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Alpine Folding as related to Continental Drift¹)

by **Theodoor Raven** (American University, Beirut, Lebanon)

With 1 figure in the text

The hypothesis of continental drift has recently obtained considerable support from palaeomagnetic research (Irving 1959) and from a renewed comparison between S-American and S-African geology (Maack 1952). As early as 1931 Holmes (1931, 1944) pointed out that convection currents in the mantle may well carry the continents.

The crucial point in Holmes' mechanism is that in the zone of compression the basaltic ocean floor forms a downbuckle and that compression in the lower parts of this downbuckle metamorphoses the basalt into eclogite with a density of at least 3.3. This point has recently found support by experiments in the USA producing this transformation (vide Girdler). Once having acquired a greater density than the peridotite of the mantle, this transformed root must actively pull the higher parts of the crust down. This might cause tension faults contemporaneous with and adjacent to compression, the latter being restricted to a narrow belt (Bull, 1931).

The purpose of this paper (which is part of a series under preparation) is to show how this hypothesis fits modern ideas about alpine palaeogeography, in particular as summarized recently by Trümpy (1958).

The old idea that the Mid-Atlantic Ridge and several other mid-ocean ridges must be explained as scars showing where the continents fitted together before drifting apart must be retained and is corroborated by recent research of Ewing and his co-workers. Even if some parts of these ridges must have drifted, e.g. the Mid-Atlantic Ridge between 10° S and 15° N latitude, a simple and reasonable assumption is that generally they remained where ascending branches of the subcrustal convection system diverged; i.e. during a convection cycle these «divides» did not shift laterally.

The Mid-Atlantic divide has a continuation across the Arctic Ocean: the Lomonossow Ridge, which joins the shelf near the New Siberian Islands. Inspection of the new geological map of the Soviet Union shows a likely pivot point at 77° N, 130° 40′ E, around which the Eurasian continent rotated anti-clockwise while the pivot point shifted SSE over 2 or 3° of arc. Reversing this combined movement we can make Europe's shelf edge meet the Mid-Atlantic Ridge S of Greenland and a slight adjustment of Greenland's position suffices to fill the actual gap between the Caledonides of Spitzbergen and Scotland (Collette 1958).

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¹) Communication, presented under the title "Dérive continentale causée par courants dans le substratum" at the meeting of the Swiss Geological Society, Lausanne, 12 september 1959.

The southern foreland of the Alps, Africa, also started its drift from the Mid-Atlantic Ridge, apparently at or after the end of the Trias. Africa drifted without apparent rotation to the NE over some 2300 km. We must assume that originally a wide and deep Tethys separated the continents; its sediments may have been dragged downwards with the basaltic ocean floor and consequently I am unable to put specimens on the table as evidence. In this respect I share the plight of most geophysicists.

My plight looks worse because my reconstruction evidently does not fit Argand's brilliant concept of alpine embryotectonics. However, a recent paper by Trümpy (1958) removes this obstacle from my path, showing that in the Swiss and French realm mesozoic sedimentation was far less typically geosynclinal (in the orthodox sense) than has been thought until recently. A few quotations may suffice to show that at least in that area no well established facts prove my story to be false. «In 1949 the northern margin of the Liassic Thetys was shown to be governed by a system of antithetical tension flexures. This kind of structure excludes crustal shortening in this external part of the geosyncline during Liassic time; it denotes, on the contrary, tension phenomena. – In the Median Prealps there is no evidence of thrusting or even of folding during the Jurassic. – In the Piemont geosyncline (Upper Jurassic) sedimentation is extremely slowed down, in spite of or because of deep sea conditions, by the lack of detrital matter. – The facies of radiolarian cherts and pelagic limestones is not limited to the Pennic Piemont geosyncline (leptogeosyncline), but transgresses on to the austroalpine realm and even further to the south. During the Upper Jurassic the whole area to the inside of the Briançonnais rise must have been covered by fairly deep water. Judging by the almost total absence of detrital sediments, tectonic activity was much reduced. However, some submarine breccias, of similar type as those of the Breccia nappe, were formed along local scarps, e.g. in the Sonnwend mountains northeast of Innsbruck.»

