THE NEGATIVE ISOSTATIC ANOMALIES IN THE EAST INDIES (WITH EXPERIMENTS)

BY

PH. H. KUENEN

> „The field of tectonics is no place for a prim individual who likes everything orderly and settled and has a horror of loose ends.''
>
> LONGWELL

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I. INTRODUCTION.

During his gravity expeditions at sea VENING MEINESZ discovered large anomalies in the force of gravity in the Netherlands East Indies. The results of his expeditions are described in two volumes (bibl. 17) together with articles by UMBGROVE on the relations to geology and by KUENEN on the relations to morphology.

VENING MEINESZ showed that the more important anomalies can be divided into two different groups. There are extensive fields of positive anomalies and a narrow, curved strip characterised by negative anomalies in the East Indies.

To explain the latter VENING MEINESZ assumes that the earth's crust has been subjected to horizontal compressive stress that caused the crust to buckle downwards. In this manner the lighter sial was forced down into the heavier sima, thus causing a deficiency of mass and consequently a negative isostatic anomaly. At the same time the upper layers of the crust were squeezed together and produced a tectonic structure.

UMBGROVE was able to show that the same belt that is remarkable for the negative anomalies, is also characterised by far more intensive tectonic disturbances than other parts of the archipelago. He thus added considerable support to the theory that crustal buckling is the cause of the deficient gravity. For a full account of the facts and the detailed argumentation the reader is referred to the original publication.

On entirely different grounds BUCHER comes to a similar conclusion concerning the deformations in an orogenetic belt (bibl. 1).

In the following article an attempt is described to reproduce artificially the conditions postulated by VENING MEINESZ in order to study the forms that result under lateral compression.

The correctness of the theory given by VENING MEINESZ can hardly be proved or either disproved by an experiment. There are so many factors unknown that may be of essential importance and so many others that cannot be reproduced in the laboratory that anything in the nature of proof is still far beyond the reach of the experimenter. But in the latter respect the geophysicist is in no better position.

Knowledge of the physics of the earth is still in the stage of tentative explanations of observed facts. Although such explanations will have to be discarded or altered as more facts become available, they have their value in lending meaning to the new data and in stimulating research.

As to the value of experiments the author is of opinion that when the experimental conditions are properly chosen they are of considerable value for checking the theoretical deductions and guiding further research in many fields of geology and geophysics. Thus in the following I hope to be able to show from my experiments: the probability of the main deductions of VENING MEINESZ as to the downward movement of a buckling layer floating on a heavier substratum, further that if this layer is not highly plastic compaired to its own thickness quite unnatural forms are produced, that the downward bulge of the crust may assume various forms or be replaced by a different mechanism altogether, that an orogenetic belt need not be more plastic than its surroundings, that the geosyncline need not be straight, or its direction at right angles to the compressive stress, that the contents of the geosyncline must have less aggregate strength than a layer of the crust of the same thickness, and finally that the negative anomaly may at the present time be caused by phenomena that are mainly restricted to the crust itself without a conspicuous bulge at the lower surface.

Even if these experiments do not definitely prove anything, they undoubtly do help to clear the field of speculation.

The author discussed the penomena related to negative anomalies in the East Indies in the geophysical section of the Geologisch Mijnbouwkundig Genootschap voor Nederland en Koloniën. He wishes to thank the members for their helpful criticism of his views. On the last pages of this article a few names will be mentioned of persons who made specially relevant remarks. The author is also indebted to Dr. P. TERPSTRA for valuable suggestions concerning the technique of the experiments, and to Professor VENING MEINESZ for helpful criticism and encouragement.

Shortly after the author had begun the experimental investigations, it appeared that Professor Dr. B. G. ESCHER (Leyden University) was also studying the buckling of the crust by experiments of the same nature. Professor ESCHER decided to suspend his investigations until the writer had published his results, in order to avoid double work.

It is with sincere gratitude that the author wishes to point out that he gained priority merely through this sacrifice by his former tutor.

II. EXPERIMENTS. [1]

The main assumption of VENING MEINESZ is that the earth's crust is formed by a comparatively rigid layer (with residual strength) of light materials floating on a denser substratum that is relatively plastic

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[1] The author will gladly supply prints or lantern slides of the photographs in this article at costing price.

and that this crust is subjected to horizontal compressive stress. The simplicity of this picture opens the possibility of imitation in the laboratory.

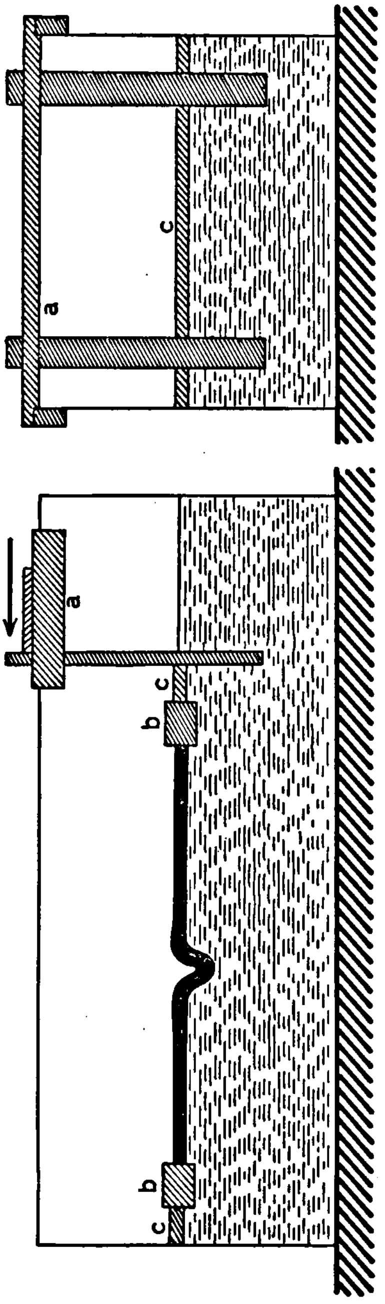

Fig. 1.

Side view and cross-section of apparatus.

a — pushing mechanism, b — floating beams, c — floating planks.

Three different types of experiments were made. In the first series a layer of paraffin, floating on warm water was used, a method already employed by Summers for tectonic experiments (bibl. 16). In the second series the crust was made of a mixture of paraffin, vaseline and mineral oil. In the third series a crust of the same materials was made on a substratum of like substance but of slightly greater plasticity in a specially constructed pressure box.

First series.

The apparatus was extremely simple (fig. 1), consisting of a large aquarium (1 m long, 50 cm broad) half filled with water, that was heated to a temperature just below the melting point of the crustal materials. The paraffin layer was cooler on top than at the bottom, but this effect could be minimized, if needed, by covering up the aquarium for some hours before the experiment took place. In this case the difference in temperature is reduced to a few degrees. A few experiments were also carried out with a plastic mixture, floating on water of room temperature, sothat the model crust was of the same consistency throughout.

Pressure was applied in the following manner. A plank rested across the top of the aquarium with runners along the edges so that it could be pushed forwards and backwards by hand, both ends always moving the same amount. Two beams were fixed on the side of the plank, running down vertically into the water. These beams exerted the pressure against a loose plank floating in the water in the aquarium. The plank in turn pushed up against a beam also floating in the water. Sometimes there were also a plank and beam at the opposite side.

A layer of melted paraffin was poured onto the water on the opposite side of the floating beam to that of the pushing apparatus.

In order to minimize the influence of the friction against the glass sides of the aquarium, the layer of paraffin had to be loosened after it had congealed. This could be effected by carefully pressing down the edges a few mm. (In the second series the crust was too soft to be treated in this manner and the loosening was obtained by slightly lowering the water level.)

Several dozens of experiments were made, that are classified and described below in convenient groups.

Experiments I A (fig. 2 and 3).

The crust was made of paraffin (melting point 43—45° C.), the temperature of the water was 39—42° C. The thickness of the crust was 3—12 mm. The average firmness is then comparable to butter at 20—25° C., but of different consistency; the material gives way quickly under a pressure of about 5 gr/cm^2.

When pressure is applied the crust sometimes begins to warp down beside the beam and is buckled into a synclinal fold, that is gradually closed. A second synclinal fold begins to form beside the first and while it is closing a third one begins and so on. All folds are of the same amplitude and form one after the other until the whole layer is crumpled up into symmetrical folds. Once formed a fold does not develop any further, but remains as it was.

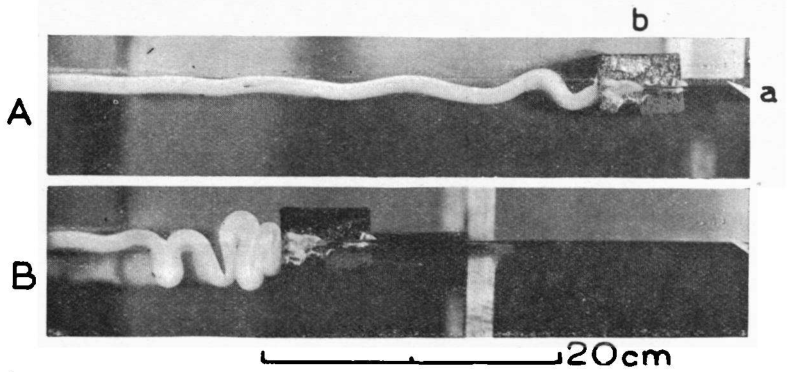

Fig. 2.

