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VOLUME II, NO. 1-4**

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REVIEWS

METHODS AND PRINCIPLES

Krückenruder. Von FRITZ GRAEBNER. (Baessler-Archiv, III, 1913, pp. 191-204.)

In his article on "Die melanesische Bogenkultur und ihre Verwandten" (*Anthropos*, 1909, pp. 726-780, 998-1032) Graebner introduced the concept of the "crutch-paddle," which was defined as having a crutch-like grip and a short, broad blade of which the greatest width is in the third nearest to its tip (l. c., p. 763). He called attention to crutch-paddles among the Carib and Arawak Indians, and to the fact that all the types of Melanesian and Indonesian blades turn up in South America (ibid., p. 1016); while in North America he found a crutch-paddle with characteristic blade on the Northwest coast (ibid., p. 1021). These resemblances served to support the theory of an ancient cultural connection between Indonesia and America. As Graebner included in his brief discussion some paddle types that did not strictly conform to his definition, it was possible to assume that the cross-handle formed the most essential part of his concept. Interpreting Graebner in this way, Dixon invoked against him the principle of limited possibilities: paddle shafts, he argued, must either end in some form of cross-handle or, broom-fashion, lack a cross-grip; hence, the recurrence of either of the only two possible forms is without historical significance (*Science*, XXXV, 1912, p. 50).

However, it appeared from a subsequent statement in Graebner's *Methode der Ethnologie* (p. 145) that the cross-grip did not completely determine his concept of the crutch-paddle. And in the amply illustrated article before us Graebner sets forth his position at greater length and with still greater clearness. The presence of a mere crutch is indeed considered of some importance since this feature is by no means universally diffused (p. 191); but stress is laid especially on the combination of the crutch with the particular type of blade defined in the previous paper (p. 193). Exceptional cases of Melanesian crutch-paddles with

non-typical blades are explained as due to Polynesian influence; on the whole the author thinks there can be no doubt that the combination of the cross-grip and "typical" blade represents a morphological and genetic unit. As it occurs in southeastern Indonesia, extending over New Guinea and a large part of Melanesia, it must be regarded as an element of the Melanesian bow-culture and the related Indonesian complex (p. 195). In Micronesia and Polynesia genuine crutch-paddles are exceptional and are best considered as variants (*Ausläufer*) of Melanesian forms. They are lacking in Africa, where, however, typical blade forms occur. Finally, Graebner notes crutch-paddles from Switzerland and Upper Bavaria in Europe; and Ostyak, Siamese, Chinese, Japanese, Bering Strait, and Aleutian forms from Asia. Making due allowance for the possible omission of relevant forms, the author is impressed with the fact that not one of the extra-Oceanian paddles mentioned combines the crutch-grip with the typical blade in the manner distinctive of Oceania.

Because this combination does not occur in other regions, Graebner regards its presence in South America—more particularly, in Guiana and the Amazon basin—as significant, that is, as evidence of Oceanian influence. Independent development, whether from technical reasons, or because of the similarity of cultural and natural conditions, is said to be impossible, whence by a process of exclusion historical contact follows as the only logical alternative. Graebner next reverts to the criteria of historical connection postulated in his *Methode*. The form criterion is considered adequate in the case discussed, that is to say, the similarity of the paddles in Oceania and America is sufficient to establish the theory of a common origin. But in addition there is the quantitative criterion, for Graebner has elsewhere noted a number of other parallels between the two cultures compared. But what, asks Graebner, was the path of diffusion? The crutch-paddle cannot have entered America by way of Polynesia, for the Polynesian forms are not very characteristic variants of the Melanesian type, which, on the other hand, is well represented in America. Hence, it must be supposed to have come in from the Northwest, the Chinese, Japanese, Aleutian, and Bering Strait paddles marking its course (200-201).

Finally, the author tests his argument by von Hornbostel's criteria of historical contact: Are the features of the crutch-paddle definitely determined? Are they unconnected with the practical end served by paddles? Are they variable? His reply is in each case affirmative. Such features as the cross-grip, the point of greatest breadth, the spine-like projection of the shaft, etc., are sufficiently definite. The crutch

cannot serve any purpose connected with paddling, as the technique of paddling is identical in paddles with and without the crutch. All parts of the paddle are variable, especially the blade, which displays an almost unlimited range ("eine in der Tat nahezu unbegrenzte Variationsbreite").

