

“The Eskimos”: Some Comments

BY KAJ BIRKET SMITH

In *Anthropologica* n.s. II no. 1 Mr. William E. Taylor, Jr. has published a review of my book “The Eskimos” and subjects it to a severe criticism. I shall not enter upon some debatable points (which does not mean, however, that I agree with his views), but limit myself to those where, in my opinion, he is definitely wrong.

He writes e.g.: “After Collins’ comments on the matter one is surprised to see the distinctive Eskimo mandibular morphology attributed to diligent chewing.” Here Mr. Taylor seems to have interpreted my text to suit his own strictures, for after having stated that the adaptation view was the opinion of Fr. C.C. Hansen and Fürst (1915) I proceed (p. 42): “Such an extreme view, however, is inconsistent with all genetic laws. Be the agreement between mode of life and physique ever so striking, it is for the present not merely our right but our duty to see whether the type cannot be explained in another manner.” (p. 42) Incidentally, it is not without interest that Collins’ observations are partly based upon what I myself wrote as early as 1940¹.

The chapter Origin and Development of Eskimo Culture is, according to the reviewer, “scarred by errors”. My statement that the present Central Eskimo very seldom use trace buckles (p. 188) “is at variance with my (i.e. the reviewer’s) limited observations and the much broader experience of others I have questioned.” I do not know how conditions are now, but it is an indisputable fact that 40 years ago trace buckles were

¹ “The total evidence, therefore, sustains the views of Hooton, Jenness, and Birket-Smith, that the Eskimos have inherited and not acquired their peculiar skull form”. (Henry B. COLLINS: *The Origin and Antiquity of the Eskimo. Ann. Rep. Smithsonian Inst., 1950.* Washington, 1951. p. 444). “Both Jenness and I have previously pointed out that such a development which presupposes a direct heritage of acquired qualities, is in conflict with all laws of heredity”. (Kaj BIRKET-SMITH: *Anthropological Observations on the Central Eskimos. Rep. Fifth Thule Exped.* III, 2. p. 111).

extremely rare among the Central tribes — and that is, of course, what matters in this context.

"It is generally held, and supported by C-14 dates, that Near Ipiutak is older, not younger, than Ipiutak" is another of the reviewer's objections. The greater age of Near Ipiutak as compared to Ipiutak, was suggested by Dr. Helge Larsen in a lecture given in New Haven in May 1960, but as late as 1958, at the Circumpolar Conference in Copenhagen, J.L. Giddings placed Near Ipiutak *after* Ipiutak, and in a paper 1960, i.e. a year after the publication of my book, he still leaves the dating question open². The reference to C-14 dating I am at a loss to understand, as absolutely no such dating exist as far as Near Ipiutak is concerned.

According to Mr. Taylor it is wrong that the whole pre-Dorset complex from the eastern Arctic has been named Sarqaq (cf. p. 191). Among some archeologists there is a rather unfortunate tendency to emphasize differences and to overlook similarities and therefore to create new names every time a site is excavated. Meldgaard has avoided this pitfall, which sooner or later will make archeology utterly confused, when he refers the pre-Dorset remains in Fury and Hecla Strait to the Sarqaq complex³. True, Eigil Knuth has used the terms Independence I and II for pre-Dorset and Dorset respectively in the Peary Land area, but this seems to be highly unfortunate, because it suggests an uninterrupted development.

According to Mr. Taylor, true burins are not absent in Dorset culture (cf. p. 191). Among thousands of Dorset specimens only *one* true burin (and that probably intrusive) has been found, viz. at Collins' T 1 site⁴, whereas true burins are extremely common in the Sarqaq period.

² J.L. GIDDINGS: A View of Archeology about Bering Strait. *Acta Arct.*, XII, København 1960. p. 29. Ejusd. the Archeology of Bering Strait. *Current Anthropol.*, I., Chicago, 1960, p. 127.

³ J. MELDGAARD: Dorset kulturen. *Kuml* 1955. Aarhus, 1955. pp. 171, cf. 176.

⁴ Henry B. COLLINS: The T 1 Site at Native Point, Southampton Island, N.W.T., *Anthrop. Pap. Univ. Alaska*, IV, College 1956, Pl. vi, fig. 1. On the other hand, the triangular microliths interpreted by Collins as evidence of a burin technique are of quite other origin, cf. MELGAARD in *Selected Pap. Fifth Internat. Congr. Anthrop. and Ethnol. Sci.* 1956. Philadelphia, 1960, p. 592.

For the statements that art does not appear till the late Dorset phases (p. 191), and that the use of polished slate in early Dorset was a transient phenomenon taken over from the Woodland cultures (p. 195), it suffices to refer once more to the huge material from Meldgaard's excavations at Fury and Hecla Strait⁵.

I leave the reader to decide whose paper is "scarred by errors."

⁵ MELDGAARD in *Kuml* 1955, p. 169 ff., cf. p. 175 ff.