This picture is in perfect harmony with my contention that during the whole Jurassic period subsidence of the Tethys floor was extremely slow (restricted to reaction to loading with sediments plus crustal shrinking due to cooling) with the exception of a narrow axial downbuckle forming a basaltic root. I cannot, however, resist the temptation to offer an alternative interpretation of part of the submarine breccias. Probably not *all* deepsea sediments of the axial belt were dragged down inside the root (as suggested above) – quite likely part of them have been stripped off so as to fill the axial leptogeosyncline. Eventually they should then form a crumpled pre-orogenic submarine ridge, subject to wave erosion before and after partial emergence. Where coral reefs (R on fig. 1) gained a precarious foothold on this axial ridge, their debris should form part of the breccias, the other elements being consolidated deepsea sediments. The ridge's central position in the deep Tethys should be evidenced by absence of terrestrial (continental) clastics. Due to its position over the continuously descending basaltic layer which shoved the deepsea sediments from both sides toward and under the ridge, submarine landslides, slumping and turbidity currents (the latter of unconsolidated sediment) should repeatedly adjust the ridge's slopes, undermining reefs and islands.

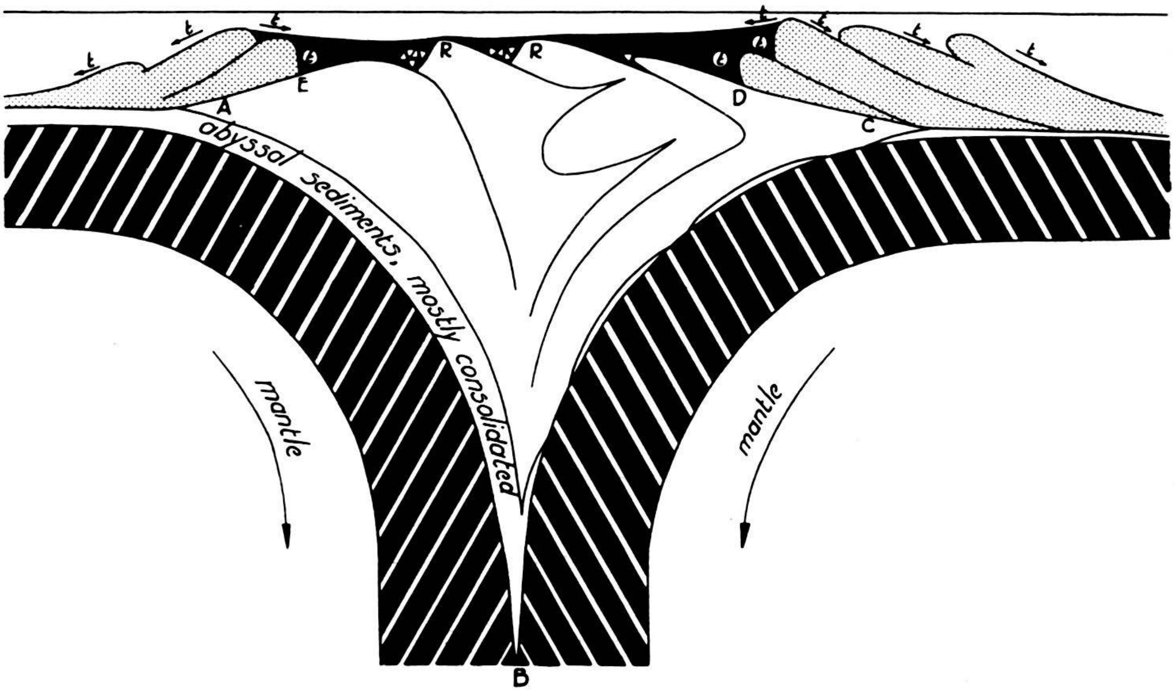

Fig. 1. Schematic section, not to scale, of hypothetical cordillera in tectonic axis of the Tethys at end of Lias: a pile of nappes whose roots vanished in depth.

Black: partly pelagic, partly neritic limestone with multiple intercalations of material of abyssal origin (e. g. radiolarites) deposited by turbidity currents (t).

Stippled: overthrust unconsolidated abyssal sediments scraped from underthrust basaltic ocean floor.

Hatched: normal basaltic ocean floor-moves with substratum.

