Letters

The Pursuit of Eminence

A major conclusion in the article by L. G. Wispé on traits of eminent American psychologists [*Science* **141**, 1256 (1963)] was stated with such proper scientific dispassion that it took a while for its full significance to sink in. Wispé's conclusion was that the attainment of eminence (in psychology, at least) tends to go hand in hand with insensitivity to the needs of others. I think this highly significant finding deserves restatement, so I offer the following lines:

If it's eminence you're seeking here's a strategy to use:
Don't ever learn to put yourself in other people's shoes.
If you seldom think of others you're likely to succeed.
(Altruism, Charlie, is the one thing you don't need.)

O, here's what you do if you want to be outstanding—
Here's how to be if you want to make your climb—
Here's how to act if renown's what you're demanding—
Just be selfish all the cotton-pickin' time.

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Curbing Authors

Although frequent complaints appear about the proliferation of scientific literature, constructive suggestions have been few. To ask journal editors to exercise greater discretion is to imply that these hard-working gentlemen are not already doing the best job they can. I therefore propose the following imperative, to be adopted by responsible scientific societies around the world:

Thou shalt not commit authorship more than once per year.

I think this would inflict a minimum of hardship on productive workers and would prevent the appearance of reports of a study in several different places under different titles. Authorship would include all forms of joint authorship and contributions to symposiums. Exceptions might be made in cases of review articles solicited by the editor.

How much such a ban would contribute to decreasing the ever-growing mountain of scientific publications cannot be estimated, but there could not but be a significant drop.

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Polynesian Origins

Ferdon's hypothesis (*1*) that the Polynesian culture had many sources is a statement of the obvious: all cultures are influenced by other cultures at some point in their histories, and Polynesia is no exception. Further, in regard to Polynesia in particular, the concept of multiple origins is in no way novel, for it has been an accepted working hypothesis in Polynesian anthropology for many years. One of the outstanding features of the literature of the area is the implicit and explicit recognition of possibly exotic culture traits and trait complexes in Polynesian culture, as exemplified in the writings of the Handys (*2*), Linton (*3, 4*), Heine-Geldern (*5*), Schmitz (*6*), Anell (*7*), and others. Physical anthropologists such as Shapiro (*8*) have also indicated multiple origins for the Polynesian race. Ferdon attempts to convince the reader that anthropological thought on Polynesian origins was tradition-bound and unimaginative, yet this literature, as well as his own statements (that there is a wide range of theories on the origin of Polynesian culture), indicates the true picture.

Ferdon attempts to show that Heyerdahl's activity was a stimulus to research in Polynesia, maintaining that only two excavations had been made in Polynesia prior to World War II. However, in the reference which he cites, an entire chapter is devoted to a discussion of *numerous* excavations in New Zealand dating far back into the 19th century. Actually, before World War II, surface archeology had been done by Emory (*9*) and Bennett (*10*) in Hawaii, by Emory in Tahiti (*11*) and the Tuamotus, and by Linton in the Marquesas (*4*). The British Museum and the Franco-Belgian Expeditions dug on Easter Island (*12*), and Emory and his associates dug some sites in the Hawaiian group (*13*). These excavations may not have been acceptable by modern standards; they were nonetheless excavations, and in New Zealand, at least, created a lively interest in the antiquity of the Maori. After World War II, archeological activity increased in the Pacific (which is not separable from Polynesia), with work of a more scientific nature by R. Duff in New Zealand in the late 1940's and by Gifford in Fiji in 1949 (*14*). It is no wonder that Emory, who had done so much previous archeological work in Polynesia, began Hawaiian excavations in 1950. Ferdon's characterization of the Hawaiian program is quite misleading: according to Emory himself (*15*), the Hawaiian program was begun as a regular archeological survey jointly sponsored by the University and the Bishop Museum. Later, the Bishop Museum embarked on a 5-year program sponsored by the Wenner-Gren Foundation, and it has subsequently obtained NSF and other funding. Because of a lack of trained students and volunteers, the Hawaiian program (like any other large academic program) has always involved training, but it is not only or even mainly a course for student training. It utilizes large numbers of volunteers, working mainly during university vacations. Some 60 sites have been investigated, yielding material far superior in volume to that obtained anywhere else in Polynesia to date.

After Emory's work, excavations were carried out by Gifford (*16*) in New Caledonia (1953) and Yap (1956), by Spoehr (*17*) in the Marianas (1953), and by Osborne (*18*) in Palau (1953). The work of the American Museum in the Marquesas was conceived in 1954, 2 years before Heyerdahl's Easter Island expedition, at a time when his intentions were not even known. Consideration of the chronology of these other archeological investigations indicates that Emory, Gifford, Spoehr, and Duff led the field. Heyerdahl was not a stimulator, but a bandwagon follower, like the rest of us.