Experiments IA. Model crust of paraffin, compressed from the right.
a — fixed point showing original breadth of crust (= 45 cm), b — floating beam.
A syncline and anticline have formed beside b in stage A;
in stage B 4 synclines and anticlines have formed side by side.

If the experiment is repeated with a thicker layer the same result is obtained with folds that are correspondingly larger. When paraffin with a higher melting point (54°—60° C) is used at the same temperature and therefore less plastic, there is no essential difference in the result.

The first warping of the crust beside the beam is caused by slight tilting of the latter. If this is avoided and the crust is sufficiently homogeneous, it will be thrown into a series of symmetrical anticlines and synclines over its whole breadth. One of these synclines will soon gain a slight advantage. At that moment it constitutes the weakest part of the crust, for the greater the curvature of a part of the floating layer, the less it can resist compressive stress. It will then suddenly buckle downwards, while the other folds flatten out considerably. The deformation of the crust was partly plastic and partly elastic. The elastic strain of the other folds is released when the syncline buckles

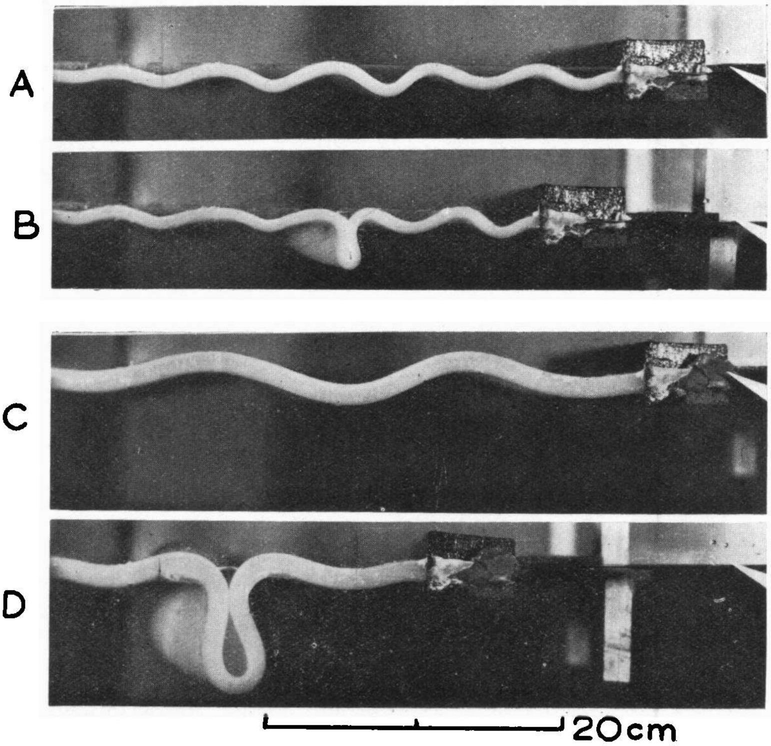

Fig. 3.

Experiments IA. Stage A (crust 5½ mm): a number of waves. Stage B: the deepest syncline has buckled downwards, some of the others have flattened out slightly. Stage C (crust 12 mm): The buckling of a thicker layer. In stage D the syncline has filled with water, before the photograph was taken. In both cases, however, the water did not enter the syncline until the experiment was finished.

downwards and only their plastic deformation remains (see fig. 3, A, B).

This process is sometimes, *but not always,* led up to by water running into one of the synclines from the side. This presses down that particular syncline and helps to start its exceptional development.

Repetition of this experiment shows, that it may also be one of the anticlines that first shows a greater amplitude than the other anti- and synclines. In this case, however, the development does not continue until a closed anticline is formed, as was the case with a syncline if it once gained an advantage[1]). On the contrary one of the two neighbouring synclines soon catches up and then forms a closed syncline. The final result in both cases is therefore a number of symmetrical waves and one deep, closed syncline like those of fig. 3 B and D. When a soft, plastic mixture is used (such as that of the second series) on water that is of the same temperature as the room, the results do not differ from those just described. Care must be taken to use a mixture that is not too brittle, as the crust will otherwise snap through before the deformation is fully developed.

Experiments I B (fig. 4).

It might be supposed that localisation of the folds on the side where pressure is applied, as in fig. 2, is caused by friction against the glass.

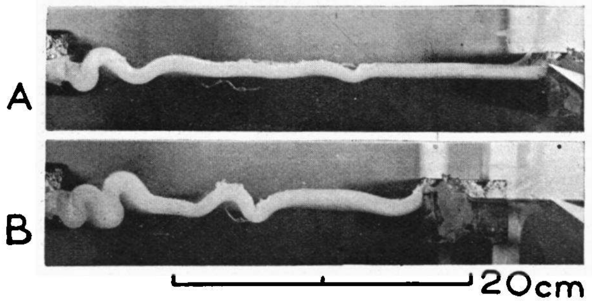

Fig. 4.

Experiment IB. Model crust first compressed from the left until crumpling began, stage A. Compression continued from the right, the crumpling goes on at the left side, stage B.

To test this the experiment was repeated, but after the first folds began to form the pressure was then applied from the opposite side. It is of importance to note that in this manner the result is similar to that of the first experiment. The folds continue to form on the side where the plication first started, although the pressure is now exerted from the opposite side of the layer.

Experiment I C (fig. 5).

Across the paraffin layer is placed a narrow strip of either clay, a rounded stick, sand, paraffin or water, half way between the pressure

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[1]) Compare with experiments ID.

side and the resistance end. This results in a slight bending down of the crust comparable to a geosyncline.

On application of pressure the syncline begins to bend down until it is closed by the two sides coming together. But while in the first experiments the syncline if it first formed beside the beam, then ceased to function, its life is prolonged after closing when it is made to start in the middle of the layer. It continues to press down deeper and deeper into the water developing with rolling hinges, that is to say the horizontal portions of the crust at opposite sides of the syncline approach and then bend down into the limbs of the syncline. When operated with sufficient speed there is practically no limit to the size to which the single syncline will develop without anticlines or new synclines forming on either side. But as the paraffin is being forced down against its floating power, it will bend to one side and rise up to a horizontal position below the crust if given sufficient time. This does not take

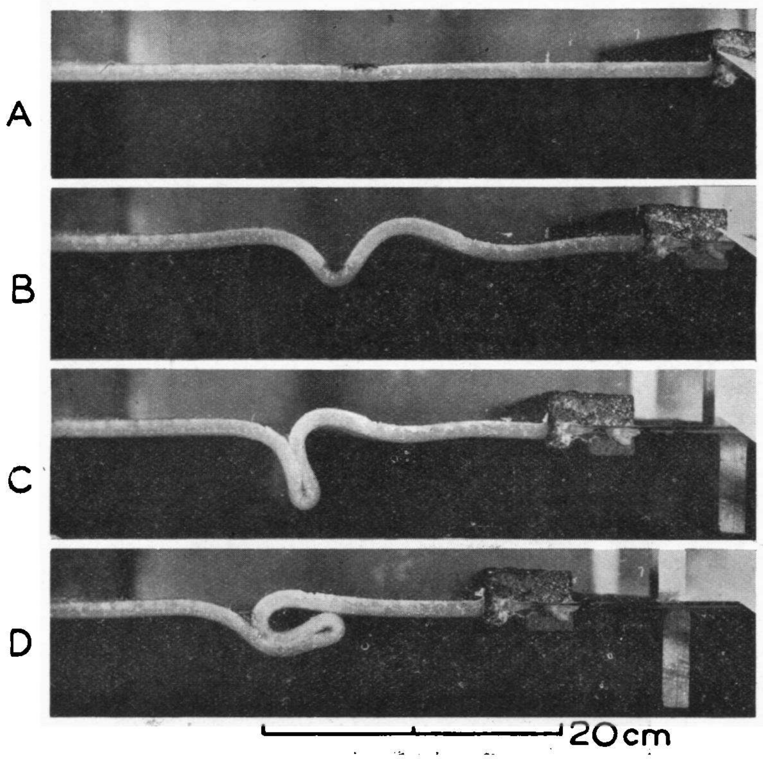


Fig. 5.

Experiment Ic. The model crust in stage A is weighted with a strip of dark paraffin. A geosyncline forms in stage B. In stage C it is closed. In stage D it has grown and floated up, proving the formation of a negative anomaly by the compressive stress. Note the depth of the topographic depression, that it several times the thickness of the crust.

place, however, until the syncline is several times as deep as the thickness of the crust.

Experiments I D (fig. 6).