A–B–C–D–E: crumpled consolidated abyssal sediments, possibly with ophiolites and temporarily capped by reefs R R.

A–B and C–B: shear planes separating same from whatever sediments are underthrust by converging branches of convection currents. Red deepsea ooze forms an excellent lubricant for the underthrust.

Frontal parts of the embryonic nappes may break off near R R leaving fault scarps with breccias (white triangles) at their base.

Continued drift might finally shove the sial of the continents under the rootless nappes (or crumpled sediments) or these may sink to form a "Zwischengebirge" or be dragged down deep into the basaltic tectogene when the convection currents gain force.

This chaotic picture can apply only to breccias formed near the axis, i.e. on the «African» side of the Piemont trough; those formed along the Briançonnais platform must have a different history and composition.

Evidence of turbidity flows has been found by Carozzi in various alpine localities. Where the turbidity mud was made only from unconsolidated deepsea ooze, lack of coarse grains might impede formation of visible graded bedding and the turbidity-origin will be almost impossible to detect. Thus locally observed great apparent thickness of the Schistes lustrés might be due to resedimentation in a narrower area of deposits formed over a wide belt: original thickness might thus be doubled or multiplied tenfold. If, as is commonly believed, tectonic repetitions again double the apparent thickness (Trümpy, 1958, p. 345 last paragraph) measuring or computing the Schistes lustrés' *original* thickness becomes a wellnigh unsolvable problem.

«After its interruption during the Upper Jurassic, detrital sedimentation sets in again: but the paleogeographic pattern is quite distinct from the earlier one» (Trümpy, 1958, p. 347). «One of these accidents (a longitudinal fault in Glarus) becomes a steep thrust-fault in the upper Valanginian and Hauterivian. This may eventually indicate the reversal from tension to compression mechanism.» In central Iran strongly metamorphosed and folded Jurassic deposits are overlain by non metamorphic Barremian (oral communication from Dr. A. ten Dam). This important orogenic phase is also known in the Caucasus and related ranges. All these facts might indicate that at Jurassic's end the northernmost peninsulas of the northeast-ward drifting African continent collided with the slower and essentially eastward drifting Eurasian continent: sialic promontories became now involved in the buckle and consequently the root's buoyancy made itself felt. The «reversal from tension to compression», viewed in this light, does not mean a reversal of the mechanism which ruled the Tethys' evolution, but indicates that as a result of Europe's drift Glarus (like the Caucasus and Iran) entered the belt of compression which should have existed near Tethys' axis during the whole time of drift. The fact that no undoubted vestiges of this Jurassic (deduced) compression have yet been found in the alpine realm could be adequately explained by their downward disappearance in the root, analogous to the «Verschluckung» advocated (sometimes overemphasized) by Kraus. Thus the Jurassic root of the Tethys tectogene would be composed of basalt (gradually converted to eclogite by metamorphosis and thereby acquiring a high density) plus a growing amount of sediments of the deepsea. The latter also could not escape considerable metamorphism when dragged down more than 500 km in the root: the resulting schists might well have densities of more than 3 (if not too much quartz were involved – red and blue muds would do better than radiolarites). Under favorable circumstances this root could be dragged down almost without buoyancy in a substratum of density 3.3. Growing buoyancy – resulting from more sediment plunging down – could keep balance with growing force of downdrag as the convection currents gained strength.

The first collision of the two drifting continents can be dated by the Iranian and Caucasian evidence as end Jurassic. If the balance mentioned in the previous sentence were imperfect, and if the sial now entering into the root exercised sufficient buoyancy to brake (even to stop) the undercurrent's downwarp, one can imagine a sort of traffic jam under the continents: incoming sima from the upcurrent temporarily being in excess of outgoing sima in the downwarp. This hypothesis would explain the widespread regression (continental emergence) as testified by Purbeckian and Wealden facies both in NW Europe and in the Near East.

Another result to be expected of the collision is subsequent directional adaptation of the undercurrents to the formidable and locally concentrated obstacle which they now met. The front, never necessarily having been a straight line in the Tethys' axis, can now reasonably be assumed to have split and spread radially so as to form great lobes like those now seen in the Western Alps and the Carpathians. The event is well dated by Trümpy's statement «the (Lower Cretaceous) paleogeographic pattern is quite distinct from the earlier one». The dating tallies with that of an embryonic folding of the Jura mountains, where the Purbeckian facies and thickness distribution is closely related to the actual folds which were finally modelled only in the Pliocene (CAROZZI).

