

**Geology.** — “Concerning the Existence of Elevation Craters in the Dutch East Indies, as supposed by some geologists.” By L. RUTTEN.

(Communicated at the meeting of February 28, 1925).

It is well-known that in the controversy between the adherents to the Accumulation-theory (SCROPE) and the Elevation-theory of craters (VON BUCH) F. JUNGHUHN<sup>1)</sup> has sided with SCROPE, in the light of his own experience of volcanoes in Java. We also know that the accumulation-theory was paramount during many decennia, but that gradually a greater activity was again assigned to the magma. It has been conclusively demonstrated that the magma is possessed of such intrinsic energy, that under certain circumstances it can force its way through the earth's crust even there where no fissures existed before. Some authors, however, went the length of assigning an elevating power to the magma, and again accepted VON BUCH's theory of the existence of “Elevation-Craters”. Strange to say, in the same Dutch Indies, which had formerly furnished so many arguments for the accumulation-theory, geologists now supposed to discover instances of Elevation-Craters.<sup>2)</sup> One of these instances has even found its way in a great Textbook of Geology<sup>3)</sup>.

It is not my object to ascertain whether real Elevation-Craters have been found in other regions. My intention is only to show that the instances adduced from the archipelago do not afford any evidence whatever in favour of “Elevation-Craters”. I feel the more prompted to do so as, to my knowledge, the views of VOLZ and RECK (l.c.) have never been contradicted, while moreover VOLZ's conception has been more or less authorized by KAYSER in his Textbook.

W. VOLZ holds that the porphyrites, which according to him and to J. HOOZE<sup>4)</sup> constitute the nucleus of the northern part of Pulo Laut, to the east of Borneo, are younger than the surrounding sediments, and that the porphyritic magma in forcing itself upwards has raised the latter so as to form a dome. To maintain this view he had in the first place to alter HOOZE'S representations of the geology of the island, since according to HOOZE (l.c.) in the island of P. Laut an eocene formation of sandstones,

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1) F. JUNGHUHN, Java, 1854. III.

2) W. VOLZ, Neues Jahrb. f. Mineralogie etc. Beil. Bd. XX. 1905. p. 354—364.

H. RECK, Zeitschr. d. Deutsch. geol. Ges. 1910. Monatsber. p. 292—318.

3) E. KAYSER, Lehrb. der Geologie, II. 6e ed. 1921, p. 6.: „In gleicher Weise hat W. VOLZ auf der Insel Pulo Laut bei Borneo einen Fall von Aufbiegung der Schichten in Folge eines grossen Magma-Ergusses beobachtet”.

4) J. A. HOOZE, Jaarb. Mijnw. 1888. Techn. Adm. Gedeelte, p. 337—429.

shales, and pitchcoal of about 100 m. thickness superimposes older porphyrite and diabase, while farther southward, in the centre of the island, HOOZE also recognizes diorite and serpentine. VOLZ, on the other hand, maintains that the porphyrites in the north are posteocene. True, HOOZE has not produced cogent evidence for the asserted older age of the porphyrites. This assertion is quite conceivable, however, when we reflect that in HOOZE'S time it was universally accepted that a formation, consisting of porphyrites and diabases, and to which also diorite and serpentine are added, must needs be pretertiary. The only passage in HOOZE'S work that may be called evidential, is found on p. 406: „where the diabase is in contact with the tertiary coal-formation, we are often confronted with a conglomerate-, and breccia-formation (Pamantjangan. Selaru etc.), which, therefore, is usually to be considered as the basis of this coal-formation". VOLZ has justly remarked, that these conglomerates and these breccia of volcanic material might as well be contact breccia (l.c. p. 360). It must be admitted, therefore, that HOOZE has not proved conclusively that the tertiary deposits are younger than the igneous rocks. But neither has the reverse been proved by VOLZ, in spite of his three arguments in favour of the higher antiquity of the sediments (l.c. p. 359).