Under the crust is placed a small rounded stick of wood, across its whole breadth. The rod lifts the crust into a slight bulge. In our experiment the crust was 9 mm thick, the bulge was one mm high and 4 to 5 cm broad. When pressure is applied an anticline begins to form, but ere long a syncline is developed at both sides of the anticline. These depressions soon grow to the size of or larger than the bulge. The latter therefore starts to sink below the level of the crust and ceases to grow. The synclines on the other hand continue development, although they are each fed from one side only. The sinking of the bulge causes the synclines to heal over towards it, pressing it down all the more until it sinks right down and the complex syncline closes over the top of it. In this manner the curious result is obtained that the lifting of the

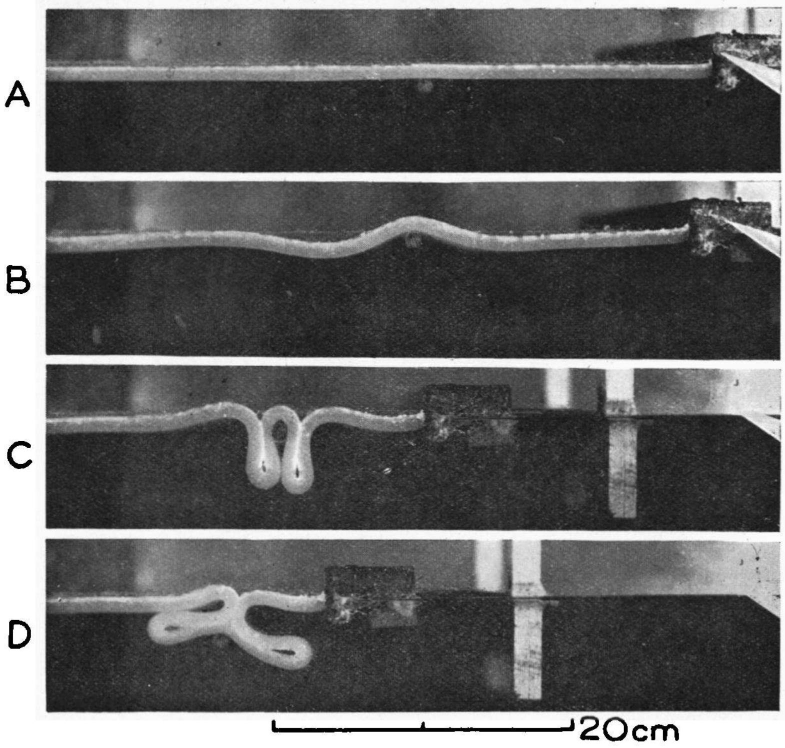


Fig. 6.

Experiments ID. A rounded stick is placed below the crust. Compression causes the crust to rise above the stick, stage B. In stage C the synclines at both sides have developed. In stage D the initial bulge has been forced down below the level of the crust.

crust causes this part to disappear downwards if horizontal compression is prolonged sufficiently.

Second series.

For these experiments the same apparatus was used, but the crust was considerably more plastic. A mixture of paraffin, vaseline and mineral oil was used, coloured by red paint in solution or white paint in suspension. The temperatures ranged from just below the melting point to several degrees lower. Mixtures of these substances have the property of remaining highly plastic far below the melting point. The melting point (± 40° C) is hardly below that of the pure paraffin, but we can easily make a mixture that is more plastic than butter, both taken at room temperature. In the mixture I used, vaseline and oil predominated, sothat at the temperatures used (35—38° C) the crust was highly plastic. (Oil — vaseline — paraffin = 4:4:1; and other similar mixtures, but as the consistency appears to change after repeated melting no exact ratios can be given). It is difficult to give an exact measure of the plasticity, partly because there is a difference between the upper and lower surface. When scooped into a tin it trickled through a hole with a diameter of 3 mm under a pressure of 10—20 g/cm^2. Under 5—10 g/cm^2 it dripped out in oblong, spindle-shaped drops. With a pressure of 3—4 g/cm^2 it still dripped out, but very slowly. It was unable to sustain a slope of 10° in a dish without flowing out immediately. This consistence is comparable to that of soft jam. The pressure at which plastic deformation sets in is of the order of ¼—1 gr/cm^2. Within the range of time of the experiments it did not behave like a liquid, however, as an appreciable force was needed before flow started. For instance blodges would float about in the aquarium for days, without spreading out into a thin layer. As long at any rate as the experiment is not prolonged for weeks or months the materials behave as a highly plastic solid (= with a small residual strength), but not as a viscous liquid.

The use of a mixture is of importance, for the range in temperature in the model crust is sufficient to render pure paraffin too firm at the surface to obtain a truly high plasticity of the crust throughout. A crust of paraffin melts at the lower surface, before the upper surface is very soft.

Natural rocks have at least one important property that is not found in the mixture, namely that they will rupture under tensional stress. As, however, at least 9 tenths of the earth's crust is under such great pressure, that it will not rupture, but flow under tensional stress, the difference in behaviour in this respect need not be considered of material importance in our problem. The author intends, however, to perform some experiments with clay floating on a heavy liquid to see whether the use of other materials introduces any new points of view.

As stated above, the congealed layer had to be loosened from the panes of the aquarium by dropping the level of the water. As the friction against the glass was still noticeable, most of these experiments were carried out by pushing from both ends alternately.

Experiments II A (fig. 7 and 8).

On a layer of 1 or 2 cm of our mixture a thin and narrow strip of the same composition or slightly more plastic is carried straight across the aquarium. Pressure being applied the same phenomena are obtained as in the first series, but with several important modifications. As long as we do not push too quickly the topographic depression at the surface remains quite small throughout. So small in fact that no water flows into it even if the layer does not touch the sides of the aquarium; in other words the geosyncline remains almost as shallow as the amount the surface of the crust floats above the waterlevel.

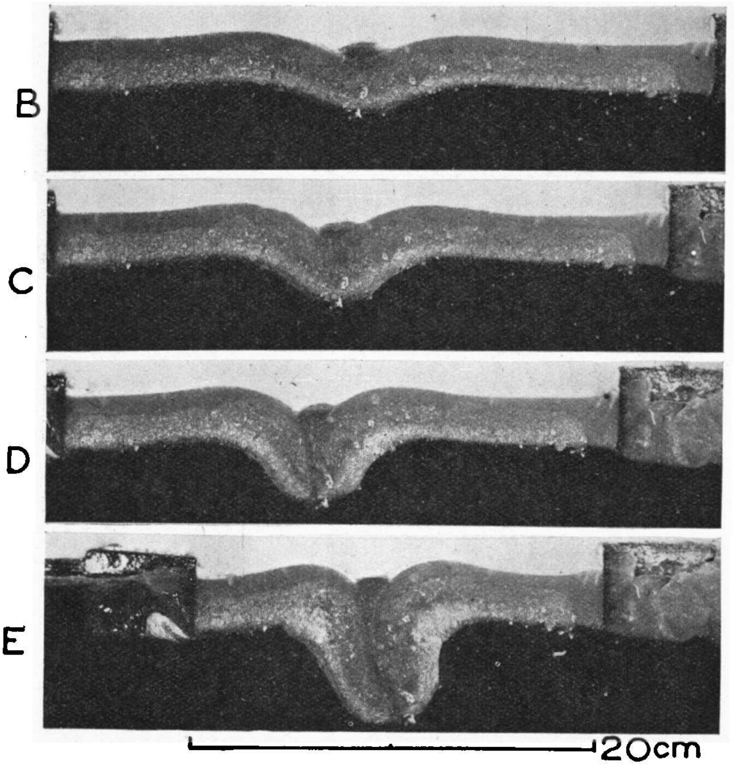


Fig. 7.

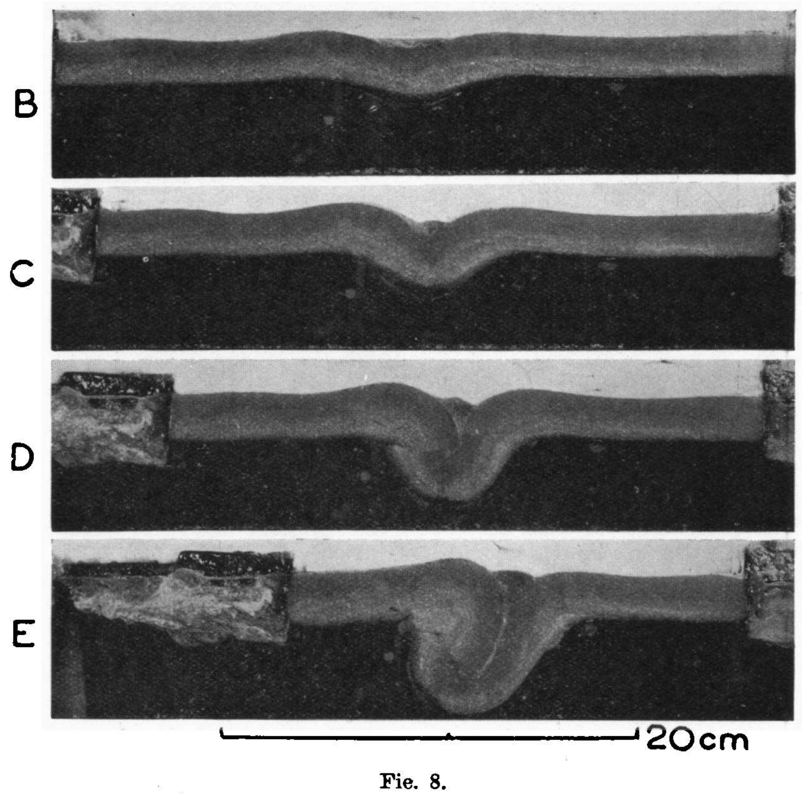
Experiments IIA. Successive stages in the growth of a synclinal fold in a highly plastic crust, that was slightly bent into a geosyncline by a strip of soft materials. Stage B after slight compression. Compare the relatively small depth of the topographic depression with that forming when the crust has more strength (fig. 5).

Another difference is that the lower and hotter and therefore most plastic part of the crust shows a tendency to flow up along the limbs of the syncline. The higher the temperature, the more pronounced is this tendency.