Why was not the main alpine orogenesis contemporaneous with or immediately following the continental collision? Criticizing Wegener,

> SCHUCHERT wrote (1928) "We are to believe, under this supposition, that this immense rifting and drifting of the continents went on during one of the earth's most marked times of crustal quietness, the early and middle Cretaceous, when almost no mountains were made in the whole world – a time almost devoid of volcanic activity, when the continents were about as peneplaned and low as they ever have been, and when they were flooded by the greatest oceanic transgression of all times. The drifting continued during almost the whole of Cretaceous time, which, on the basis of radium disintegration, means for 65 million years, before the continents showed any marked crustal unrest, or even marked volcanic activity. WEGENERS' theory emphasizes accumulating or lagging effects in the crust, but why lagging of something like 50 million years?"

My answer is that the acme of *compression* must occur when the convection currents had optimal speed and power but the acme of *drift* was when the slowly accelerating currents were not yet bothered by sial in their descending branches, i.e. before the wide Tethys was bridged by sial in its floor. A current is strongest when it has moved 2900 km (= the mantle's thickness) but Africa collided with Asia after a drift of only about 2100 km. Thus the drift was impeded at a time when most of the cycle's potential energy was unspent.

However, the effect of continental sial entering the root must have been tremendous: as soon as the mean density of the composite root had fallen below about 3.3, this could not descend any more -it would be kept in temporary equilibrium by the sial's buoyancy. How then could the energy of the rising current (under the divide) be effectively used for later folding? I see only one possibility: to resume movement in the descending column (under the eugeosyncline) the sima must get rid of the brake exercised by the sialic root: it tore itself loose from its moorings. This means that whereas first the sima current carried crust and sediments on its back (like does a conveyor belt), from the beginning of Cretaceous times onwards continental drift of Africa and Eurasia could not be more than equal to crustal shortening as evidenced by the later folding. This structural evidence is far from easy to interprete quantitatively – 300 km seems a fair estimate for the Swiss Alps. This then would be about the maximum of Africa's post-Jurassic drift -assuming that Europe did not proceed southward.

We now have to face the problem whether also *in the mantle* post-Jurassic flow was restricted to about 300 km under the Tethys. If so, the convection cycle would remain uncompleted, checked effectively by the continental «moorings». But it appears mechanically impossible that convectional rising under the Atlantic could be impeded by a brake applied only under – and by – the continents. The result surely would be folding of the Atlantic's floor with its sedimentary cover or of the Atlantic margins of Africa and America. Consequently I must assume that the convection system reacted to the changed conditions by parting company with the continental crust. In other words: as from the early Cretaceous the mechanism switched from HOLMES' recipe to that of VENING MEINESZ.

Is this solution improbable? I claim that on the contrary the separation of the crust – remaining now almost on the spot – from the substratum flowing under it is less difficult to conceive in stage 2 than in stage 1 of the cycle. A car does not skid when it starts very slowly (as do the convection currents) but when it goes fast. It skids when the brake is applied – in our case the sial entering into the root applies the brake. Moreover this braking effect, making itself felt in different places at various times as and when progressively thicker sections of sial were drawn into the slowly growing roots, seems absolutely essential to explain the occurrence of *several* folding phases as due to *one* convection cycle. Without intermittent braking the convection mechanism would result in *one* compressional phase reaching its final folding at the end of stage two, although this end may have occurred at different times in different places. Assuming two or even three convection cycles after the Trias can not solve the problem and is moreover inconsistent with the time required for heating the lower part of the circuit.

The braking effect, alluded to above, is due to the buoyancy of any light material, not necessarily only granite. It is conceivable that radiolarites could float like scum on a quiet spot of the convection front e.g. where the Africa current spread in the concave side of the Carpathian arc. If accumulated to sufficient thickness (more than 10 km) they could finally offer a passive resistance almost equal to that of the granitic-gneissic «Zwischengebirge» – in the orogenic belts buoyancy has a much greater effect than has mechanical strength in determining the pattern of mountain ranges.

