

are also Devonien — — —, auf EKLUND¹⁾ und mich hinweist ohne die Verdächtigungen, die er jetzt vorbringt.

Selbstverständlich habe ich während der Expedition im Sommer 1934 mit den meisten geologischen Expeditionsteilnehmern über Geologie gesprochen, ich möchte jedoch gleichzeitig betonen, dass ich bereits, ehe ich BÜTLER im Sommer 1934 überhaupt traf, selbst den begangenen Irrtum berichtigt hatte, und dass ich bzgl. der Eruptivserie in Canning-Land darüber im klaren war, dass ihr Liegendes die gefaltete Eleonore-Bay-Formation war. Da ich bei meinem dritten Besuch über den Eruptiven fossilführende devonische Sandsteine fand, mussten sie natürlich ein anderes Alter haben, als es von L. KOCH²⁾ und anfänglich von mir selbst³⁾ angenommen war.

An der Zweiteilung des devonischen Vulkanismus habe ich keinerlei Anteil und habe auch niemals die Forderung erhoben, ihn zu haben. Wenn ich mit dem Moskusoksefjord-Gebiet Vergleiche angestellt habe, so geschah dies nicht zum wenigsten darum, um die Aufmerksamkeit auf die Feststellung des palaeozoischen — devonischen — Alters der dortigen Eruptive zu lenken (ORVIN, TEICHERT, BÜTLER⁴⁾), eine Feststellung, die im Gegensatz zu der von H. BACKLUND⁵⁾ gegebenen Darstellung dieser Gesteine als spätpräkambrische (prätilitische) stand.

Aus dem angeführte ergibt sich klar, dass die von mir in meinen Arbeiten der Jahre 1936 und 1937⁶⁾ vorgebrachten Gesichtspunkte auf selbständigen im Sommer 1934 angestellten Beobachtungen beruhen. H. BÜTLER hat auf Canning-Land nicht vor dem Sommer 1936 gearbeitet, und nach diesem Zeitpunkt habe ich nicht mit ihm gesprochen noch sonst irgendwie in Verbindung mit ihm gestanden.⁷⁾

ARNE NOE-NYGAARD.

Some Misinterpretations of "Remarks upon LAUGE KOCH: Geologie von Grönland. 1935".

(Medd. fra Dansk Geologisk Forening. Vol. 8, Pt. 5. 1935).

In the lines that follow, some misinterpretations of the paper mentioned in the superscription will be pointed out. Taken separately they may seem immaterial, but taken together they show a tendency which it is necessary to contest. The two examples mentioned first are to be found

¹⁾ Bergingenieur O. EKLUND arbeitete auf der Wegener-Halbinsel im August 1934, und zwar gleichzeitig mit meinem dritten und letzten Besuch auf Canning-Land.

²⁾ Medd. om Grøn. Bind. 73. 2. Købh. 1929. p. 124.

³⁾ Medd. om Grøn. Bind. 103. Nr. 1. Købh. 1934. p. 40.

⁴⁾ Skrifter om Svalb. og Ishavet, Nr. 30, Oslo, 1930. — Medd. om Grøn. Bind. 95. Nr. 1. Købh. 1933. — Medd. om Grøn. Bind. 103. Nr. 2. Købh. 1935.

⁵⁾ Medd. om Grøn. Bind. 87. Nr. 4. Købh. 1932.

⁶⁾ Medd. om Grøn. Bind. 118. Nr. 6. Købh. 1937.

⁷⁾ Aus dem obigen geht gleichzeitig hervor, dass die von WEGMANN (Medd. om Grøn. Bd. 113, p. 32) vorgeführten Beschuldigungen, dass meine Arbeit von 1937 »to a great extent (is) based on BÜTLERS investigations«, unrichtig sind.

in a paper by G. SÄVE-SÖDERBERGH: Medd. om Grønland. Vol. 96. No. 5. 1937, and the remainder in C. E. WEGMANN'S paper: Medd. om Grønland. Vol. 113. No. 2. 1938.

On p. 35 SÄVE-SÖDERBERGH writes:

"The find of beds containing *Asterocolamites scrobiculatus* in the valley W. of Mt. Kollen proves that KOCH — is right, in so far that continental Carboniferous beds are present on Canning Land, and his critics (BØGGILD and others 1935, p. 504) are wrong in their somewhat naive assertion that there is no continental Carboniferous on Canning Land because one of them (NOE-NYGAARD) failed to find it."

In "Remarks upon LAUGE KOCH: Geologie von Grønland" (in the sequel denoted as "Remarks") there is no mention at all of what O. B. BØGGILD and others think about the presence of continental Carboniferous on Canning Land, but it is pointed out that KOCH'S statement "Zufolge NOE-NYGAARD ist kontinentales Karbon auch auf der Wegener-Halbinsel und auf Canning-Land weit verbreitet" is incorrect, since NOE-NYGAARD found no continental Carboniferous at all in Canning Land. — The naivety is entirely SÄVE-SÖDERBERGH'S since he seems to think that KOCH'S incorrect quotation is rendered more correct by the fact that continental Carboniferous has since been found there.

In SÄVE-SÖDERBERGH'S paper on p. 36 is further found:

"The conglomerates and white and pink sandstones, following above the dolomite on Depot Island, form the Depot Island Formation. For the present it seems best to define this formation to include the sediments between the dolomite and the sandy shales with Ammonites, thus excluding the latter strata, which are possibly of Eotriassic age." and in a footnote referring to this:

"It will be clear without comment to any reasonable and competent reader (though apparently not to BØGGILD and others 1935) that this is the sense in which the name Depot Island Formation is used by KOCH (1935)."