Ferdon states that Polynesian languages may have submerged other, earlier languages in the Islands, obscuring earlier populations. If so, this would be the first time such submergence occurred without leaving evidence of the submerged languages in surviving terms, place names, and the like. Ferdon neglects to mention that, despite a large volume of linguistic data, no conclusive evidence of any influence exists. Claims have been made that a few non-Austronesian, Asian terms and one possible American Indian term exist in Polynesian dialects, but these remain to be proved. This is hardly what would be expected in a case of language submergence or even significant contact. If the exotic origins of these terms are certified, the mode of their transmission must still be determined.

One of the most curious sections of this article is that dealing with serology. In 1960 I pointed out the invalidity of Heyerdahl's serological survey in Eastern Polynesia (*19*), showing that any Polynesian-Peruvian Indian comparisons purporting to show common origins were meaningless because of the tremendous amount of post-contact, mainly Caucasoid miscegenation in Polynesia. In 1961 Heyerdahl's staff, including Ferdon, wrote an unpublished protest (*20*), in which they defended the purity of the population sample for this serological study. Now Ferdon, reversing his field, claims that criticism of the purity of the sample is "fatuous" because purity is impossible to attain in such a sample (precisely my original point!). The resemblance between Polynesian and American Indian blood-group and gene frequencies, to which Ferdon attributes some unstated significance, is meaningless in view of the highly dissimilar morphological characteristics of the two populations and is best explained as the result of chance. As Hooton pointed out (*21*), the most morphologically diverse, geographically separated, and culturally unrelated populations have similar blood-group and gene frequencies.

Many other points raised in the article could be discussed at length (for example, the significance of the Galapagos finds, the "significance" of the *Kon-Tiki* voyage, and so on), but to conclude, Ferdon's article appears to be mainly an attempt at justification of the *Kon-Tiki* theory, the method and style of justification being similar to Heyerdahl's. The only addition to this standard approach is the ineffectual attempt to claim for Heyerdahl the role of a motivator of research.

The most significant feature of the article is that in 1963, after years of talking and writing, and an archeological expedition, supporters of the *Kon-Tiki* "theory" do not have one objectively acceptable piece of evidence of prehistoric Peruvians in Polynesia (in the form of Peruvian artifacts in stratigraphic archeological contexts) and must resort to hackneyed journalistic tactics, relying on nonarcheological, mostly irrelevant data about blood groups and ocean currents to "prove" their point. While laying stress on the importance of the Easter Island "evidence," which is a product of these journalistic techniques, as Barthel (*22*) and Emory (*23*) show, Ferdon makes no reference to the results and implications for his theory of all other archeological investigations in Polynesia (Tonga, Samoa, Tahiti, Moorea, Raiatea, Nuku Hiva, the Hawaiian group, Mangareva, and New Zealand), in which no objective evidence of Peruvian contact or influence has been found.

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21. E. Hooton, *Up From the Ape* (Macmillan, New York, ed. 2, 1946), p. 557, Table 9.
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It was inevitable that Robert C. Suggs would write the commentary that he did, and I am gratified that he has done so, for it stands as documentary proof that there was good reason for my plea in "Polynesian origins" for an open-minded approach to this complex problem. For the reader's ease of reference I shall answer Suggs' commentary paragraph by paragraph.

Par. 1. Suggs has obviously missed the point of the article. It is not that all cultures are influenced by other cultures but that Polynesian culture is potentially the result of influences from a variety of cultures around the Pacific ring, *including the Americas.* I can hardly be accused of trying to persuade the reader that I am presenting a "novel" idea, for a list of the major geographic areas which have figured in Polynesian origin theories is specifically included in the article. The tradition-bound factor alluded to was precisely the one displayed by Suggs—an unwillingness to consider American contact.

Par. 2. Suggs is correct in saying that I erred in attributing only one excavation to the New Zealanders prior to World War II, and I apologize to my New Zealand colleagues for this oversight. However, since controlled excavations are our principal sources of meaningful information, the New Zealanders would probably agree that the number of such excavations could hardly be called "numerous."

As for the influence of Thor Heyerdahl's "radical" views in stimulating research in Polynesia, even one of Heyerdahl's severest critics, Thomas S. Barthel, admits "Heyerdahl's role as a pleasant *advocatus diaboli* who has provoked a mighty upsurge of archeological field-work in the Pacific area" (*1*). The reader might also note the interesting "coincidence" that every one of the expedition dates spotlighted by Suggs in his paragraph 3 *postdate* the publication of Heyerdahl's formal presentation of his hypothesis (*2*), while Emory's 1950 excavation coincides with the first appearance of the English translation of Heyerdahl's account of his 1946 raft voyage, in which he popularly describes the nature of his conjecture (*3*).

