ELASTIC OR PLASTIC BUCKLING OF THE EARTH'S CRUST

by

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In a very simplified form one could say that with the exception of the Undation theory of VAN BEMMELEN (1933a—d, 1948, 1952) and suggestions by GLANGEAUD (1947, 1948) the orogenic forces are generally sought in a lateral compression either by contraction due to cooling (JEFFREYS, 1952) or under the influence of convection currents (VENING MEINESZ, 1934, 1950; GRIGGS, 1939; HESS, 1948, etc.) below the crust. In the original conception of VENING MEINESZ the buckling started as an elastic deformation but since BIJLAARD (1938) showed that plastic buckling oblique to the main stress direction was more probable, VENING MEINESZ changed over to this view. The difficulty in the elastic buckling hypothesis was the enormous stress (40—50.000 kg/cm^2) needed to buckle a crust of some 30 km thickness, a force which long before the elastic waves were formed would have crushed the material. With SMULOCHOWSKI (1909) VENING MEINESZ considered the possibility that a layered crust (some 15 layers would be needed) could be elastically deformed by a much smaller stress (3000 kg/cm^2), but there was no reason at all to expect such discontinuity surfaces in the crust, and the idea had to be abandoned.

The buckling, whether plastic or elastic, was proposed in order to explain the negative anomaly zones, found by VENING MEINESZ to be running parallel and next to the great Indonesian island arcs. The depth to which the buckle of sialic crust penetrates into the sima, causing the mass defiency measured at the surface, depends in this theory on the total compression and on the age of the phenomenon, because fusion at the bottom of the buckle would diminish its size gradually. Therefore the root below the Alps had its size decreased since the major paroxysms in post Eocene and Miocene times.

VAN BEMMELEN attacked this origin of the root buckle of the Alps wrongly on the grounds that a compression zone with some 400—500 km shortening would be some 8 times larger than its actual gravity minimum could explain and BULLARD asked in the Hershey colloquim (Trans. Am. geoph. Union, vol. 32, no 4, August 1951) whether modern concepts of gliding tectonics could not bring this supposed controversy into agreement.

Evidently the buckling of the earth's crust by compression is still a controversial subject.

Geologists never could be very enthusiastic about BIJLAARD's plastic zones oblique to the stress because the shape of the surficial folds and their consistent longitudinal alignment parallel to the orogenic axis is strongly opposed to a deformative force not perpendicular to this orogenic belt.

In 1938 KUENEN and DE SITTER published a joint paper on several experimental illustrations on folding mechanisms. The one that interests us mainly at present is here reproduced as fig. 1. In a thoroughly kneeded

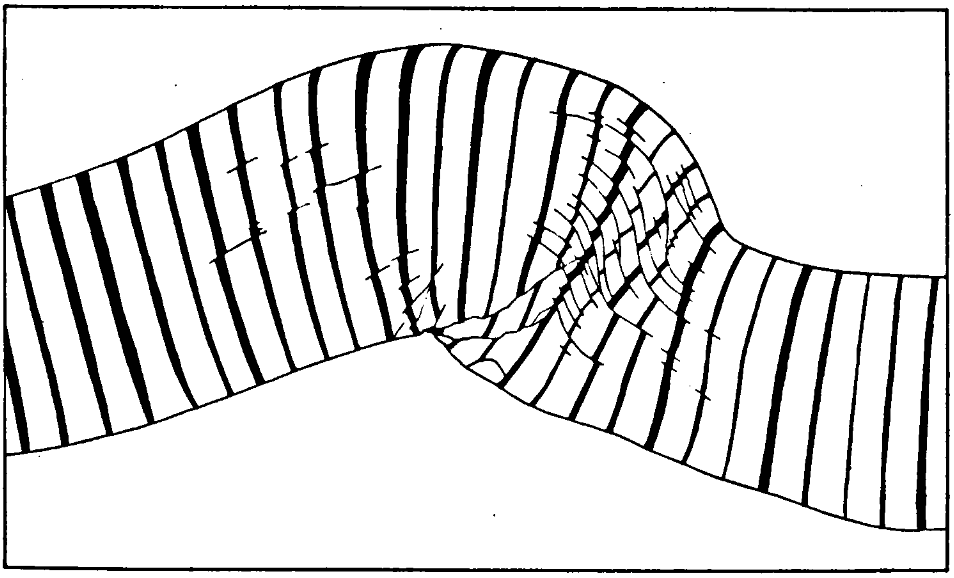

Fig. 1. Clay cake provided with black ink stripes, folded by tangential stress. Spontaneous shear planes developed parallel to upper and lower surface. (After KUENEN & DE SITTER, 1938.)

claycake, given an initial very slight bending, shear zones developed parallel to the upper surface under lateral stress, first one more or less in the centre, then subsequently three and more, each time dividing the remaining unsheared cake in two. The shear zones developed in the flank of the initial structure, the anticlinal and synclinal axes remained free from shear planes.

