



NEW ZEALAND
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NEW ZEALAND ARCHAEOLOGICAL ASSOCIATION NEWSLETTER



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6 September, 1961.

Mr. R.J. Scarlett, Editor
New Zealand Archeological Society Newsletter
Canterbury Museum
Christchurch, New Zealand.

Dear Sir,

In your Newsletter, Vol. 4, No. 2, March 1961, appeared Bulmer's review of my book, Island Civilization of Polynesia. While I am gratified that the book is receiving attention, I should like to take the opportunity to comment on a number of Bulmer's often personal statements, and, at the same time, clarify a few matters pertaining to the book and the views expressed therein.

First, in Bulmer's condescending opening remarks, the reader is told that the book is at best a bit premature; further, that it reflects only one man's opinion and that this opinion is highly colored by my own field work. Dismissing the amateur depth psychology, I must respond that if Bulmer had read the introductory and concluding chapters of the book she would undoubtedly have seen my own statements on this score. I am, peculiarly enough, in a position to realize the temporary status of any conclusions and have expressed myself clearly on this point (Suggs 1960, p. 11), stating that the interpretations in the book are my own alone, that they do not represent a dogma, personal or otherwise, and that any new addition of fact can and will undoubtedly change them. The concluding chapter on future areas for research as well as references to lacunae in the data scattered throughout the volume indicate the dynamic state of our knowledge of Polynesian prehistory. Having stated these qualifications myself, I would prefer to receive credit for it, rather than have my own remarks presented by the reviewer as her own. If Bulmer did not read the chapters and statements in question, she should not have reviewed the book; if she did read them and has nonetheless failed to report my qualifications of my own work, "intellectual dishonesty" is perhaps the most polite term to describe her methods.

The remarks over the prematurity of the book are amusing: the same whining cliche' characterizes many reviews by graduate students and biased professionals in American journals of all disciplines, as well as in the popular press. What is usually intended is: "It is premature for anyone except the reviewer (or the reviewer's idols) to publish." There is no need for any scientist to apologize for a general or popular summary of any field, no matter how immature its literature is.

A matter of more importance is Bulmer's view of the status of the book. From the very beginning, it is assumed that the book represents a full-dress scholarly publication. Bulmer criticizes illustrations, footnoting, and bibliographic practices, saying that the latter are not scholarly. In what I trust was a flight of feminine whimsy, she holds up Penguin as a standard of some sort for technical and popular writing. Throughout the rest of the review the matter of standards of scientific writing comes up again and again, particularly in her discussion of my Marquesan work.

Eschewing the tempting discussion of how one who has done so little writing can suddenly become an expert on standards, I shall attempt to clarify the status of the book, although to an ordinarily perceptive reader, I frankly fail to see why clarification is required. To begin with, I have stated in my introduction that the book is for the general reader and that it does not enter into the minutiae which are of importance to the specialist (Suggs, *ibid*). For ordinarily perceptive readers, a statement of this type is sufficient. Further, New American Library is not in the business of publishing, in its Mentor Series, scholarly volumes on any subject, as should be evident from its Madison Avenuesque motto: "Good reading for the millions." The Mentor Series is intended for the informed laymen and college student segments of our reading public. The volumes are intended to provide basic acquaintance with a broad range of topics such as: Zen Buddhism, major world religions, schools of philosophy, oceanography, relativity, astronomy, the Classics and ancient civilizations. Even the most elementary examination of the Mentor Series will show that none of these books are what could be strictly termed "scholarly works." With few exceptions they do not have elaborate bibliographies (see as extreme examples Lord Russell's ABC of Relativity (Mentor 1959) without bibliography or footnotes and Rachel Carson's volumes The Sea Around Us (Mentor 1954), Under the Sea Wind (Mentor 1955) and The Edge of the Sea (Mentor 1959) none of which contains a bibliography and only one of which has a reading list); and they are not heavily illustrated (a result of the nasty real-world of production costs). Furthermore, the illustrations that are provided are generally not elaborate: it is difficult to crowd a highly detailed map or line drawing into a page of the Mentor size without losing a bit! Despite the formal shortcomings of the Mentor line, Bulmer may be interested to know that they are most highly regarded by educators throughout the world (as indicated by distribution figures and personal comments) for precisely the reason they were intended: providing a general acquaintance with a given topic or field.