In the first place he found "im Urwald einen Contact von Porphyrit und Thon; der Thon war direct am Contact auf geringe Entfernungen gefrittet". Now it is certain that for a contact between a very thick diabase- and a porphyrite-mass of 5 km. broad, this contact-metamorphism must be considered very poor. Further contact-metamorphism is absolutely unknown: the composition of the coal is invariably that of the normal Borneo eocene coal; the pelitic rocks possess the normal consistency of old tertiary shales; the sandstones display no trace of contact metamorphism and the Foraminifera-limes and the Orbitoides previously examined by the present writer had changed only very little diagenetically<sup>1)</sup>. However, every one, acquainted with disclosures in primeval forests, knows how little a supposed contact between porphyrite and burnt clay signifies. In a primeval forest with intense, partly lateritic weathering, many things can gradually assume the appearance of burnt clay. Indeed, VOLZ himself seems not to attach great value to this argument.

In the second place volcanic tuffs near Sigam are lying rather level, while in the vicinity eocene layers show dips of 18°—30°; lastly, and this is considered by VOLZ to be the most important argument — in the valley of the Sg. Semblimbingam horizontal tuffs occur between upheaved eocene layers in the west, and porphyrite in the east. It stands to reason — as also appears from VOLZ'S picture — that we have not to do here with a continuous row of disclosures. It now seems to me that these observations cannot prove anything, since VOLZ has not shown that the deposits, supposed to be tuffs, are not detritus of older volcanic

<sup>1)</sup> L. RUTTEN, Jaarb. Mijnw. 1914. Verh. 2. 1915. p. 74—77.

rocks which have been very recently redeposited. This possibility deserves no doubt consideration because deposits in a valley are concerned. It may be difficult to demonstrate this, but unless this proof be brought forward the youth of the porphyrites cannot be considered as established.

So, for the present the age of the P. Laut porphyrites remains doubtful, although there seem to be indications of their being younger than eocene. But we know that there are many difficulties in establishing the age of effusive rocks in the tropics, notably in South Borneo. The literature confirms this. The effusive rocks in the neighbourhood of Pengaron were supposed by VERBEEK to be decidedly posteocene <sup>1)</sup>; HOOZE considered them as cretaceous porphyrites <sup>2)</sup> while Kemmerling again referred them to the posteocene, with some reserve, though <sup>3)</sup>.

In addition I wish to say that a sandstone from Kota Baru in P. Laut, (near the lighthouse) collected by myself some ten years ago, has been entirely (or nearly so) made up of quartz-grains and finely granular siliceous rocks, while also HOOZE (l. c. p. 376) records that the sandstones from the eocene of P. Laut are often of a pure white. This would not imply a higher age of the porphyrites, inasmuch as in the sandstones the detritus of the porphyrites is lacking altogether. There is, however, a possibility that this detritus occurs only in the lowermost parts of the eocene formation.

In the second place VOLZ has failed to show that the entire nucleus of northern P. Laut is composed of diabases and porphyrite, that we have really to do here with an extensive, connected eruptive mass. With the exception of his discovery of diabase-porphyrine at the highest top of the island, HOOZE's observations have been restricted to the coastal zone. VOLZ, it is true, has traversed the whole island ("Das Gebirge ist mit dichtem Urwalde bedeckt" p. 356), but there is no evidence whatever of his having then gathered enough geological data to warrant the conclusion that the entire nucleus of northern P. Laut is one continuous eruptive mass. Given the fact that in the south of this island quartz-diorite and serpentine occur, which elsewhere in S. Borneo are often associated with schists, I do not think it at all impossible that these rocks occur also in the centre of the northern part of the island, and that the material of the sandstones from the eocene has been supplied by the schists. It would probably put any engineer in P. Laut to very little trouble if he should devote his off-days to some excursions in the principal valleys on the west coast, in which the very boulders would supply important hints as to the structure of the rocks in the interior.

So, when propounding his highly important hypothesis: "Die Insel Pulo Laut... (ein) Beispiel einer Hebung durch einen Massenerguss",

1) R. D. M. VERBEEK, Jaarb. Mijnw. 1875, I. p. 3-130.

2) J. A. HOOZE, Jaarb. Mijnw. 1893, p. 1-431.

3) G. KEMMERLING, Tijdschr. Kon. Ned. Aandr. Gen. 1915. p. 717-774.

VOLZ has omitted to prove two very weighty matters: 1<sup>o</sup> that the eruptiva are indeed posteocene; 2<sup>o</sup>. that the nucleus of P. Laut is indeed one large continuous mass of eruptiva <sup>1</sup>).

