Although "new" synthesis is not the object of the book, the authors point out several instances of convergence into higher-order principles of findings from several fields. Two illustrations are given—one is from findings on adaptation level in experimental psychology, social psychology, and sociology, and the second, called the "spiral finding," describes the "positive feedback" effects of social deprivation and deterioration.

Many readers will be surprised that the work of the great psychological and social theorists is not prominently reviewed here. The authors feel that the great names, such as Freud, Wundt, William James, Max Weber, Durkheim, and Simmel, lie behind most of the findings reported but that these theorists were followed by those who took on the task of verifying or disproving. The findings of the book generally come from the work of the latter group rather than from the theorists.

The empirical image of man that emerges from this combining of psychology, sociology, and anthropology will, no doubt, disturb many readers. Those who place "reason" at the forefront of human behavior will not find confirmation and support in these pages. Those who believe in biological inheritance as a major determiner of human behavior will find that "behavioral science man is social man—social product, social producer and social seeker." Those who believe in self-interest as the primary motive behind human behavior will discover that man is a "creature making others and made by others."

The critical reader, who does not demand confirmation of a particular philosophical view of man, will still be concerned about two rather serious deficiencies in the behavioral sciences model of man, namely, (i) it sounds too much like the U.S. early 1960's model to be representative of mankind and (ii) it seems quite full of holes and gaps, particularly in areas of interest to the artist and the humanist.

The authors recognize and deplore the fact that the "hard" data of the behavioral sciences are often based solely on modern Western Man, and most often on inadequate samples of North Americans. The holes and gaps are excused on the basis of the relative youth of the behavioral sciences and the criteria, such as replicability, for establishing a "fact." I would add that the level of financial support by the federal government for the behavioral sciences (about 2 percent of the total federal budget in the support of sciences) is certainly related to their currently underdeveloped state in relation to the physical sciences. The behavioral sciences have fared somewhat better at the hands of private sources of support, and, for a while, they were the subject of a special program in the Ford Foundation.

A book review is no place to report original research by the reviewer, but I cannot resist the temptation to report the results of my empirical investigation on the operational meaning of the name, "behavioral sciences." One of the many good outcomes of the Ford Foundation program was the establishment of the Center for Advanced Study in the Behavioral Sciences at Stanford, California (a center somewhat like the older Institute for Advanced Study at Princeton, New Jersey), where scholars from the disciplines making up the behavioral sciences could come to study together for a year. A list of the names (414) and the disciplines of the fellows of the Center for the years 1954 to 1963 makes it possible to generate the 1046th "finding"—that some 66 percent of these scholars come from five disciplines—19 percent from psychology, 14 percent each from anthropology and sociology, 11 percent from political science, and 8 percent from economics. Only one scholar identified himself as a behavioral scientist! These results do raise a question about the lack of coverage of economics and political science in Berelson and Steiner's book. However, since both of these disciplines are heavily theoretical in nature, it is not inconsistent to find them well represented at the Center for Advanced Study in the Behavioral Sciences but essentially ignored in preparing an inventory of empirical knowledge in the behavioral sciences. But what are the behavioral sciences?

This is an important book, a well-written and a very well-organized book (the authors comment that it is easier to study than to read!), a book dedicated to a new and promising level of integration for some of the scientific disciplines that deal with human behavior.

An African Mesolithic Series in Northeastern Morocco

La Nécropole Épipaléolithique de Taforalt (Edita, Casablanca, and Institut de Paléontologie humaine, Paris, 1962. 183 pp.), by Denise Ferembach, with J. Dastugue and M-J. Poitrat-Targowla, deals with human skeletal remains discovered at Talforalt in northeastern Morocco, in a Mesolithic context dated by carbon-14 as extending roughly from 12,000 to 10,500 B.P. They represent 80 adults (39 male, 31 female, and 10 doubtful), 6 adolescents, and about 100 individuals under 16, including 45 less than 1 year old. Forty-one skulls, 11 with mandibles, plus 19 isolated mandibles were reasonably complete. Maximum age at death was about 40 or a little over. The first 102 pages are devoted to a minutely detailed bone-by-bone discussion of this material, including teeth, with excellent illustrations and tables of measurements with statistical constants. Seldom if ever has a human skeletal series been so well described. Throughout the author emphasizes the physical resemblances between the population of Taforalt and the well know Afalou series, both of which were culturally "Iberomaurusian" (=Mouillian=Oranian), in contrast to their at least partially contemporary Upper Capsian neighbors who were physically "Proto-Mediterranean."

The inhabitants of Taforalt were big, strong people, purportedly of "the same racial stock" as the series from Afalou (eastern Algeria) and Obercassel (Germany). That they constituted an inbred isolate the author deduces from their apparently long occupation of a single cave, a very high rate of *spina bifida* and related sacral defects, very high infant mortality, and the universal presence of wormian bones. And I think she is right. Unfortunately she also cites, as an example of rapid gene spread within an isolate, an old French Protestant community (Sutter and Tabah, 1951) in which "a gene producing epilepsy had attained practically all the families" before the end of the last century. No form of epilepsy, however, is positively known

to be hereditary, and the diagnostic differentiation from hysteria must have been most uncertain at that time and place.