One of the most humorous aspects of the whole review (intentionally, I hope) is the elevation of the Penguin series as an example of technical writing which also has appeal to the public. Having read all of the Penguin archeologies I find this statement most amusing. First, the content of the Penguin series is almost invariably at a much higher technical level than that of any Mentor book on any subject. I might

cite W.F. Albright, Archeology of Palestine (Pelican 1949); S. Piggott, Prehistoric India (Pelican 1950); S. Cole, Prehistory of East Africa (Pelican 1954); O.R. Gurney, The Hittites (Pelican 1952); and G. Vaillant, The Aztecs of Mexico (Pelican 1951) as a few examples of writing that is not only over the head of the informed layman, but is often of little use or interest to students seeking general knowledge of the area. I always found it hard to get enthusiastic over the fact that Stratum IX at Beth Shan was first attributed to the 15th century but is actually a century later, while Stratum C at Tel Beit Mirsim and the contemporary Late Bronze Age Occupation of Jericho are both of the 14th Century, not 15th century as supposed (Albright, op. cit. pp. 98-99). Neither did my knees grow weak at the fact that Stage 5 at Olduvai represents the junction between Beds II and III and that it differs from Stage 4 by virtue of the inclusion of a few stone tools made by the cylinder-hammer technique (Cole, op. cit. p. 134). Likewise unenthralling were Vaillant's statements that the Lower Middle Cultures of the valley of Mexico (the upper stages of which coincided with the Upper Middle Cultures) are exemplified at Early Arbolillo I by Figurines C3a, C3b, C1-2, C2, etc. (Vaillant, op. cit. p. 62).

Naturally, not all Penguins enter into this type of detail: one finds for example C and J Hawkes: Prehistoric Britain (Pelican 1952), V.G. Childe: What Happened in History (Pelican 1952) and The Prehistory of European Society (Pelican 1958) which are at the level which Mentor is attempting to reach. Such volumes are certainly not the average Penguin fare, however.

Before leaving the topic of the Penguins, I should like to discuss the matter of bibliographic references and illustrations which are such an important part of the Bulmer standard. It is strange that the Penguin Series, which Bulmer feels is so exemplary, is quite the opposite when her own standards are applied to it. One finds in Vaillant's book, for example, that only 7 photos out of 64 plates have scales in them. In the same way, many of Piggott's line drawings of artifacts also lack scales (see Piggott, op. cit. Figs. 3, 4, 5, 6, 7, 8, 9, 10, 12, 13, 14, 15, 16, 22, 23, etc.) The renowned Sir Leonard Woolley has failed to include a single scale in 24 pages of plates or in his line drawings of artifacts (Woolley 1953).

When one considers bibliographies, the basis for Bulmer's praise is even more surprising. Even such a figure as Wheeler (Wheeler, R.M., Rome Beyond the Imperial Frontiers, Pelican 1955) contents himself with a single page bibliography at the end of 214 pages of text and a few brief footnotes. Piggott (op. cit.) does not even reference his statements or sources but gives a brief note at chapter endings indicating best references on the subject matter of the Chapter. Further, Cole's (op. cit.) volume on East Africa contains only 9 pages of bibliography after a discussion of a very complex subject. Is this "adequate" bibliographic referencing? Without

asking whether Bulmer would care to rigorously define "adequacy", the obvious conclusion is that the Penguin books by her own standards are as deficient and irregular as Mentor or any series. They themselves lack the very things she has criticized in Mentor. Yet we all know that they are still most excellent books, worth reading and keeping, as several feet of blue and white bindings on my own book shelves testify. Their value to professionals is, I submit, to be found in the ideas presented within, regardless of any deficiencies in the superficial trappings of bibliography, illustrations, or even author's style.