The next difference is that there appears to be a limit to the growth of the syncline. The crust at last becomes so thick and floats so high, that development ceases and a new syncline forms beside it. This tendency is most marked if pressure is renewed after a few minutes of rest.

Taken as a whole the deformation of the crust comes closer to a mere plastic thickening, than is the case with a more rigid layer. Nevertheless there remains a decided downward warping; a definite synclinal development.

It is a surprising experience, after watching the crust warp and bend down and continue to move in a growing syncline with rolling hinges, to find, on attempting to lift the layer out of the aquarium, that it is so plastic as to run out between the fingers in a trickle.



Fie. 8.

Experiments IIA. Conditions as in fig. 7. Stage B after slight compression. In stage D a thrust has formed in the root, but stage E shows that it has not developed further.

As long as we use a single layer of the same plasticity throughout (but slightly harder at the surface in consequence of the lower temperature) all the materials that are forced into the syncline move downwards. After some crumpling and narrowing of the strip that was placed above the developing syncline, it is usually seen to disappear down into the core of the syncline. There is no tendency for the formation of a bulge or anticline above the syncline by the superficial parts.

This experiment has more chance of succeeding if we apply pressure soon after dripping on the hot materials to form the geosyncline. The crust is then more plastic in consequence of the slight heating. This helps to start the warping in the right place. If one waits until the geosyncline has cooled, it not infrequently happens, that the crust deforms beside the beam first and continues to warp or thicken here. Unnatural complications arise and the experiment has to be done all over again. If the strip is built of a more plastic mixture the deformation generally starts below it, even when this part of the crust is no hotter than the rest.

The deformation does not always take the shape of a synclinal fold. Sometimes the crust breaks soon after or even before the warping has begun and the moving half is forced over or under the stationary part. An underthrust or overthrust of the whole crust is formed (see fig. 8 and 12). The hotter and more plastic the model crust is, the more apt it is to show this phenomenon.

Experiments II B (fig. 9).

When pressure is applied from one end only and without loosening the crust from the sides of the aquarium, the crustal fold soon takes on a bent course, concave towards the side from which we push.

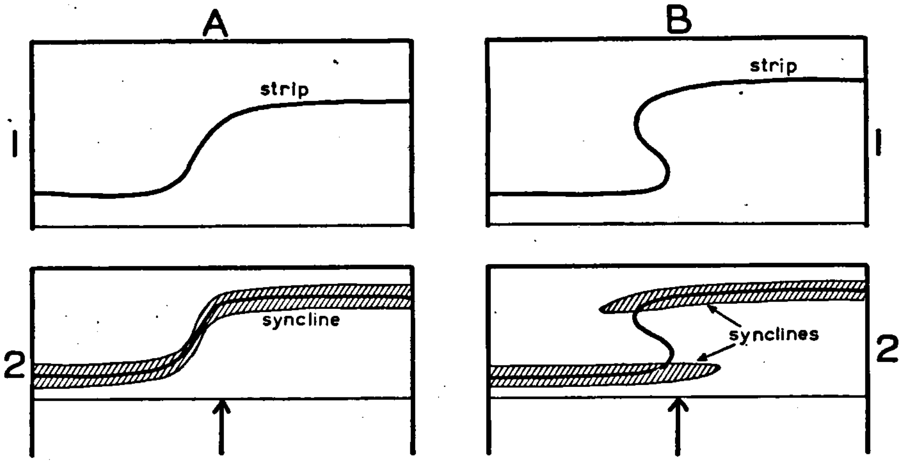


Fig. 9.

Experiments IIB. Ground-plan of the formation in two stages (1 and 2) of a crustal fold. In A the geosyncline (= strip) is bent; the size of the syncline (and root) is proportionate to the angle between the syncline and the direction of the stress. In B the geosyncline (= strip) doubles back; two synclines and roots are formed en-échelon.

A less complicated case is produced when we run the strip of overburden in a curve across the crust, after the latter has been loosened.

A synclinal fold is formed in both cases resembling that of the experiment II A, but the smaller the angle between the strike of a portion of the syncline and the direction of the pressure, the smaller the size of that portion of the syncline is. If the loop of the belt is soo marked as to run back again partly, two synclines are formed en-échelon (fig. 9, B).

Now and again, however, if we repeat this experiment the central part of the belt overruns the foreland, without the production of a synclinal bend. The moving half of the crust is pushed bodily over the fore-lying part of the unmoved half of the crust.

Viewed from above the difference with the normal case is not directly apparent, but closer observation reveals a difference. In the former experiments we always saw the superficial materials moving relatively from both sides towards the seam above the syncline. Now we observe that the relative movement is restricted to the underthrusting part. The plasticity of the materials prohibits the production of a wedge-shaped topographic syncline.

Experiment II C.

When two strips are placed at a distance from eachother that is many times the thickness of the crust, one will develop sooner and more fully than the other.

Experiment II D (fig. 10).

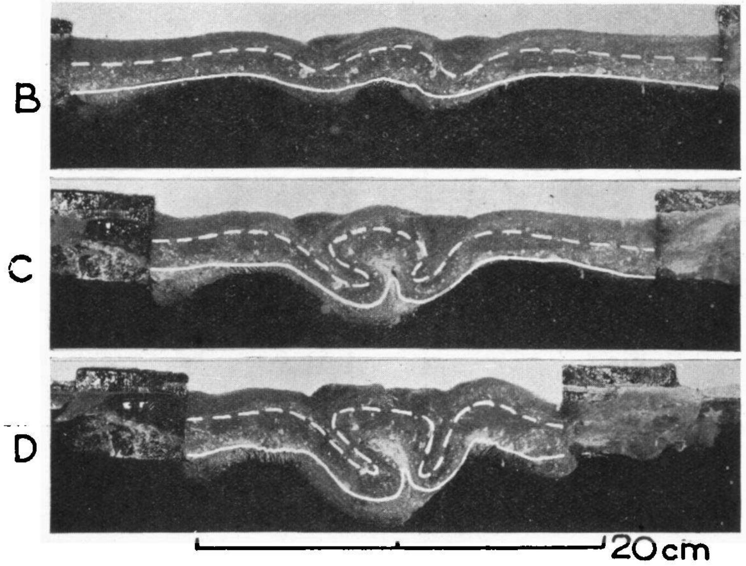


Fig. 10.

Experiments IID. Two geosynclines are placed close together (3 times the thickness of the crust from centre to centre). Stage B after slight compression. (White line drawn in to show lower edge of the crust on the section against the side of the aquarium, the broken white line drawn in to show the deformations). In stage C the two roots have met and movement in the central part begins to die out. To stage D the movement consists principally of underthrusting from the two sides below the „dead'' central belt.

When two strips are brought onto the crust a short distance apart a new and interesting phenomenon may be observed. At an early stage the two synclines begin to interact. The belt of the crust between them is too narrow to yield material for the two adjoining limbs of the synclines. Movement dies out in this part and henceforth a single tectogene develops by movement at the opposite sides only. The crust is pushed under the central „dead'' belt from two sides to meet at the bottom. A picture is obtained that resembles the two-sided orogene of Kober in many respects.

It appears highly probable, that with this mechanism there will frequently occur complications in the lower regions of the tectogene.

Experiments II E (fig. 11—15).

In these experiments attempts were made to produce a series of folds or overthrusts in the upper part of the crust, while the lower main part was forced into a synclinal fold. Up to now all the materials, even the strips of overburden, had moved down into the syncline, either directly or afterwards, before any clear imitation of a mountain range had formed. Many of the results already described were obtained during experiments that were originally intended for this group.

When a more rigid layer is used as overburden and it is brought onto a lubricating film of oil, it does not bend down into the syncline at once, but overthrusts onto one of the two sides of the syncline. Shortly after, however, it is carried down into the welt or a new syncline originates elsewhere. As a stronger and more brittle consistency for the contents of a geosyncline, as compared to the remainder of the crust, appears very unnatural, the experiments along these lines were not carried on further.

Satisfactory results were obtained when a geosyncline and upper crust were made, of greater plasticity than the crust. The lower crust buckled down and formed a closed syncline, leaving no room for the softer surface layers. Consequently these folded and were thrust into a slight, rimpled bulge, the actual tectogene. As it is almost impossible to form very thin stratified layers right up to the glass of the aquarium and as friction causes distortions, the author was not able to produce a closer resemblance to natural conditions than shown in the photographs. Better results are obtained, however, by cutting the crust some distance from the edge (fig. 15). The reader can easily imagine an alpine section in the darker geosynclinal part of the sections and thus mentally complete the picture.

Third series.

These experiments and the complicated apparatus will not be fully described as they were not made with a view to illustrate the theory of Vening Meinesz. It is sufficient to mention that a crust that was only slightly more rigid than the substratum reacted to horizontal compression in the same manner as in the experiments of series I, if it was given a slight initial dent across the surface.

