Strategy must be as much concerned with the *threat* of force as with its application. Deterrence, nuclear blackmail, limited war, accidental war, arms agreements, and the problem of credibility epitomize the modern meaning of strategy.

So defined, strategy is a backward science. We do not even have a rudimentary common terminology to cover the different cases of deterrence that are continually being discussed. In these circumstances, it may underrate Brodie's book to say that it is the best available. There are remarkably few good books on strategy, and even few books at all. Part of the reason is beautifully laid out in Brodie's early chapters. This is an "intellectual no-man's land," traditionally avoided by the military who have shied away even from strategy in the more restricted sense. It is equally true, Brodie points out, that the civilian side of government has been reluctant to equip itself to deal with strategy.

There is nothing very strange about this. We do not expect business executives to produce management science or navigators to produce astronomy; and we should not expect military officers, whose jobs require them to be executives, engineers, bureaucrats, and leaders of men, to produce a body of theory. It may well be the lack of an academic counterpart to the military profession that explains the slow development of strategy. Part 1 (about 150 pages) is a perceptive and absorbing review of the role of strategy and strategists from Napoleon to the Strategic Bombing Survey, with particular emphasis on air strategy during the period since World War I.

Brodie's book is at once the most advanced comprehensive treatment of strategic problems and the closest thing to a textbook in a discipline so unorganized that it lacks even this mark of respectability. Part 2 (roughly 250 pages) is a knowledgeable, balanced, and responsible discussion of current strategic issues as they involve the long-range striking forces of the United States. (This includes limited war, but principally in its relation to the strategic background—a background that ordinarily receives too little emphasis.)

Part 2 may not contain much (except evident wisdom) that is altogether novel to anyone who has kept up with what little good literature has been produced in recent years, a literature to which Brodie himself has been a main contributor. Issues that can be discussed within 250 pages, even lucid and well organized pages, are bound to sound pretty basic and familiar to professionals, unless the author has some favorite gimmicks. Brodie has no gimmicks. But press conferences, congressional hearings, and the writings of military commentators so rarely come up to the intellectual level that can be achieved within the scope of 250 intelligently written pages that Brodie's book might be considered advanced reading. For the nonprofessional, whose stake in these matters is only his life and civilization, *Strategy in the Missile Age* is intellectually accessible, written with a dignity and good taste that inspire confidence in the author's sense of responsibility, and enlightened by a rare touch in the use of historical perspective. (The introduction is a masterpiece.)

If a distinguishing theme or message has to be singled out, it is that the most urgent problem, now and continually, is to ensure the invulnerability of our strategic retaliatory forces, which cannot deter an attack on the United States if they can be destroyed in the act they are intended to deter, and which can hardly rely on anticipating an attack and retaliating in advance. Brodie's main premise is that general war is possible, terribly possible if our strategy is poor, because the technology of modern surprise attack may not make the initiation of general war anything like suicide unless the side being struck has taken this problem seriously, and because the very fear of being second might at some time make going first seem a conservative policy for the enemy, unless he knows that our strategic forces need not strike first to avoid their own destruction. In the last 12 months or so, this concern has spread rapidly in the United States, but the opposite view was widely taken for granted scarcely 2 years ago.

To those who, influenced by the accounting practice of modern press conferences, already think of Atlas as yesterday's weapon, or to whom the commissioning of the first ballistic-missile submarine (without its missiles) makes land-based weapons uninteresting, the book may seem old fashioned. It even discusses airplanes. But anyone worried about the several years we must live through next, and interested in decisions still to be taken for the years after that, will find the analysis not only timely but a reminder that we cannot get beyond the near future without living through it. And before anyone anticipates the early obsolescence of Brodie's analysis he should reread Brodie's own chapters in *The Absolute Weapon*—a book he edited less than a year after Hiroshima, chapters that looked way over the horizon into the nuclear era. His record is awfully good.

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Methods of Experimental Physics. vol. 6, *Solid State Physics*. Part A: Preparation, structure, mechanical and thermal properties. xvi + 466 pp. $11.80. Part B: Electrical, magnetic, and optical properties. xiv + 416 pp. $11. K. Lark-Horovitz and Vivian A. Johnston, Eds. Academic Press, New York, 1959. Illus.

In this 900-page, two-part volume something is said about the techniques for making essentially all physical measurements in solid state physics. Each of the 68 experts contributing to

the volume has written a chapter attempting to cover his own field. Since this averages out to 13 pages for each contributor, most techniques can only be treated superficially. Nevertheless, enough references are cited so that anyone undertaking to make a measurement in solid state physics will find this volume a good starting point. Although there is no other book like it in the field, I cannot recommend it for individual purchase since only a small fraction of the volume is devoted to the average experimentalist's problems. However, any library utilized by about four or more experimental solid state physicists can favorably consider its purchase.