Several objections will naturally occur to most readers. The single features of the crutch-paddle, however well-determined in the initial definition, cease to be so in the further treatment accorded to them by Dr Graebner. As I pointed out some time ago ("On the Principle of Convergence in Ethnology," *Journal of American Folk-Lore*, XXV, 1912, pp. 24-42, especially p. 36 f.), even geometrically similar forms may produce very different psychological results. The problem in the classification of objects consequently is not whether they can all be brought under the same geometrical or some other concept suggested by the classifier, but whether they are assembled together by the natives themselves or are actually known to have been derived from the same form. All this applies, of course, with far greater force when the morphological resemblance between the objects compared is *nil* and they are arbitrarily made to fall under the same catchword. Perhaps the worst offence committed by Graebner in this regard is his classing the Massim paddle grip of his Fig. 17 as a variant of the simple cross-bar and as connected with the pierced grip of a Doreh paddle in his Fig. 38. Probably all other ethnologists will be inclined to connect the Massim grip with the openwork carvings of spatula handles from the same district. Whether the style of carving was first developed on the paddles and afterward transferred to the spatulas, or *vice versa*, is immaterial in this connection. The essential point is that the style is something unique and irreducible to such abstract concepts as "crutch" or "pierced handle."

What is true of Graebner's use of the "crutch" concept in this particular instance applies with equal force to his discussion of the "typical" paddle blade. When we compare the blades of Figs. 1 and 59 from the central Solomon Islands and the Rio Negro respectively, the resemblance is unmistakable, no matter how we may interpret it. But Graebner assumes that blades of very different appearance are merely variants of these "typical" forms. Granted that such variability is natural, how do we know that the process of differentiation has not taken place in the reverse direction? Why cannot the lozenge-shaped Javan blade of Fig. 11 be taken as the original Oceanian type and the shovel-like Brazilian blade of Fig. 62 as the South American prototype? Hardly any one would consider these two forms morphologically or

genetically related, but on the hypothesis of an almost unlimited variability such blades as those of Figs. 1 and 59 might of course develop convergently from the Javan and Brazilian blades respectively.

So far as the practical value of the crutch is concerned, it is clear that a cross-bar need not have any influence on the paddling process itself and yet have a utilitarian significance inasmuch as it may afford a more convenient grip. Moreover, it is conceivable that in many instances the crutch may be connected with a paddle from non-utilitarian motives, for example, by imitation of other implements with crutches, whether these do or do not exist for utilitarian reasons.

To sum up. The combination of a crutch with the "typical" blade found in South America and Oceania cannot be accepted as satisfactory evidence of historical connection even if we limit the consideration to really similar forms: (1) because the cross-grip may arise independently from various reasons; (2) because, granting the variability of paddle blades, the convergent development of similar forms must be recognized as a possibility. Here as elsewhere a disregard of obvious alternatives vitiates the author's argument. The article on *Krückenruder* should be studied by every ethnologist because hardly anywhere else has Dr Graebner taken his colleagues so fully into his confidence regarding his comparative methods; but for that very reason most readers will remain skeptical as to the results of these methods.

ROBERT H. LOWIE

The Element of Fear in Religion. By W. D. WALLIS, University of Pennsylvania. (Journal of Religious Psychology, July, 1912, Vol. v, pp. 257-304.)

At the beginning of the article, the author takes pains to make a distinction between religion and religious practices and beliefs. "Religion is psychological and individual," but "the particular practice and form of it may be social" (p. 260). It is "religion and religious emotion" that the author proposes to treat rather than "the manifestations of religion and religious emotions themselves."

Now, in the first place, religion and religious emotion belong in the realm of psychology, as our author realizes. Whatever treatment is given them should be according to the methods of psychology. What those methods are only a psychologist knows. The popular belief is that psychologists proceed either by the new method of experimentation, confined mostly to the sense perceptions, or by the old method of subjective self-analysis. The paper, however, does not present the results of experiments nor does it establish the conclusions by *à priori* reasoning and subjective introspection.