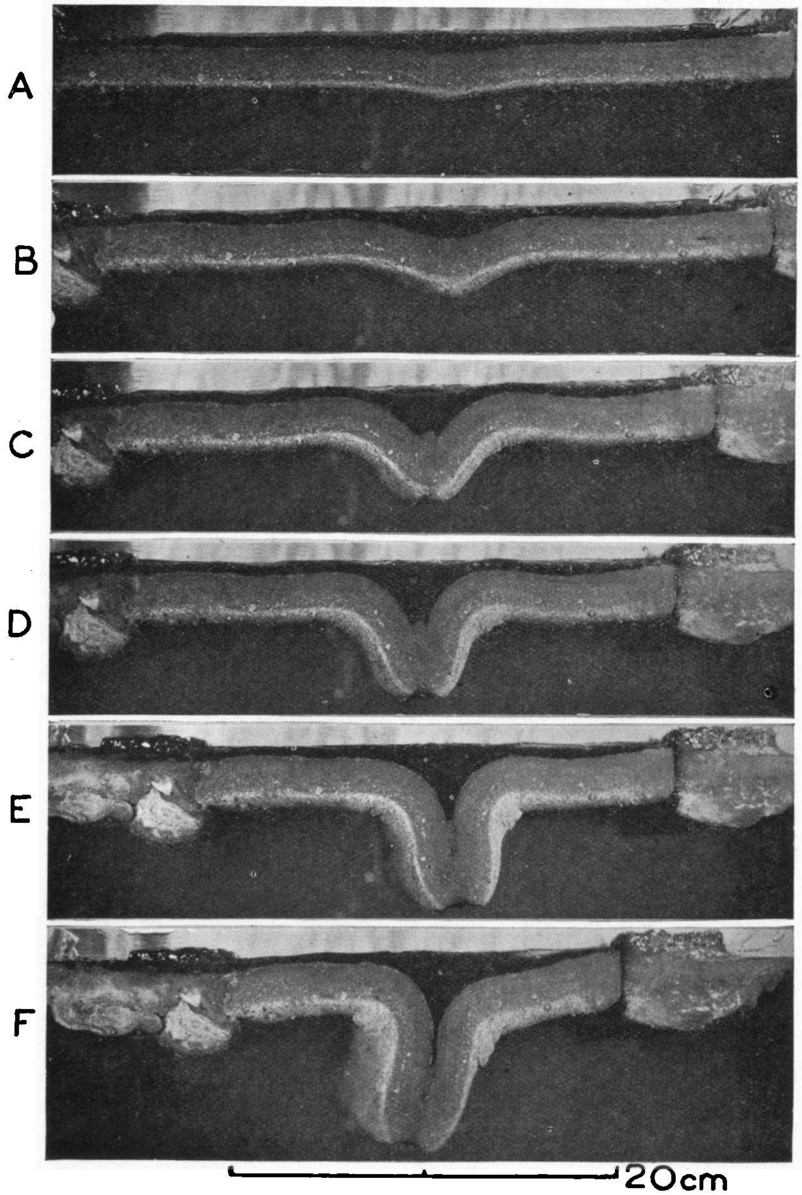


Fig. 11.

Experiments IIE. Successive stages in the compression of a crust that buckles down below the slight initial geosyncline of stage A. The very soft, dark, upper layer is squeezed out of the centre of the root. The slight irregularity at the tip of the root only occurred up against the glass of the aquarium.

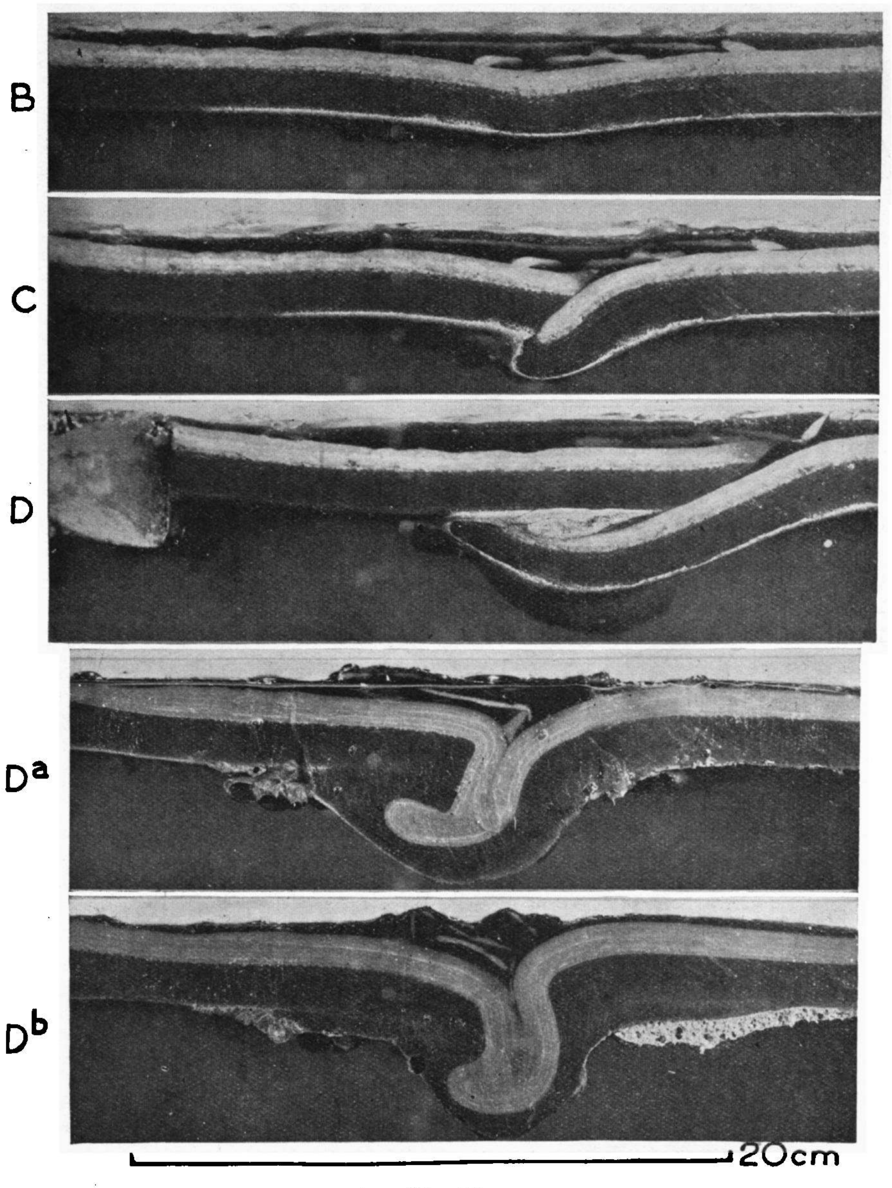


Fig. 12.

Experiments IIE. Stages B, C and D. Successive stages in the compression of a crust consisting of two layers of the same composition, covered by much softer layers. In the geosyncline two light layers were introduced that have greater strength than the intervening (black) strata of the covering. Against the glass the crust has broken and thrust. In stage D it may be seen that a slight distance from the glass a deeper root has formed. In D a and D b two sections are shown, taken at 5 and 35 cm from the glass. Note: a, the gradual change from thrust to an asymmetrical and almost symmetrical fold; b, the disappearance of the contents of the geosyncline out of the root; c, the spreading of the root along the lower surface of the crust, because the lower layer was almost molten; d, the surface of the water in which the crust floats in D a, visible as a thin white line.

N.B. The light part of the crust was squirted onto the lower in thin layers; froth had formed below the latter when it was poured onto the water.

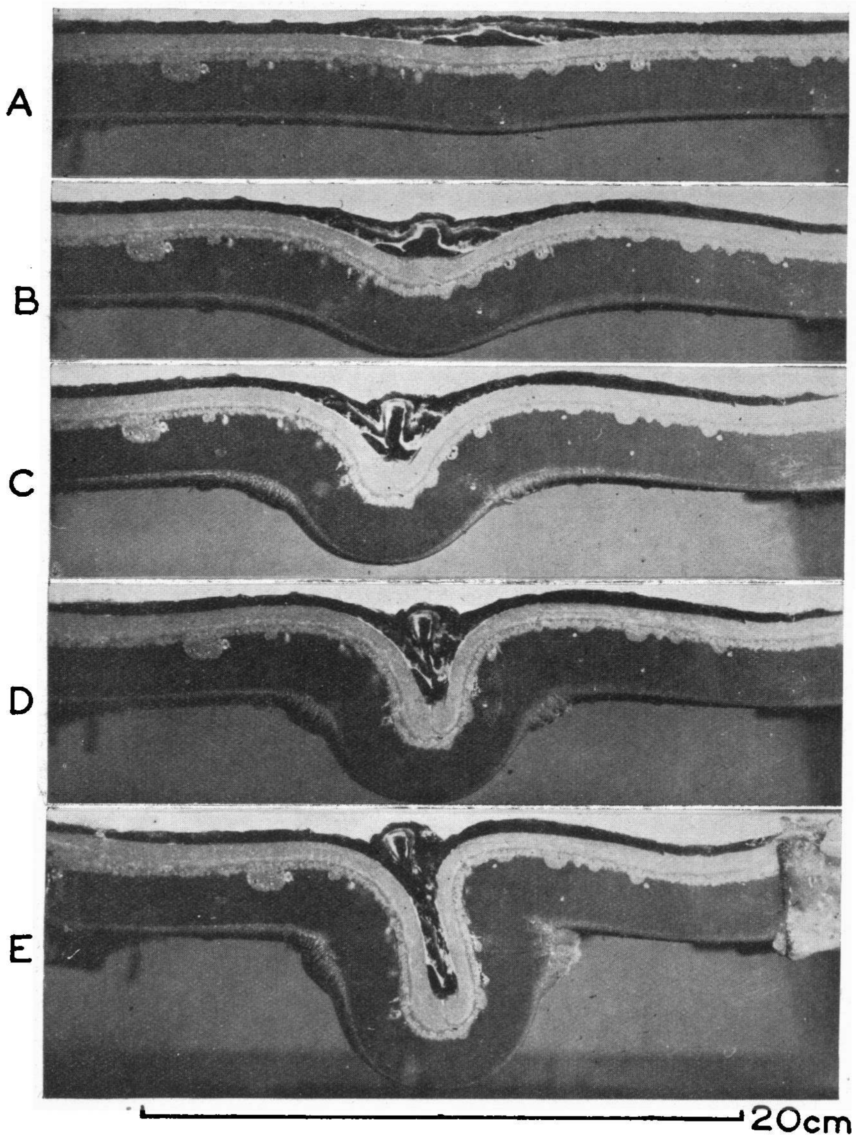


Fig. 13.