No valid explanation was given, because the nature of the deformation of rocks was insufficiently understood at that time. Since then many laboratory experiments on deformation of rock specimen have proved that although the deformative stress may rise above the yield point and cause permanent or plastic flow there still remains a recoverable strain in the specimen after unloading (cf. DE SITTER, 1952b). The elastic properties of the rock are not lost during its plastic and viscous flow during the folding process. BURGERS (1935) has pointed out that such mechanism can be understood when we imagine the elastic strain being slowly replaced at successive points by a permanent adjustment, a point of view which is wholly consistent with the experiments. This elastico-viscous flow explains why in a folded mountain chain like the Jura a series of parallel folds can be formed simultaneously, a very common arrangement in nature. The whole sheet of sedimentary rocks is everywhere in the same elastic stress field and slow permanent adjustments gradually replace the elastic strain. When the initial elastic arch of our clay cake had been formed the elastic strain originated secondary stresses which caused a stretching of the outer arc and a compression of the inner arc. In a section of the arc we then find these secondary stresses diminishing downwards and after passing a zero point changing to the opposite direction thus setting up couples parallel to the upper surface as has been theoretically deduced by KIENOW (1942) and experimentally proven by the above mentioned experiment. The shear planes in the clay cake of our experiment represent the proof of KIENOW's theory, and prove that concentric folding, that is folding along shear planes parallel to the bedding, is even possible in unstratified units of a sedimentary series.

When this principle is applied to buckling of the earth's crust it becomes obvious that the crust need not have an original stratified structure but that the initial elastic strain will itself generate the shear planes, and in the numbers that it needs, to attain the curvature that its thickness and the time the stress is active prescribe. The deformative force can then never grow beyond the yield value of these shear planes, the rock will not be crushed but will be folded in a permanent fold, the shape of which is governed by the elastic stress field.

I think that the first objection against elastic and plastic buckling has been met adequately.

The second objection, the discrepancy between the observed shortening and the observed mass of the root is more complex and more difficult to refute.

In the first place I should like to point out that there are many Tertiary mountain chains where the transverse shortening is so slight that no appreciable thickening of the crust can be imagined (cf. DE SITTER, 1952a) the High Atlas for instance. Others like the Pyrenees or the Apennines might have a shortening of some miles during its Alpine phases, but perhaps hardly enough to explain a root. Therefore the question arises only in the case of extreme folding as in the Alpine mountain chain, where a maximum shortening of 480 km has been postulated (cf. J. CADISCH, 1953, p. 287). However, most geologists will agree that this extreme value is excessive. There are several reasons to expect a much more modest figure to be nearer the truth.

First of all R. STAUB (1924) ascribed the sinking of the geosyncline preceding the compression by a stretching and thinning of the crust, others suppose that in the long geosynclinal phase, part of the bottom of the sinking crust may melt and be transported sideways. The total mass of sial would decrease, and there would be less to be pressed downward.

Secondly the great nappes of the Helvetian type are regarded nowadays as sheared-off slices which originally were situated as tiles of a roof one *on* the other (DE SITTER, 1939) and not one *behind* the other as was thought originally. Moreover much of their displacement was not due to compression, which was responsable only for the initial shearing phase, but to gliding down a slope, originated by an uplift in the core of the mountain chain.

Thirdly the great Pennine nappes have a distinctly different mechanism of deformation than simple folded surficial rocks. They consist largely of recristallized rocks, gneisses, where stretching takes a very important role in the deformation process. The less metamorphic Triassic rocks which separate the units are most certainly also severely stretched and torn, their present length may be several times longer than the original breadth of the basin. Moreover the views on the mechanism of the formation of these nappes have greatly changed. NABHOLZ (1945) describes their early history as a continuous process of sinking of the bottom of the several parallel basins, joining them at the end to one basin in which the Bündnerschiefer and its Flysch blanket were deposited. The final folding stage becomes then more like an upward surge of already imbricated structures of the basement, which in this way would not contribute to the downward accumulation of sialic matter in the root.