If SÄVE-SÖDERBERGH will read "Remarks" from p. 505 line 10 below to p. 506 line 2 above, he will see that what is taken objection to is not the momentary view of KOCH or others of the limits of the Depot Island Formation, but the circumstance that KOCH in his exposition of 1935 pretends to have had the same view of this deposit already in 1929. This latter assertion is not, however, correct and was designated by BØGGILD and others as misleading, a view that must still be held—in spite of SÄVE-SÖDERBERGH'S assertions—"by any reasonable and competent reader".

In WEGMANN'S paper, at p. 8 above, the following passage is found referring to "Remarks" and pretending to state the opinion of BØGGILD and others on the Archæan of West Greenland: "that the earlier investigations have supplied the full explanation of the structure of the ancient shield."

In "Remarks", however, from p. 499 below to p. 500 above the text runs as follows:

"Thus in the stratigraphic section the Archæan is not mentioned at all in spite of the fact that the whole of Greenland's west coast, and not a small part of the east coast, are built up of this formation. There are numerous papers dealing with this formation by EBERLIN, HEIM, KNUTSEN, NORDENSKJÖLD, STEENSTRUP, and WAGER. LAUGE KOCH himself considers the whole of Greenland as an independent shield, and the book contains a chapter: "Der grönländische Schild," so we might have expected an account of the geology of the oldest formation."

As will be seen the "Remarks" merely take objection to KOCH's insufficient treatment of Archæan, hence the above-mentioned passage from WEGMANN's paper is incorrect.

From p. 10 below to p. 11 above there is a section in which WEGMANN seems to criticise LAUGE KOCH's working methods: at the end of the section, however, the reader is surprised to see BØGGILD etc. (1935) mentioned as the guilty parties. As usual there is no reference to pages, and it is impossible to see what WEGMANN means.

The same must be said about the following passage from WEGMANN's paper pp. 12—13, which is likewise without any reference to pages: "a group of persons have attempted to introduce a number of contrary principles¹⁾ into the geology of Greenland (BØGGILD, etc., 1935). The same persons have likewise postulated the invariability of classifications once adopted by earlier authors within the geology of Greenland." This is incorrect and cannot be deduced from "Remarks".

At p. 23, middle, WEGMANN writes: "Hence it seems justifiable to say that as regards the Arsuk group, too, the old division cannot be maintained; it is moreover designated by BØGGILD (1935) and his adherents as a meaningless term, though neither he nor his collaborators have attempted to go more fully into the question; possibly further information and publication of their new results on the pre-Cambrian basement of Greenland may be expected."

In answer to this it shall merely be pointed out that "the Arsuk group" is not mentioned at all in "Remarks", and thus the abovementioned comment is without any justification whatever.

At p. 133 WEGMANN writes about the Igaliko sandstone:

"If the Devonian is maintained, as done recently by BØGGILD (BØGGILD, etc. 1935) (unfortunately, however, without a statement of his motives) — —".

"Remarks" p. 498, however, reads thus:

"Another side of LAUGE KOCH's working methods is illustrated in his treatment of the results of other writers' examinations. On p. 21 it is for instance stated that USSING considered the Igaliko sandstone to be Devonian, and that BACKLUND has found certain similarities between this sandstone and the East Greenland Devonian; but LAUGE KOCH nevertheless, with reservation, but without stating any reason, ascribes the Igaliko sandstone to the pre-Cambrian."

As will be seen, BØGGILD and others have not expressed any opinion as

¹⁾ As regards modern pre-Cambrian research.

to the age of Igaliko sandstone in the "Remarks", nor does WEGMANN himself seem to think so on p. 131, line 9 above.

The above may be summed up as follows: WEGMANN has made use of his paper to attack O. B. BØGGILD and others. His accusations are either so obscurely worded that it is difficult to reply to them, or the foundation of the attacks is not in accordance with the truth. By omitting to quote page-numbers WEGMANN has made it difficult for most readers to detect the incorrectness of his accusations.

Even though the example that follows is not concerned with "Remarks" but refers to NOE-NYGAARD. Medd. om Grl. Vol. 103, No. 1. 1934, it should, nevertheless, be included in this connection:

WEGMANN, p. 133:

"In the southern area also (Wegener Peninsula and Canning Land) the Devonian strata are folded, as may be seen from the photographs published by NOE-NYGAARD (1934)."

WEGMANN, footnote p. 133:

"His photographs show a very peculiar case in the history of geological investigation, viz. that the photographic camera has detected more than the geologist at work, since NOE-NYGAARD denies in his text and diagrams (pp 80—81) the folds recorded by his camera and reproduced in his figures and plates."

NOE-NYGAARD, p. 20, line 4—1 below:

"The area between Vimmelskafet and Lagunenasset shows a gentle folding. An anticline is seen in the coast immediately to the south of Vimmelskafet; the axial strike is SE—NW., seemingly with a slight dip towards the SE."

and p. 68, middle:

"The supposed oldest movements within the district are met with in the (Middle) Devonian just to the south of Vimmelskafet, where, as stated above..., a slight folding has taken place prior to the deposition of the marine Upper Carboniferous-Lower Permian."

and p. 70, line 16 below:

"The supposed faulting with downthrow and gentle folding of the Devonian, prior to the sedimentation of Continental Carboniferous, may very likely be parallelized with the "Bretonic (or "Sudetic") phase" of movements."

When WEGMANN has read the above three quotations from NOE-NYGAARD's paper, as he ought to have done before he wrote his peculiar foot-note, one would think that he would admit the incorrectness of his passage "that the photographic camera has detected more than the geologist at work". It is obviously incorrect to say that NOE-NYGAARD "denies in his text . . . the folds recorded by his camera", and as regards the diagrams (pp. 80—81) it is not sufficient to look for foldings in a diagram in the section "Course of Sedimentation."

RICHARD BØGVAD.