Experiments IIE. Successive stages in the compression of a crust. Conditions as in fig. 12, but contents of the geosyncline only slightly less strong than the crust. Note: a, the rolling hinges of the synclinal fold; b, the remains of the upper strata in the core of the root; c, very slight spreading of the root, as the lower limit of the crust was not quite so hot as in fig. 12; d, that folding in the geosyncline starts early (stage B) and is gradual. This also applies to the formation of the root. The same is seen in fig. 14.

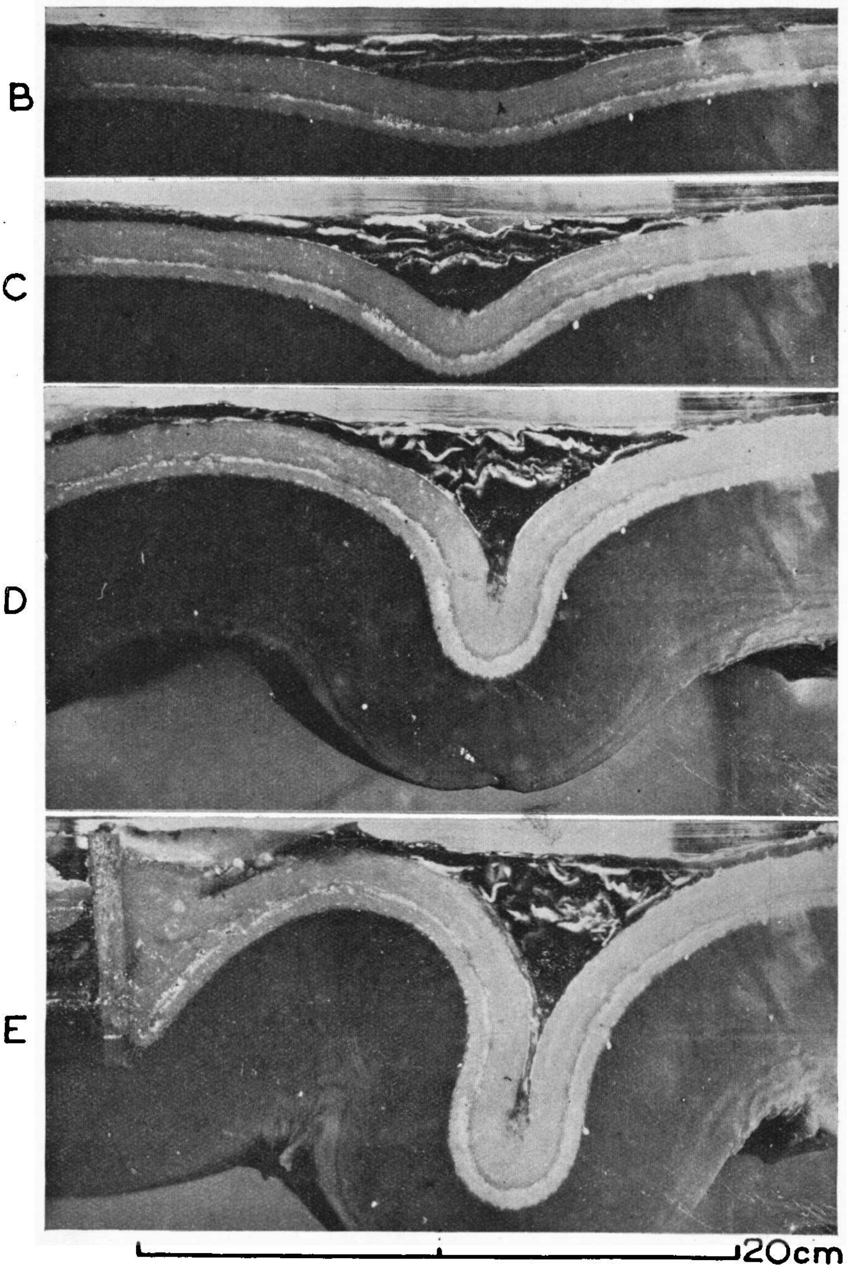


Fig. 14.

Experiments IIE. Successive stages in the compression of a crust. Conditions similar to those of fig. 12, but with 4 harder white layers in the geosyncline, sprayed onto the crust. Irregularities against the beam (stage E, on the left), and slight distortion against the glass (see fig. 15). From both sides the superficial strata are continually forced under the edges of the geosyncline, especially in the later stages.

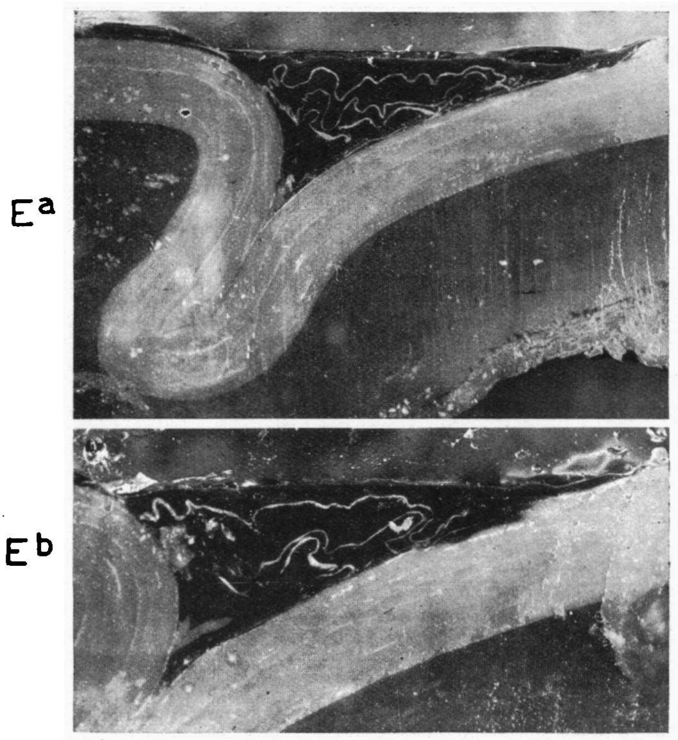


Fig. 15.

Sections through the crust of fig. 14 (stage E) showing the folding of the harder, white layers in the geosyncline. The root is slightly asymmetrical in consequence of the slipping of the lower part of the crust below the beam on the left of fig. 14, stage E.

III. CONCLUSIONS FROM THE EXPERIMENTS.

First series.

These experiments are not of much *direct* value to illustrate the theory of Vening Meinesz, as the model crust was far too strong in comparison to its thickness. The dimensions are on a scale of about 1:5.000.000. The average strength of the materials of the earth's crust might be assumed to be such that they yield under a pressure of at most 2000 kg/cm². This amount would correspond to about ½ g/cm², on the scale of our experiments. Our crust is therefore about 10 times too strong at least and this amount is increased when we take into account that the specific gravity of natural rocks is 3 times that of paraffin. The degree of plastic reaction is therefore far too small.

The consequence of too great a strength of the model crust is that the fold develops in an unnatural manner, that is in some respects different to what happens to the earth's crust under compressional stress. We saw, that the syncline is deeper during a certain stage of its development, than the thickness of the crust. Translated to the earth, this would mean that the topographic depression at the surface would be of the order of 50—100 km at the very least. The anomaly of the gravity would be of the order of 5000 milligals.

Without the results of the second series of experiments, in which the strength is of the correct order of magnitude, it would even be open to doubt whether the first series was of any value at all. As, however, the two series give similar results in many respects, we may draw from the first series also a few tentative conclusions that appear more clearly than when the highly plastic crust is studied.

The stress in the crust is transmitted to the weakest spot, where warping sets in. A very slight bending up or down of the crust is sufficient to convert it into the weakest spot. This is either effected by the beam, an overburden or chance when several waves form.

It might appear to be more „natural'' to leave the first bending to the influence of the beam. But this is not the case. The beam, although floating, is not comparable to anything in nature and if a piston were used the artificial character would be even more pronounced. In practically all theories dealing with tectonics of the crust either a mass-force is assumed (tidal force, „Polflucht'', shrinkage of the substratum, etc.) or un undertow of subcrustal materials. In other words our method of applying a force by pushing against the model crust is in all likelyhood incorrect. As soon as the movements are concentrated in a narrow belt, however, this difference is eliminated. It is therefore pure self-deceit to believe it is more natural to allow the initial warping to begin where it likes. The crust of the earth is not homogeneous and sedimentation, denudation, intrusion etc. cause deformations. The orogenetic phase follows on the formation of a geosyncline: the crust in nature, just as in the experiments, is warped down and covered by a thick coating of sediments before it is buckled by the compressive stress.