But even if future investigation should put VOLZ in the right on these two points, Pulu Laut would never afford *conclusive evidence* for the existence of Elevation-Craters or of related phenomena. It may reasonably be *believed* that the eruptiva of P. Laut have pushed up a portion of the earth's crust, but every effort to prove this would be hampered by the fact that P. Laut lies in the region of S.-E. and E. Borneo, where folding is a normal phenomenon, and where the main trend of the folding movements coincides with the direction of the eocene strata in the island, so that it might always be argued that the upheaval of the strata results from normal folding <sup>2</sup>), which would, indeed be the most plausible view. VOLZ slurs over this difficulty when he says: "Zwar wurde Indonesien nach dem Eozän so wie an der Wende von Tertiär und Dilivium von Faltungen heimgesucht (deren Wesen und Wirkung aber für Borneo so gut wie unbekannt ist), aber ich glaube nicht, dass man die oben beschriebenen, eigenartigen aber geringfügigen Dislokationen auf eine Faltung zurückführen kann" (p. 363). Now, VOLZ could have known that as early as 1905 records were on hand, from which various information regarding the folding in Borneo could be obtained. Not only had C. SCHMIDT <sup>3</sup>) pointed out, in accordance with modern conceptions, that there are in N. W. Borneo normal, tertiary folding-mountains, but since long folding processes in the tertiary had also been established for S. Borneo and E. Borneo <sup>4</sup>). It might still be assumed that in some localities those folding processes were owing to the action of igneous rocks — a conception which, from its very nature, may occasionally be met with in the oldest literature — but in many other localities no facts could be discovered that implied the co-operation of igneous rocks in the folding process. In some places the folds were very sharp, in others weak, while the anticlines displayed the semi-domelike shape, which is known from P. Laut (i. a. at the Berau-river. HOOZE l. c.). Besides in VOLZ'S time already the literature contained records concerning the general tectonic trend in S. E. and E. Borneo N.-E. to N.-N.-E., which consorts with the main trend of the coal-layers

<sup>1</sup>) While I was correcting the proofsheets E. GÖLLNER'S publication (Jaarb. v. h. Mijnw. 50, Verh. 1, 1921, 1924) came to my notice. It seems to show that VOLZ'S view is erroneous, and that porphyrites and the diabases of P. Laut are pre-eocene.

<sup>2</sup>) It should be noted that the interpretations given by HOOZE of the causes of the upheaval of the eocene strata in P. Laut are very primitive, and slightly contradictory inter se: VOLZ refers to these conceptions at large — which, by the way, were completely out-of-date even in his time.

<sup>3</sup>) C. SCHMIDT, Bull. Soc. géol. de France (IV). 1. 1901. p. 266—267.

<sup>4</sup>) See also for S.-E. Borneo VERBEEK l. c. 1875, HOOZE l. c. 1893; for Kutei HOOZE Jaarb. Mijnw. 1887 Tech. Adm. Ged. and 1888 Techn. Adm. Gedeelte; for Berau HOOZE Jaarb. Mijnw. 1886 Tech. Adm. Ged.

in P. Laut. These data have of course been largely accentuated by subsequent researches <sup>1)</sup>, so that we feel sure nowadays that a perfectly normal tertiary range of folding mountains runs all along the east coast of Borneo, certainly as far as the Dutch boundary in the north.

Consequently VOLZ'S pronouncement that P. Laut is an instance of upheaval through volcanic eruption is unproved, unprovable and contrary to all reasonable expectation.