WENK (1953) argues that quite possibly several of the granite gneisses which form the cores of the main Pennine nappes may be of an Alpine age and that their channels of intrusion are found in the so called root zones. This implies that the steepness of the root zones is no longer a purely structural phenomenon and that perhaps much of the horizontal lobes of these nappes are intrusive contacts and not only due to folding. The radical hinges of the nappes need no longer be hidden in the root zone but are simply the southernmost mesozoic wedges between the nappes. All these considerations work in one direction, a considerable reduction of the supposed shortening of the Pennine nappes.

Finally the Austro-alpine and Préalps nappes contributed originally considerably to the total amount of shortening. But when we see that the Préalps are sometimes no longer regarded as having their origin south of the Pennine zone (TERCIER, 1952), and the Klippes may have been carried forward on the back of the gliding down Helvetian nappes, their contribution to the shortening becomes considerably less. Moreover I think it is not probable that each of the Austro-alpine nappes had their own root, at least the Upper Austro-alpine nappe is largely a sheared-off blanket below which part of the compression of the Pennine nappes took place, and therefore its contribution is already included in that of the Pennides and does not contribute anything itself to the general shortening (DE SITTER, 1947).

It is impossible to estimate the actual shortening when we take into account all these different and complicated mechanisms, but I think that we may safely assume that these deductions together amount at least to three fifths of the maximum amount. The shortening would be reduced to some 160 km or even less which means a maximum depth of the mountain root not exceeding much the measured mass deficiency. Moreover, with this value of the total compression the Alpine geosyncline would become about a straight line in its pre-folding phase.

Having met two serious objections to the bent buckle as the root-forming mechanism of a major orogenic belt it may be pointed out that no preference can be given on these grounds to the contraction — or the convection or any other current hypothesis. Neither do these arguments really affect the grounds on which the concept of a mountains root is based. In a former paper (DE SITTER, 1952a) I called the attention to the fact that not all lofty Tertiary mountains chains are due to great compressions, neither can the post-tectonic uplifts always be attributed to isostatic adjustments due to the existence of a mountain root.

EWING (1954) showed that no buckle need be present below a gravity anomaly above a deep sea trough when he found by seismic evidence that the deep Puerto Rico trough was filled at its bottom with such a thick layer of loose sediments that the shape of the trough could explain all the mass deficiency. We can challenge even the evidence of real strong mountain building compression in the great Sunda arc of Indonesia when we ascribe the supposed thrust structures of Timor, where Triassic Klippes are lying on much younger strata, to gliding instead of to large scale low angle thrusting.

Another axioma of the buckling hypothesis which could be challenged is the supposed viscous nature of the substratum. Its viscosity ratio has been calculated from the post-glacial rise of Scandinavia due to the melting of the glacial ice cap. Many objections against this procedure can be made. In the first place the rise has been recorded by raised marine erosion terraces on the coast of Norway, *i. e.* each terrace represents a pause in the upheaval process, and we possess no method to evaluate the relative length of time of the pauses and the active phases of this process. Hence the velocity of upwarping remains unknown and consequently the calculated viscosity is a maximum.

A more serious objection can be raised, however, against the theory that the rise is due to an isostatic postglacial unloading. A part of the Scandinavian peninsula, together with the Botnian Gulf and Finland constitute together the Scandinavian Pre-Cambrian shield, which since the last 500 million years has been a positive region of the Earth's crust. Its southern limit, running from the great lake district on the Finno-Russian frontier to the Swedish province of Skania, has again and again been the principle hinge line between a rising block in the North and a subsiding block in the South. A considerable doubt seems justified when the last recorded rise is assumed to have a different cause from all the preceding ones. When the unloading due to melting of its ice cap is only a secondary effect, which seems much more probable than it were the cause, we remain completely in the dark about the stress which caused the postglacial elevation and hence about the viscosity constant of the substratum.

It seems, therefore, that, although some objections against the buckling hypothesis can be met, so many of its postulates and generalisations are of doubtful value that we must handle it with considerable circumspection.