Vening Meinesz pointed out that the crust is elastically compressed by the stress and becomes denser. This must aid the tendency for the initial deformation to be directed downwards. This phenomenon is not reproduced in the experiments. Our materials would have to show a cubic compressibility 5.000.000 times that of rocks, instead of being of the same order of magnitude.

It is therefore closer to the natural conditions to cause an artificial downward bulge in our model crust before compression ¹), than to allow the deformation to initiate over the whole length or beside a hard beam or piston. In the latter case the inability of the pushing mechanism to partake in the deformation of the crust, causes the first fold to cease development at an early stage. When the deformation is forced to begin in a narrow strip away from the ends of the crust, one single

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¹) This does not apply to the first formation of waves of the type of fig. 3; that is the initiation of a geosyncline.

fold is formed in a downward direction. Even when the crust is originally bulged upwards by a float, it will ultimately show a downward tendency into a complicated synclinal shape.

The consequence of pressing against a floating and plastic crust with a slight geosyncline is therefore the formation of a negative anomaly. That part of the compressional stress is converted into a negative anomaly, follows also from the floating upwards of the syncline during or after cessation of the application of stress. It is not possible to form a positive anomaly of anything like the intensity of the negative anomaly that may be produced. Even a slight initial positive anomaly as in experiment I D will not be produced in nature, for the crust is first depressed in a geosyncline before the intensive tectonic movements begin. Whether the geosyncline is caused by compressional stress, as suggested by VENING MEINESZ, or by some other cause (injection, stretching, stoping, metamorfism etc.) need not concern us here as it will be discussed presently.

These experiments may also aid in finding an explanation for the phenomenon, that the crust buckles downwards and not upwards. VENING MEINESZ pointed out (bibl. 17, p. 52), that during the initial formation of waves in the crust, there is no apparent reason why a downward bend should gain an advantage sooner than an outward bend. He mentions in all 6 secondary possibilities that may help to give a synclinal wave a greater amplitude than an anticlinal one.

Five of these are ruled out in the experiments, to wit: 1) curvature of the earth, 2) water in the syncline, 3) sediments in the same, 4) higher temperatures in the same, 5) compression of the materials. These may therefore have an influence in nature, but they cannot form the main cause of the downward tendency. To these five we can also add the possible influence of the greater plasticity of the lower and hotter part of the crust, because with unheated water there is still no outward bulge formed.

The author is of opinion, that the reason must therefore be sought in the last phenomenon mentioned by VENING MEINESZ. The latter points out that when the deformation begins to take on the shape of a bulge, a thickening outwards must be obtained against the full force of gravity, whereas a downward bulge is only opposed by the difference in weight between the substratum and the crust.

In the experiments of fig. 3 we find, that at first a number of symmetrical waves are formed in accordance with the theory developed by VENING MEINESZ. As soon, however, as one of the waves attains a greater curvature, a new process sets in. This wave forms the spot with least strength and begins to move down or up, according to its synclinal or anticlinal nature. (When there is only one wave the same development takes place). There is the same degree of probability for the two cases to occur. From this stage onwards the crust reacts as a flat plain with a single zone of deformation crossing it. This deformation is no longer a simple wave, but consists of two slight bulges at the sides of a lager bend in the opposite direction. This bend tends to close and develops with rolling hinges. The crust to both sides is pressed through the slight bulges, either up into the anticline or down into the syncline, as the case may be. In the former case the entire weight of the bulge opposes the movement; in the latter case only the difference in weight of the bulge and the displaced amount of the substratum (see fig. 16, C and D). This latter difference is much smaller, both in nature and in the experiments. This must be the reason why an initial upward bulge never attains an advanced stage of development (see especially experiment I D), whereas a downward bulge may grow to a great size.

The more pronounced the plastic component of the deformation is, the sooner and more forcibly these differences between upward and downward waves are developed. As only a downward bend develops fully and finally forms an orogene, the impression is given in nature, that no upward bulges are formed atall. This is not the case, for upward bulges are prooved by the denudation products in the geosyncline.

Second series.

In these experiments the strength of the model crust is probably of the correct order of magnitude. Although we do not know what the average strength of the earth's crust is, there are valid reasons for assigning to it an appreciable rigidity (DALY, bibl. 4; BUCHER, bibl. 1). Thus JEFFREYS writes concerning the crust: „To sum up, the observed distribution of height and of gravity requires strengths in the neighbourhood of those of the strongest surface rocks down to depths of about 50 km.'' (bibl. 10, p. 95).

KÖNIGSBERGER and MORATH when experimenting on tectonics many years ago, pointed to the importance of using materials of the correct strength (bibl. 12). But it is no simple matter to determine the value the various constants should have when the model is to react as the earth's crust. YOUNG's elasticity modulus, the shearing strength, the linear dimensions and the specific gravity are all changed, but they should not be altered in the same ratio. The values are not known for the deeper layers of the crust and the influence of the plastic yielding introduces new unknown factors. This being the case we cannot hope to calculate the correct values for our model. Perhaps the method followed by the author gives the best approximation, namely to seek such a consistency that the largest topographic relief that can be sustained, has the same ratio to the thickness of the crust as in nature. In other words the vertical distance between the lowest and highest parts of the surface during the compression should remain considerably smaller than the thickness of the crust¹).

This series teaches us: that a sheet of material, that is too weak by far to sustain its own weight, even for a moment, will buckle down deeply into a more plastic substratum of higher specific gravity under

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¹) As the author did not succeed in photographing an experiment with an ultraplastic crust, the illustrations represent cases in which the topographic depression is rather large. In some of the experiments that were not photographed the depression was only about half that of fig. 7.

the action of horizontal compressional stress; that the movement begins at the spot where a very slight deformation of the crust is induced beforehand; that movement is concentrated here for a long time and that the remainder of the crust therefore transmits the stress over a long distance and therefore also that it is not necessary to assume a plastic belt in the earth's crust to explain the localization of the deformation; [Orogenetic belts are generally considered to represent strips of the crust that are more mobile than the surroundings (see for instance the sections drawn by KOBER). This may be the case, but a mere initial bending is sufficient to render that portion less resistant to horizontal compressional stress than the surroundings[1]], that part of the stress is converted into a negative anomaly along a narrow belt; that this belt may form any angle with the direction of the stress and need not be straight; that with a homogeneous crust all parts, even the surface layers, are carried downwards; that the materials move towards the zone of deformation from both sides (relatively) whereby the surface particles from both sides meet along a line that lies only slightly below the general level of the surface of the crust, to be carried down vertically afterwards.

Besides the results already enumerated this series of experiments also points to some other possibilities of crustal deformation the first of which VENING MEINESZ had also foreseen in his theory.

In the first place the crust need not always buckle down, but one part may overrun the opposite side of the belt of deformation. In the experiments this was apt to occur with a strongly curved belt, the part on the concave side overrunning that on the convex side.

The nature of the materials was also found to have an influence. The softer the crust the more apt it was to overthrust.

This puzzled the author, because from everyday experience warping would be expected to take place more easily when softer and more plastic materials were used. Prof. VENING MEINESZ pointed out to him that on the contrary, the experimental results are in close agreement with the theoretical deductions. When a crust is subjected to horizontal compressive stress it will not buckle (warp) until the stress exceeds a definite limit (buckling limit), dependant on a number of factors. If the shearing strength is reached before the warping stress is attained the crust. cannot buckle but must break through and thrust. If the same crust is studied at increasing temperatures, the shearing strength will be found to decrease from a larger value but more swiftly than the buckling limit. Above a certain temperature it will have become the smaller of the two and the crust will then refuse to buckle.

When the thickness and the specific gravity of the crust are invariable the stress at which buckling sets in is proportional to $\sqrt{E}$ (E = YOUNG's modulus of elasticity).

As long as the decrease of the shearing stress by the same rise

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1) This result confirms BUCHER's deductions (bibl. 1, p. 202), but it lends a slightly different aspect to the matter.

of temperature is relatively more than the decrease of the square root of E, the range between the buckling limit and shearing stress will decrease and finally change from positive to negative. It is around the temperature were this change takes place, that the experiments show a reasonable topographic relief. Therefore this is probably the range of temperature at which the experiments give the best representation of actual circumstances. Theoretically the crust must also be close to this point.

The author is of opinion that the thrusting mechanism will take place seldom or never in nature. Nevertheless it is of importance to add this process to our working hypothesis of crustal deformations as a possibility. The topographic depression at the surface need be no larger in this case, than with the normal shape. The negative anomaly would show as a strip, that becomes broader, the further the overthrusting has proceeded, but that will not become intenser as in the normal case.

In the second place two geosynclines close together (experiments II D) merge into a single belt of deformation, which is broader than in the normal case but may ultimately develop intensive negative anomalies along the central line. We are reminded of the two-sided orogene of KOBER, but there are important differences. KOBER assumes a belt of higher plasticity than the adjoining portions of the crust. When these two have met, the mobility must cease. In our picture there is practically no limit set to the amount of compression, for the crust may be of uniform plasticity. This need not be the case, but as stated above the assumption of a plastic belt is not necessary to obtain a localised tectonic phenomenon. Then the *bending* down into the substratum of large parts of the crust is also a feature not shown in KOBER's tectonograms.