H. RECK (l.c.) thought he could point out several Elevation-Craters in Java, but in doing so he committed even more serious errors than VOLZ. In the first place he refers to the G. Lima to the east of Patjitan: In the middle of VERBEEK and FENNEMA's map <sup>2)</sup> there occurs a diatrema: "zwischen ihm und das jüngere Miocän schiebt sich ein Kranz älteren Miocäns, das den Kern von allen Seiten umschliesst, ein Lagerungsverhältniss, das nur durch die vulkanische emportreibende Kraft des zentralen Kernes erklärt werden kann" (p. 308). Obviously RECK has neglected to study properly the text accompanying VERBEEK and FENNEMA's map, else he would have seen, that the nucleus of the G. Patjitan is referred to the "old Andesites", that the inner rim of miocene belongs to the  $m^1$ , and is, therefore, younger than the nucleus, and that the outer rim of  $m^3$  is of course still younger; consequently it is clear that according to VERBEEK and FENNEMA the older andesite can never have pushed up the younger miocene ( $m^1$  and  $m^3$ ). It is very well possible, though, that the true condition differs from the one illustrated on the map, but even then the existence of "Elevation-Craters" cannot reasonably be assumed. At all events for the present the G. Lima near Patjitan does not furnish any evidence *in favour* of the Elevation-Crater theory. Furthermore RECK has endeavoured to prove that the G. Butak in North-Rembang is a typical Elevation-Crater (because the tertiary strata dip away radially from the mountain) and that the present elevated position of the tertiary mass between G. Putjak and Lasem (Ngargopuro) resulted from a volcanic upheaval, "da eine so starke tektonische Aufwölbung in dieser Gegend gänzlich überraschend wäre" (p. 310).

Here again RECK has disregarded without bringing forward fresh evidence VERBEEK and FENNEMA's view that the three andesite-massifs belong to the "old andesites" and are older than the surrounding  $m^3$ . But even when admitting what came true later on <sup>3)</sup>, viz. that the volcanic masses of North Rembang are younger than most sediments surrounding them, it will not do to use RECK's arguments in favour of Elevation-

<sup>1)</sup> See i.a. H. JEZLER, Z.f. prakt. Geologie. 1916, p. 77—85, 113—125; L. VAN ES. Jaarb. Mijnw. 1917. Verh. 2. p. 5—143; L. KROL, Jaarb. Mijnw. 1918. Verh. 1. 1920, p. 281—367.

<sup>2)</sup> R. VERBEEK et R. FENNEMA, Géologie de Java et Madoura, 1896.

<sup>3)</sup> C. 'T HOEN. Jaarb. Mijnw. 1916. Verhand. 2. p. 202—254.

Craters. Indeed, from VERBEEK and FENNEMA's map we knew already that North-Rembang is situated in a true folding region. Now it is not at all strange that when a complex of sediments including a diatrema are folded collectively, the sedimentary strata will, after the folding process, usually dip away from the more rigid eruptive masses. It might also be conceived — without assigning elevating power to the magma — that in certain cases an eruptive mass, which forces itself upwards just when the folding is taking place, goes to the highest parts of the anticlines, i. e. to their dome-shaped elevations. Neither is the fact that part of the tertiary sediments lies on a rather high topographical level, in any way indicative of volcanic upheaval, as we knew already from the profile of VERBEEK and FENNEMA that considerable amplitudes occurred during the folding of the tertiary, and also that since that folding intense denudation has taken place. The actual occurrence of tertiary masses on such a great height may be readily explained if we assume that there the tertiary has been preserved between two more resistant masses of volcanic rocks, whereas elsewhere it has, at such a level, been long since denuded.

Meantime our knowledge of the geology of the region has been greatly clarified by 'T HOEN's profiles (l.c.) so that RECK's conceptions are still less excusable. It now appears that the G. Butak andesite has pushed itself upon an anticline, but not on its highest part, because from the W. the sediments dip away under the G. Butak. It also appears, on the contrary that the G. Lasem has come to eruption above a synclinal region and that on all sides the sediments dip away under it, while lastly the G. Putjak has come to eruption at the side of an anticline. Further it appears that, besides the half-dome in which the G. Butak-andesite has broken through, two other beautifully formed domes occur in the territory (that of Ngandang-Lodan and that of G. Banju-Uja) into which, however, no andesite masses have broken through. He who is familiar with the geology of Java, also knows that further southward in Rembang there are still a number of dome-shaped elevations, on which the oil-boring territories are situated. The domes, therefore, have originated through normal orogenic movements and only once — the G. Butak — has an andesite-mass broken through.

*The conclusion, therefore, is warranted that neither the observations made by VOLZ, nor the interpretations brought forward by RECK tend to prove anything for the existence of Elevation-Craters in the Dutch East Indies.*

Utrecht, Jan. 14, 1925.

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