Our author is well acquainted with the anthropological method for he has repeatedly called attention to its misuse. Were he treating an ethnological subject such as religious belief and behavior, he would realize that there is only one proper method of procedure. First, he should take a definite region which has been carefully worked by a reliable and trained ethnologist and discover from the reports on that region what actually exists in the way of religious practices and beliefs. It would then be justifiable to point out and suggest the possible sources and causes of the practices and beliefs. When that had been done, related facts from other regions equally well authenticated and considered in their relation to other facts in that region might be compared with the results obtained in the first region.

It is certainly a misuse of ethnological method to cite the effect of the sound of the bullroarer in Australia and then the falling of a piece of bark in Alaska (p. 269). Such incidents are of ethnological value only when viewed in connection with other religious activities and feelings of the social group to which the individual belongs. But the author is not even discussing an ethnological subject. If he has concluded on some other grounds that fear is the main cause of religious emotion, why does he resort to Frazer's method of citing a multitude of disconnected happenings from all over the world, which, judging from the quoted sources, may never even have happened?

As to the main thesis, that religion has to do with the unusual and uncanny, it is only necessary to read carefully an account of the religious activities of some people who have been fully and carefully studied to realize that such causes explain but a small part of religion. The normal and ordinary sunrise has as much influence on religious feeling as does an eclipse. Religion is not a mere part of human thinking and acting, it is all of life viewed from one angle. It is not the reaction of the mind toward one sort of phenomena, but one of the attitudes of the mind toward all of life.

But it is not wise to allow what one thinks about religion to go into print even in a review. Newly gathered facts seen in new relations are sure speedily to modify and enlarge whatever view has been formulated. It is much more important that we work with safe and proper methods than that we reach correct conclusions.

PLINY E. GODDARD

Studi di Antropogeografia Generale. I. Studi sulla distribuzione dei caratteri e dei tipi antropologici. By RENATO BIASUTTI. Florence, 1912.

This is *anthropogeographie* in its somatological aspects. "Geographie without somatology seemeth a carkasse without life and motion: So-

matology without Geographic moveth, but in moving wandereth as a vagrant without certain habitation"—to adapt an old phrase—is the author's plea. This making of physical type and location "twinnes and unseparable companions" is attempted by Biasutti, who assumes little less than "to take up the whole World on his shoulders." In a series of maps, patterned somewhat after the manner of Ripley, he gives us the geographical distribution over the globe, of various physical characteristics, such as cephalic index, facial index, pigmentation, stature, etc., etc., and a concluding linguistic map. So far as he points out association of characteristics, such, for example, as the "indifferent" association between stature and pigmentation (p. 45), his treatment is neither satisfactory nor pretentious. There is little attempt to separate the unrelated characteristics from the interrelated ones. Thus, his argument that the Negrillos and Negritos are a surviving substratum out of which the types around and remote from them have developed, is pure assumption. The theory presupposes the influence of geographical environment, or of inherited spontaneous variations in order to work at all; and when we presuppose these, it must be admitted that a theory directly opposed to that of the author would fit the facts equally well. To say the least, physical environment, as a possible factor in fixing type, must be ruled out before we can talk of *paleomorphs* and *neomorphs*, if we use these terms with genetic and historical connotation. (See, for example, pp. 108-9, 121, 158-164.)

Biasutti gives the following list of characters based on his view of their comparative worth in determining type, though the arrangement is, we take it, not otherwise intended to indicate a hierarchy of values (p. 90). Less valuable: color of the skin, stature, facial index, degree of prognathism, nasal index, orbital index, and cephalic index. More valuable: structure of the hair, somatological proportions and "shape" of the body, shape of the face, type of prognathism, morphology of the nose, shape of the eye, and morphological "habitus" of the cranium.

In his method of classification, however, the author has certainly not fallen into the sins of "conceptual realism," from which physical anthropologists seldom turn away repentant to be saved. Biasutti would not classify a people unqualifiedly as dolichocephalic if that represented merely the average of diverse types; but rather as predominantly dolichocephalic type with such and such percentage of subdolichocephalic or brachycephalic. A similar method is pursued in considering hair, pigmentation, stature, etc. This makes possible a more correct classification according to actual resembling types rather than according to the type of the average, which, after all, may not be a common type.