Next Ferembach examines possible racial relationships and origins. She rejects my hypothesis that the Afalou series represents a hybrid population, involved in a process of genotypical homogenization, of which the Taforalt series possibly represents a later stage. On the contrary, she argues that both are representatives of a single, uniform, racial subtype, but that, as each population was an isolate, certain minor divergent characteristics were developed in each locality. This argument turns in part on the implied assumption that the two series were fairly closely contemporaneous—but no level at Afalou has yet been dated. It turns also on a painstaking analysis of the statistical validity of differences between representatives of my postulated ancestral types, although the small numbers involved invalidate all such analyses.

A comparison of the Taforalt means with those of my Northwest African Mesolithic total series, which includes practically all the other known specimens from both "Iberomaurusian" and Upper Capsian sites, reveals an extraordinarily close general correspondence. My total series is much more variable of course, while Afalou occupies an intermediate position in this respect. These facts, together with my visual impression of the Taforalt material, which Ferembach very kindly showed me, lead me to think that I am right. But the real truth of the matter is that we simply do not have enough material available to justify definite conclusions. Have Ferembach and I gone too far in piling up hypotheses? Only further discoveries can tell, and it is possible that they may answer in the affirmative. In any case, Ferembach has given us a most admirable description of the biggest African Mesolithic series yet discovered, and she has given us also much food for profitable thought, not to mention a remarkable multilingual bibliography.

In dealing with the pathological aspects of the Taforalt skeletons, Dastugue points out that on the whole they seem to have been very healthy. He found no traces of rickets, osteomyelitis, skeletal tuberculosis, or cancer and almost no congenital defects aside from the frequent sacral anomalies already mentioned. Fractures too were exceptionally rare by prehistoric standards; only two individuals had serious head wounds, and one of them had survived long enough to heal satisfactorily. Osteoarthritis, however, and particularly hypertrophic spondylitis, were very common, but this was so in many other prehistoric populations. Two partially healed parietal lesions are interpreted by Dastugue as scars of trephining, and at least one obviously is. One is reminded of the curious symmetrically paired scars on the Gambetta skull (Balout and Briggs, 1949; Briggs, 1955). One woman with multiple injuries that must have practically immobilized her survived until after her broken bones had healed completely, which shows that care of the hopelessly infirm was not unknown. One wonders why such seemingly healthy people leading a sheltered sedentary life should die so young in general and so often shortly after birth, but, as Dastugue points out explicitly, there is no way of knowing with only bones as evidence.

Poitrat-Targowla's description and discussion of the dental pathology is both detailed and very interesting. Evulsion of one or both upper central incisors was universal. Out of 677 other teeth examined, 66 showed hypoplastic pitting. In one case an apparently nonsyphilitic, "mulberry"-like, hypoplastic condition characterized the second molars of both jaws. These phenomena, caused probably by severe fever during early childhood, fit well with the high rate of infant mortality. The author is inclined to attribute them primarily to the nutritional stress of weaning, but I fail to see any particular reason for this assumption. Only 40 teeth were carious, but others had undoubtedly been lost before death. Of the jaws with teeth still in place, 28.7 percent had periodontal or alveolar (periapical) abcesses, the latter often caused by caries that penetrated the pulp cavity. Generalized marginal periodontitis was a more serious problem, for it seems to have affected nearly 60 percent of the population. In short, the people of Taforalt had a lot of trouble with their mouths and teeth.

In closing I must say that even where I disagree with the authors' propositions, I still think that they deserve thoughtful consideration. Everyone engaged in the study of prehistoric human remains would do well to read this monograph.

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Cultural History

Myth and Cult Among Primitive Peoples. Adolph H. Jensen. Translated by Marianna Tax Choldin and Wolfgang Weissleder. University of Chicago Press, Chicago, 1963. x + 349 pp. Illus. $8.75.

This translation of Jensen's *Myth and Cult among Primitive Peoples,* originally published in 1951 and now available in English translation with a new author's preface, makes more accessible to American readers the current thinking of the so-called Viennese school of anthropology on the subject of primitive religion. The Viennese school carries on the traditional German and central European emphasis on culture history as the preeminent problem of ethnology. Concern, therefore, is directed at the historical explanation of contemporary phenomena rather than at psychological or structural-functional explanations. This tradition was, in its early days, dominated by the *Kulturkreislehre,* a school of thought that envisaged culture history as a temporal succession of globally distributed culture types. One of the tenets of many workers in this tradition was *Urmonotheismus*—the primitive and pristine divine revelation of God's word to early man, of which later pagan religions were degenerate descendants, and which was rehabilitated in the words and deeds of Jesus.

The newer historical school, which Jensen represents, no longer adheres to the *Kulturkreis* dogma and does not take a doctrinaire position on primitive Christianity and *Urmonotheismus.* In many ways, Jensen's viewpoint strikes a sympathetic chord to the American ear; he asks for recognition of primitive man's humanity, his rationality, and his psychological continuity with modern man. And he develops the interesting thesis, echoing Oswald Spengler in sophisticated fashion, that authentic, spontaneous, genuine religion, which is produced by a human group in intuitive response to its reality, degenerates over time into selfish and utilitarian application of the cultural forms (myth and ritual) which once were alive and vibrant expressions of wonder, mystery, and awe.

But the scientifically inclined reader is also disturbed by a mystical theme whose influence on the development of the argument may have been considerable. Jensen believes that magic is