Because of the brevity of each chapter, this volume will be of limited use to the researcher who must decide what experimental technique can best be applied to his problem, but having chosen a technique, he can well begin his literature survey here. While the foreword to the volume states that "Indications of limitations of both applicability and accuracy are an important part of this presentation," this intention is fulfilled only in the outstanding chapters, for many chapters fall far short of yielding any such indications. In addition, a newcomer to a field will glean little information concerning the accuracy generally obtainable by various experimental methods.

The book appears to be well edited and fairly complete in broaching experimental solid-state measurements. It is an important contribution in its field.

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The Lost Cities of Africa. Basil Davidson. Little, Brown, Boston, 1959. xvi + 366 pp. Illus. $6.50.

Africa. Its peoples and their culture history. George Peter Murdock. McGraw-Hill, New York, 1959. xiii + 456 pp. Illus. $11.75.

Both George Murdock and Basil Davidson write about the pre-European periods of Africa, but their books are quite different. Since the latter's book is of the more familiar genre, perhaps it would be best to discuss it first. Basil Davidson, a journalist with a long time interest in African archeology, attended both conferences on African history and archeology at the School of Oriental and African Studies (1953, 1957), and he gives us a useful popular survey of archeological research on the entire continent. This is very welcome, especially since, aside from the reports of the above conferences, rather more dryly written, and a slight book in French by de Pedrals, there is nothing of this sort on a continental scale, and even regional surveys, where they exist, are apt to be out of date.

Archeological reports are difficult for the layman to read, and since no archeologist has seen fit to provide a summary of African excavations for the general reader, Davidson is to be congratulated for undertaking a task that many must have shrunk from because of its magnitude and because interpreting a field in which the material is still scanty presents many pitfalls. So far as I can judge, he has come through well, and for areas where I am familiar with the original reports, he has adhered scrupulously to the data. I am afraid that some readers may have doubts on this because of the style of the book. The author's earlier books are mostly of a polemic and political nature, and archeology does not lend itself so well to polemics. Davidson wishes to demonstrate that the African has produced some cultural accomplishments, and even though there are many benighted readers who do not understand this, it is not as radical a statement as the author seems to think. If it is not yet "old hat," at least it does not bear repeating so often in a book of this kind. The effect may be to make the archeology seem to be the appendage of the argument rather than the other way around and, thereby, to raise doubt concerning the veracity of the actual data. This would be unfortunate.

The literature on the kingdoms of the western Sudan is quite extensive, and A. J. Arkell's writings provide an adequate account of the eastern Sudan, but Davidson adds an interesting chapter on the middle region in between. The "Azanians," still very much of a problem even as to identity, are discussed. Zimbabwe is reviewed, and other well-known and many less-known ruins pass before the kaleidoscope. Davidson has made his book indispensable to students of the general history of Africa.

Africa is based in large part on a method that has been relatively little employed for historical reconstruction, and never, to my knowledge, on such an extensive scale. Murdock has attempted to trace the spread of domesticated plants from known places of origin to their present distribution. Since agriculture is the basis of the Neolithic culture on which all subsequent civilization is built, there is a good rationale for this approach.

Other kinds of data, particularly linguistic data are used for historical inference, but these data seem to come in unevenly. It could, in fact, be argued that the book would have been more effective, or that it would at least have had a clearer line of argument, if it had been restricted to a consideration of plant "cultigens." The present state of knowledge makes it well-nigh impossible to bring together the various types of materials that might be useful in historical study of the African peoples; therefore, we need laudable onslaughts on a particular kind of material, such as Murdock has made on the botanical data, until we have a series that can be compared and cross-checked and, finally, interwoven. When one method is used extensively and another scantily, the impression is created, erroneously perhaps, that the minor thread is used only when a crutch or a sign-post is needed, or when a bit of evidence is too well known to be ignored.

This is the feeling that arises when we are told that the spread of the Bantu-speaking peoples was made possible by plants of Indonesian origin that came onto the continent on the Indian Ocean coast and then moved across the continent on the northern fringe of the forest before being used as a means of penetrating the forest *from the west.* Greenberg's classification of African languages leads to the conclusion that the people of Bantu speech originated in the Cross River-Benue region (Nigeria). Therefore, the plants *had* to go westward north of the forest; otherwise the speech of the forest peoples would have a different affiliation. Was this the reasoning? We would like to know whether the interpretation came strictly on botanical grounds or on linguistic, but it is impossible here to disentangle them. Had Greenberg delayed his classification until after this study had been made, and considering Harry Johnston's dictum that the Bantu originated in the area of the East African lakes, would we have been told that the Indonesian complex of plants began its penetration of the forest in the east? As an explanation of plant distribution, it is the simpler one, but it is not now compatable with the necessary explanation of language distribution. One would like to have a clearer