Nevertheless one may ask whether two of my assumptions are acceptable, viz. complete absence of granite under most of the Mesozoic Tethys and presence in its axis of a narrow belt of compression during the whole time of drift.

The former assumption requires only insignificant amendments to Trümpy's statements. «The sialic crust was certainly thinned under the Piemont trough, but the gneiss cores of the pennic nappes are there to prove that it was not altogether removed» (1958, p. 348). My interpretation of the facts is: the thin sialic crust represents the wedge-like margin of the European continent and then the sial-free axis of the Tethys must be situated south of it. The sudden appearance of ophiolites in the Lower Cretaceous suggests that at that time (when the substratum started flowing away from under the moored continents) warmer peridotite (or eclogite according to Lovering (1958)) appeared under the sea: the fact is in perfect harmony with my hypothesis.

As to not being able to show the «cordillera» which I assume to have been present in the narrow axis of compression, I am no worse off than Trümpy, who writes (1958, p. 349) «Curiously enough, some of these cordilleras seem to have vanished into thin air, like the one which furnished great quantities of a peculiar type of granite into the ultrahelvetic flysch». I suggest that these missing cordilleras shared the fate of my missing link: they plunged into thick sima when the down current became strong enough to drag them down.

«All our paleogeographic reconstructions are piecework, as many elements have disappeared altogether, either drawn into the depths of the infra-structure or worn away during one of the (later) erosion phases.» As this applies to the world's most intensely probed mountains, I hardly need to apologise if my intended contributions to this worldwide piecework are based often on extremely flimsy evidence. Indeed many elements must have disappeared – downdrag and suction by descending currents, once admitted, *must* have affected the Tethys' axis and therefore the evidence simply cannot be complete. To some extent the drift hypothesis might encourage alpine geologists to consider new interpretations of old facts. Thus «certain décollement nappes, consisting only of post-hercynian sediments and sheared off along an incompetent horizon» (like the Median Prealps – op. cit. p. 351) might never have had a sialic or hercynian basement, they might well have their origin on the flanks of my vanished central cordillera and their basal shear planes might have functioned as such while the conveyor belt shoved underneath (see fig. 1).

Evidently the mechanism must have brought together sediments which were deposited more than 1000 km apart and therefore had different facies. One can easily imagine deposition of shallow water limestones on one flank of the central cordillera being periodically camouflaged by large-scale turbidity flows of radiolarian ooze which was continuously piled onto the cordillera as the deepsea floor at the other side was shoved under it (cf. the curious situation of the radiolarites of Barbados (see figure 1)).

To clear the way for continental drift I adopted Holmes' idea of a wide Tethys separating Gondwanaland from Laurasia. Therefore we must now inquire whether paleogeographic evidence allows this essential departure from Wegener's conception of Pangaea.

The answer is positive, some points deserving special consideration are:

(i) The northern hemisphere's Carboniferous coal fields find their proper place in the tropical belt, though not centered symmetrically around the reconstructed equator.

(ii) Those Permocarboniferous tillites which cover areas too wide to allow of their interpretation as mountain glacier deposits (many were formed at sealevel, as testified by their relation to marine sediments) are grouped conveniently around the South Pole.

(iii) Two landbridges permit land animals and plants to cross the Tethys at all times when they were not flooded. The eastern landbridge is still present (but out of commission) as a continuous sialic swell connecting SE Asia with Australia. The western one comprises the sialic crust under Italy, which is allocated geologically to Africa by all alpine geologists, and a part of the ocean floor between the Azores and Portugal and Morocco. In the latter area Bourcart found evidence for a continental flexure, which was attacked subsequently by Gigout (1952) but a general dip of the Lower Pleistocene of 5° seaward was conceded as a working hypothesis. A downwarp of this order of magnitude would suffice to founder a continental segment to oceanic depth. Relatively shallow depths between Madeira and Lisbon and suspicious behaviour of seismic waves support the idea that here (exceptionally) local foundering of former land is probable.

Before drift this former land must have linked the Italian promontory of Africa (its Northern tip then situated near 11° W 31° N, coordinates of present globe) with Portugal, which then was touching the Azores, whose position does not appear to have shifted. Soon after Jurassic's end the function of this cross-Tethys landbridge was taken over by Arabia then meeting Iran.

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