From the above discussion, the status of Island Civilizations should be clear: it is not a full-dress scholarly work; it is a general treatment of Polynesian Prehistory. It is a bit more specific in nature than most Mentors, but this is a result of my own writing preferences: it is still a general book, which is what NAL asked for and paid for. Naturally, the volume presents my own opinions; I believe it is still an author's prerogative and I have warned the reader of this, as noted above. Clearly, I knew that professionals would read the volume, but I hoped that they would interest themselves in the concepts rather than focus on technical writing matters like freshman composition teachers. The maligned bibliography and notes surpasses in frequency of references and sources a majority of books in the Mentor Series as well as many volumes of the Bulmer-approved Penguin Series, and is far more than adequate for a general work, as are the scaleless illustrations and the sketch maps.

To turn to some of the specific matters which Bulmer has raised I should like to take up a few which I consider of importance and interest.

1.

The derivation of Marquesan culture from Western Polynesia: On the basis of the prevalence of Western Polynesian and Melanesian artifacts at Ha'atuatua, it is difficult to draw any other conclusion, regardless of what one's original slant may be (I once expected Tahitian origin!).

The evidence, if Bulmer will read again, consists of more than quadrangular adzes, pottery, and Tonna scrapers.

2.

Neglect of other theories (Duff, Speiser, etc.): In writing, general or technical, especially with constraints on length, one has to carefully select the materials for inclusion as I stated in my introduction (Suggs 1960, p.10). I did not include the Duff theory because I did not feel it differed too markedly from earlier formulations which I discussed. Golson's formulation, which I find most stimulating, could not be included as the book was already done and going into galleys when his paper was being prepared. In a personal letter some months ago, I assured him that a revised edition would give full space to his own views.

The European views were omitted with only a brief mention because I did not wish to get into the culture historical approach, which would require a volume or two to handle properly. Bulmer has apparently discovered Speiser: if she looks a bit further, she will find that my own views coincide strongly with those of Schmitz and others as expressed in the discussions of the Austro-Melanide Mischkultur (Schmitz 1961).

For future reviews, I offer Bulmer a few more gratuitous shots; she can point out that I have not mentioned:-

- (1) Micronesia: its role in the Polynesia settlement
- (2) Madagascar: its role in Polynesian dispersion
- (3) Possible North Asian influence in Polynesia
- (4) A full discussion of South America-Oceanian relations especially in the Pre-Formative and Early Formative of Peru
- (5) The Lost Continent of Mu
- (6) J. Frank Stimson's theories, especially the Kiho controversy
- (7) D.S. Marshall's theory of Polynesian occupation
- (8) Greenberg's recent linguistic grouping of Australian, Andamanese, and Malayo-Polynesian
- (9) Possible early Indo-European influence on Polynesian languages
- (10) The meaning of Shang and Chou motifs in Polynesian art
- (11) Kelley's connection of the Uto-Aztecan and Malayo-Polynesian languages

3.

"Unilineal" interpretation of cultural evolution:

Bulmer should enquire into the meaning of some of the terms she uses, such as "unilineal." I am far from being a unilineal evolutionist, believing that all cultures develop through similar growth mechanisms and similar stages. If anything, I am more inclined to accept Steward's views (i.e. Steward 1955). It so happens that I feel that the Polynesian cultures represent a "type" (like hunting and gathering bands or irrigation societies), and that the evolution of this type happens to be best explained in general terms by Sahlins' (Sahlins 1959) formulation.

It may come as a shock to Bulmer, but neither the true unilinealist, nor myself, would overlook or deny the existence of contacts between societies and the role that such contacts play in the dissemination of traits, but there is unfortunately a difference between trait diffusion and cultural evolution. One can have much contact and much diffusion without any change in the evolutionary status of the inferior group: this is a common phenomenon in American archeology. The kind of contact possibly productive of actual micro-evolutionary changes is that known as "culture-unit diffusion" (see Rouse 1959), pp. 63-67 and Wauchope et al. 1956, Chs. 1 and 2) in which a sizable group establishes itself in alien territory.

The archeological diagnostic for this is an entire community intrusive into a previously inhabited community with all artifacts and burials.