Failure to mention the early surface archeological work of Kenneth P. Emory and others was not an oversight, for I was specifically reporting *excavations*. That this work was not mentioned may be explained to the uninitiated reader by pointing out that surface archeology concerns itself only with the surface manifestations of a prehistoric site. The information thus gained about its historical content is equivalent to what a medical student might learn of the structure and function of the human body by a superficial examination. In other words, dissection is needed in both cases to make the understanding significant.

Suggs is mistaken in crediting the Franco-Belgian Expedition with excavations on Easter Island, as the following quotation from his own reference on the matter indicates (*4*): "The archaeological investigation of the island was undertaken by Dr. Lavachery, who will publish separately the results of his *survey* [italics mine] of the north coast." He is correct, however, in attributing excavations to the Routledge expedition (British Museum); but he fails to inform the reader that these excavations have never been published, and that the field notes have not been found, so that his reference to them is somewhat pedantic, inasmuch as an excavation unreported is like any scientific experiment that has not been reported—that is, it fails to exist as a useful scientific document.

As for his claim that Emory and his associates excavated in pre-war times, *they* say (*5*): "Except for an excavation by J. F. G. Stokes of Bishop Museum at a fisherman's shelter on Kahoolawe in 1913, no significant digging was done until 1950. During the intervening years it was assumed that little could be learned through excavation." To this we might add that these pre-1950 excavations have not been reported and therefore fall into the same category as those of the Routledge expedition. My characterization of Emory's 1950 excavation as an adjunct to a course on archeological field methods at the University of Hawaii follows faithfully Emory's own explanation of the genesis of the program (*6*). That it later developed into something more substantial is to be applauded by all.

Par. 3. It is not clear why it is important to Suggs that the American Museum's expedition to the Marquesas was conceived in 1954, 1 year (not, as Suggs states, 2) before Heyerdahl's expedition went to Easter Island. All plans must be conceived at some time, but the important thing is when they are carried out and when the results are made available. Heyerdahl advised me in 1953 that I had a berth on his expedition if I cared to come along.

Par. 4. Suggs's insistence that there is no known case of language submergence without a linguistic trace in the form of surviving terms and place names may be proved in error. If he were correct in saying that terms and place names survive in spite of other incoming linguistic groups, then it would long ago have been a relatively simple matter to trace the migrations of linguistically different peoples throughout the world. That this has not been done is verification that languages can be lost or submerged, and we now know there was ample time for linguistic loss to have occurred in Polynesia.

Par. 5. It is to be expected that Suggs would find my section on serology "curious" because I have changed my position from what I thought a few years ago. An open-minded approach to science necessitates a stocktaking from time to time, and only a person unaccustomed to reorienting his thinking would find such a procedure curious.

Since, in his paragraph 7, Suggs accuses me of having employed "hackneyed journalistic tactics," it is only fair to point out his misrepresentations. Consider Suggs's statement: "The resemblance between Polynesian and American Indian blood-group and gene frequencies, to which Ferdon attributes some unstated significance, is meaningless. . . ." In truth, I specifically point out the need for a greater understanding of the role of microevolutionary forces "before much more can be said about the biological relationships of the Polynesian peoples" (*7*). *Nowhere* do I impute some unstated significance to these blood-group and gene frequency resemblances. On the contrary, I toss them out as meaningless. Again, in his paragraph 7, Suggs's statement "While laying stress on the importance of the Easter Island 'evidence,' which is a product of these journalistic techniques . . ." is fallacious. My only reference to Easter Island is in connection with the discovery that ceremonial structures there were added to through time and that these additions, like artifacts, could be used as a means of relative dating on the island.

Par. 6. Here Suggs again misrepresents my intentions. The only point I attempted to prove was that certain areas of the Pacific, which included the Americas, had varying degrees of environmental potential conducive to accidental voyages into the Pacific. What this has to do with "proving" the Heyerdahl theory (more properly hypothesis) is obscure unless, to Suggs, the mere inclusion of the Americas in the discussion is tantamount to pushing the Heyerdahl hypothesis.

Par. 7. One cannot demand a particular class of evidence to prove that Peruvians were in Polynesia and not demand the same kind of evidence for proof of contact with Asia. What Suggs fails to inform the reader is that *no* artifact of either Peruvian *or Asiatic* origin has been found in Polynesia in "stratigraphic archeological contexts."

Space does not allow the recounting of the full evidence of possible American contact, especially that resulting from the Easter Island excavations. Since Suggs would like to pass off our technical work as journalistic trickery, I had best let Betty J. Meggers of the Smithsonian Institution state the case by quoting from her recent review of our Easter Island volume (*8*): "Although it is possible to suggest alternatives for some of the explanations offered by the authors of this volume, it is impossible to dispute their evidence that contact took place between Easter Island and the South American mainland in pre-European times, unless we wish to deny the validity of comparative analysis for showing cultural connections. The evidence relating certain elements with the South American mainland is the same as that used to derive others from Polynesia, and we may not deny the one while accepting the other."

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