Postscriptum

Since the manuscript went to the editor I read the Symposium *Crust of the Earth,* special paper 62, Geol. Soc. of America, 1955. I found that STILLE on p. 173 has the same doubts on the same grounds as I have on the origin of the post-glacial rise of Scandinavia. Further I found that BUCHER (p. 347) and WORZEL (p. 87—99) also introduce the conception of a thinning of the crust (stretching) in the geosynclinal stage, originally proposed by STAUB.

References

BEMMELEN, R. W. VAN, 1933a. Die Anwendung der Undationstheorie auf das alpine System in Europa. Proc. Kon. Akad. v. Wet., vol. 36, pp. 686—694.

——, 1933b. Versuch einer geotektonischen Analyse Südostasiens nach der Undationstheorie. Ibid., pp. 730—739.

——, 1933c. Versuch einer geotektonischen Analyse Australiens und des Südwestpazifik nach der Undationstheorie. Ibid., pp 740—749.

——, 1933d. Die neogene Struktur des Malayischen Archipels nach der Undationstheorie. Ibid., pp. 887—897.

——, 1948. Cosmogony and geochemistry. C. R. 18th Int. geol. Congr. London, pt. II, p. 9—21.

——, 1952. The endogenic energy of the Earth. Am. J. Sci., v. 250, p. 104—117.

BURGERS, J. M., 1935. First report on viscosity and plasticity. Kon. Ac. v. Wetensch., Verhandel.
BIJLAARD, P. P., 1938. A theory of plastic buckling with its application to geophysics. Proc. Kon. Ac. v. Wet., v. 41, pp. 468—480.
CADISCH, J., 1953. Geologie der Schweizer Alpen. Basel.
EWING, M., 1954. Gravity anomalies and structure of the West Indies. I. Bull. Geol. Soc. Am., v. 65, pp. 165—174.
GLANGEAUD, L., 1947. Orogénèse et pétrogénèse profonde, d'après les théories géophysiques nouvelles. Révue Sci., t. 85, pp. 1107—1120.
——, 1948. Thermodynamique de la pétrogénèse profonde. Rep. 18th Int. geol. Congr. London, pt. III, pp. 56—64.
GRIGGS, D., 1939. A theory of mountain building. Am. J. Sci., v. 237, pp. 611—650.
HESS, H. H., 1948. Major structural features of the western N. Pacific. Bull. Geol. Soc. Am., v. 59, pp. 417—446.
JEFFREYS, H., 1952. The Earth. 3rd ed.
KIENOW, S., 1942. Grundzüge einer Theorie der Faltungs- und Schieferungsvorgänge. Berlin.
KUENEN, PH. H. & DE SITTER, L. U., 1938. Experimental investigation into the mechanism of folding. Leidsche Geol. Med., v. 10, p. 217—240.
NABHOLZ, W., 1945. Geologie der Bündnerschiefergebirge zwischen Rheinwald, Valser und Safrental. Ecl. geol. Helv., v. 38, pp. 1—119.
SITTER, L. U. DE, 1939. The principle of concentric folding and the dependence of tectonical structure on original sedimentary structure. Proc. Kon. Akad. v. Wet., vol. 42, pp. 412—430.
——, 1947. Antithesis Alps-Dinarides. Geol. & Mijnbouw (nw. ser.), v. 9, pp. 1—13.
——, 1952a. Pliocene uplift of Tertiary mountain chains. Am. J. Sc., v. 250, pp. 297—307.
——, 1952b. Laboratory experiments on strain of rock specimen. Geol. & Mijnbouw (nw. ser.), v. 14, pp. 8—18.
SMOLUCHOWSKI, M. VON, 1909. Uber ein gewisser Stabilitätsproblem der Elastizitätslehre und dessen Beziehung zur Entstehung von Faltengebirgen. Anz. Ak. Wiss. Krakau. Math. Naturw. Kl.
STAUB, R., 1924. Der Bau der Alpen. Beitr. geol. Krt. d. Schw., N. F., Nr. 52.
TERCIER, J., 1952. Problèmes de sédimentation et de tectonique dans les Préalpes. Rev. Quest. scient.
VENING MEINESZ, F. A., 1934. Gravity expeditions at sea. Neth. Geod. Comm., v. 2. Delft.
——, 1950. Earth's crust deformation in geosynclines. Proc. Kon. Ak. v. Wetensch., v. 53, pp. 27—46.
WENK, E., 1953. Principelles zur geologisch-tektonischen Gliederung des Penninikums im zentralen Tessin. Ecl. geol. Helv., v. 46, pp. 9—22.