I have indicated cases based on archeological evidence and legend where such intrusions might have taken place (Suggs 1960, pp. 99, 142) and have also indicated that sporadic trait intrusion contacts took place.

To say that I regarded Western Polynesia as conservative is simply untrue. I said, in fact, that regional variation was probably retarded by contact in the early period (Suggs. *ibid.*, pp. 94, 101). Inhibition of regional variation does not imply conservatism, a fact which Bulmer evidently has not yet grasped. I fail to see, however, how my presentation of the historic-level divergency of Western Polynesian cultures and the meager archeological data on their development could lead anyone to label them conservative. Nor do I see how my views of Tahitian cultural evolution could be labeled conservative by Bulmer (because the house type did not change!) in the face of the data on the decided changes in temple architecture which I presented (now receiving support from stratigraphic excavation!).

4.

The Marquesas sequence: As pointed out above, the book is a general book: there is no reason for me to include all 474 pages of my Marquesan report in this chapter merely to satisfy Bulmer. The Marquesas chapter is a summary of the very lengthy volume on my excavations: therefore, details had to be omitted; further, the reader is asked to take the author's word for a few things, a trust of which Bulmer is apparently incapable. For example:-

- . Architectural sequences: These are not logical as Bulmer states, but are derived from stratigraphic excavations in dated sites: a point explained in my coming report.
- . Time scale: the dating of sites in the Marquesas was achieved through C-14 dating (Shapiro and Suggs 1959), artifact seriation and artifact dating (also explained in my coming report).
- . Exotic contacts: I could not truthfully say (not even to please Bulmer!) that any evidence of exotic contacts existed, beside those which I myself mentioned (i.e., Tahiti either direct or through the Tuamotu Islands). This contact, if such it was, is of the trait intrusive type. Again, my rationale for this is presented in some detail in my report!
- . Comparative data: Much of this is included in the other chapters of the book (of which, I am now beginning to think Bulmer has only received second-hand accounts). The rest of the comparative data is in my coming report!

Finally, it is unpleasant to find in the writings of such a stickler for objectivity and careful use of sources a number of distortions, some of which border on what are politely termed "fabrications."

1.

"In fairness to Suggs..." Bulmer states that I occasionally admit that some of the Marquesas islands lagged behind the others. I have stated the only evidences of culture lag that I know of in the Marquesas: that of megalithic architecture on the one hand (north-south diffusion) and stone carving (probably south-north diffusion) on the other (Suggs 1960, p. 126). For the present, nothing more can be said except that on the ethnographic level there were regional differences in all areas of culture (which still exist in some cases) and in language which is still very clearly defined. Perhaps "in all fairness" Bulmer might have stated that Suggs also has remarked on apparent regional variation in Western Polynesia (ibid., pp. 94, 101), the Society group (ibid., pp. 133, 102-3), the Tuamotus (ibid., pp. 119, 140), Hawaii, (ibid., pp. 155) and New Zealand (ibid., p. 201). After all, now, don't we all stand for fair play?

2.

The inspiration for Marquesan stone carving of all types did not derive from poipounders. Marquesan poipounders, however, apparently developed as a result of the influence of introduced poipounders (Suggs 1960, p. 124). In my report, Marquesan monumental carving is attributed to an application to stone of techniques and motifs developed over a long history of wooden sculpture.

3.

Petroglyphs with classical tattoo motifs (and only with tattoo motifs) appear in the 1400-1740 A.D. period, but petroglyphs are found at all points in the sequence (see Suggs 1961).

4.

I do not attribute all fortifications to the 1100-1400 A.D. period. I stated that two specific fortification complexes were built by the Taiohae inhabitants during that time: this does not imply that forts (which I described at length for other areas of Polynesia) did not exist prior to 1100 A.D. It refers to specific cases only, as an example of what I believe was a general trend of building larger, more elaborate forts.

One could go on at much greater length; but to conclude: reviewers have an obligation to give an objective, careful evaluation of the volumes they review. Well-done reviews are of value to the area specialist; the professional, specializing in other areas; the public, and the author.

Robert C. Suggs, Ph.D.