Experiments II^{E}.

If the upper layers are of the same strength as the remainder of the crust the buckling in consequence of compressive stress must always result in their diving down in the centre of the syncline. The syncline is formed by the strength that prevents mere thickening and induces warping. If the surface layers are strong they form part of the cause for the buckling; in other words they bend down to form the inner lining of the syncline.

If the upper layers are weaker, however, the lower parts will act independently, forcing their way down in a closed syncline. No room is left for the upper part to remain in the core of the syncline. It will either crumple and thrust independently of the lower part, or it will soon be squeezed up out of the syncline. It does not appear unlikely that the latter process is responsible for the curious mode of formation of the Penninic nappes of the Alps, combined with the process demonstrated by the experiments II D.

In the opinion of the author the experiments II E show the nature of the general deformations that take place when a geosyncline is compressed. The different reaction of crust and sediments is not caused so much by greater strength of the individual component parts of the former as by absence of stratification in thin layers. The thinly stratified sediments have less aggregate strength than a homogeneous, solid column of the same thickness would show.

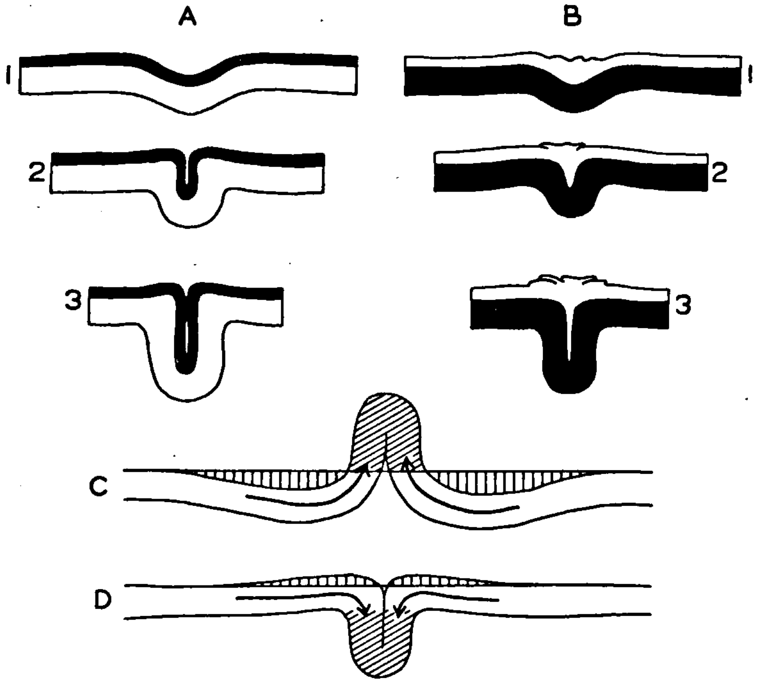

Fig. 16.

Successive stages in the compression of a floating crust. A with a hard upper layer, B with a soft upper layer. C and D: the formation of an upward and downward bulge.

The first stages of these experiments illustrate the conditions in weakly folded geosynclines. (For instance the idiogeosynclines of the East Indies that show a slight negative anomaly, see UMBGROVE, bibl. 17, II, p. 155—160).

Third series.

From these experiments it follows, that there need not be a very large difference between the strength of the crust and that of the substratum for the crustal fold to develop.

The formation of the geosycline.

VENING MEINESZ assumed that the geosyncline is produced by horizontal compressive stress, that causes the earth's crust to warp into waves. The stress also compresses the materials which therefore sink down isostatically. It was pointed out above that this cannot be imitated experimentally. Moreover, as stated, the method of applying our stress was also not quite natural. In the experiments with a more plastic crust we had to follow a different method to produce the initial deformation, by weighting and or warming the model crust. For this reason the experiments shed no direct light on the problem of geosynclines, except perhaps those of fig. 3.

Besides the mechanism suggested by VENING MEINESZ there are many other possibilities. Injections of basic magma, thinning of the crust under tensional stress (BUCHER), local stoping of the lower surface of the crust, downward convection current in the substratum, metamorphism. The interesting point to be learnt from the experiments is, that it does not appear to matter by what mechanism the geosyncline is initiated, because the heaping up of a pile of shallow water sediments to considerable thickness proves that the supporting crust has been warped downwards below a geosyncline. If compression then sets in, the substratum of the geosyncline will buckle downwards whatever the cause of the initial warping and whatever the direction of the stress is, and without the necessity of assuming it to be more plastic than the surroundings [1]). The shape and position of the geosyncline appears to be predestined by the structure of the crust or of the substratum, even if the chief or sole cause of its formation is compression. Sufficient compression at a later stage will cause the crust to buckle downwards and the contents of the geosyncline that have less aggregate strength for the same thickness, to be squeezed up and over its edges whether geosyncline and stress are at right angles or not and whether the former is curved or straight.

There does not appear to be a sudden change of the type of movement during the production of the crustal fold. There is no break between the stages in which the surface of the model crust bends down and in which the superficial particles begin to move down vertically. It might be supposed, that the earth's crust is first warped slightly by compressional stress, and then gives way suddenly by buckling down. This would explain the fact, that the long continued subsidence, recorded in the geosynclinal stage of development, is followed by a sudden folding and thrusting of the sediments during the short-lived orogenetic stage. When watching the plastic movements in the experiment the gradual nature of the process becomes evident [2]). If this conclusion is correct, we are bound to assume that tectonic stesses do not operate continuously in the same unvarying degree, but that the short duration of the orogenetic act is the consequence of a sudden increase in the stress, or a sudden decrease of the resistance other than that set up by the deforming belt. This conclusion is supported by the fact, that the alpine geosyncline was compressed in a number of phases with long periods of stability in between. As only one of these could correspond to a sudden buckling of the lower crust it is more logical to suppose that they are all due to intermittant increase, a pulsation, of the stress. The notion of a sudden giving way of the crust by buckling is based on

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[1]) See footnote on p. 192.

[2]) Compare with note d, fig. 13.

experience with comparatively rigid materials. When the substance is highly plastic compared to its size, and when its deformation is opposed by gravity (isostasy) and by strength or internal friction of its substratum, then the conception of sudden buckling as opposed to warping or bending looses much of its importance.

IV. ISOSTATIC PHENOMENA RELATED TO THE BELT OF NEGATIVE ANOMALIES IN THE EAST INDIES.

A. Objections to a recent formation of the anomalies.

It was pointed out above, that the theory of crustal buckling received important confirmation from the geological evidence brought together by UMBGROVE for the East Indies and by BUCHER in a more general sense. The negative anomalies were found to coincide with the regions where the most intensive tectonic movements have taken place. On the other hand VENING MEINESZ and UMBGROVE were confronted by a tantalizing problem when it was realized that the tectonic structures had been developed during the Upper Miocene [Tertiary f (2?)]. It is generally admitted, that the earth's crust reacts swiftly to loading or unloading by vertical movements by which isostasy is again attained. We are therefore forced to assume, either that the negative anomalies were not produced together with and by the tectonic paroxysm, or that for some reason isostasy has failed to be reestablished for several millions of years in the East Indies. VENING MEINESZ and UMBGROVE have adopted the former point of view. They imagine that the miocene diastrophism produced both an anomaly and tectonic structures. The former has disappeared by elevation and the melting and spreading of the root of the mountains. The anomaly that is now found has been formed by renewal of the stress. The thrusting and folding of the rocks ,that must be taking place, are buried below the surface and cannot be seen. All that can be perceived are the vertical displacements of the superficial layers above the orogenetic belt.

This hypothesis harmonizes with the views of MOLENGRAAFF and BROUWER who earlier assumed, that thrusting and compression are going on in the depth, while vertical movements and vertical and transversal faulting take place at the surface in the same regions where tertiary orogenesis has been demonstrated.

It must be admitted, however, that this solution of the problem is not satisfactory for the following reasons:

1. The larger features of the orographic forms in the Moluccan region were examined by the writer after the Snellius expedition had added considerably to our knowledge of the submarine topography. He found support for STILLE's view, that the deformations of the earth's crust in latter times show the character of slight undulations. This follows principally from the symmetrical profile of the deeps that occur

in the neighbourhood of the belt with negative anomalies. A number of arguments are also presented in his report (bibl. 13) that oppose BROUWER's hypothesis, where the latter concludes intensive horizontal movements in recent times from the shape of the island arcs and deep-sea throughs.

As diastrophism has set in again and again in many parts of the East Indies, there can be no objection to assuming movements at the present time on the mere fact that we do not see the results at the surface. VENING MEINESZ and UMBGROVE have shown, however, that the regions where compression was moderate, are not characterised by a strong negative anomaly. No root was formed unless the diastrophism was intensive.

All that has so far been definitely ascertained concerning deformations of recent times in the belt of negative anomalies, is the occurrence of vertical movements and tilting of strata. This is meagre evidence on which to found a theory that intensive orogenetic stress and tectonic activity are in full swing.

2. According to the theory of VENING MEINESZ a root must have been formed below the tertiary orogenetic zone, that disappeared quickly to reestablish isostasy. Two processes can be imagined by which this was chiefly accomplished. Either the crust rose up bodily, or the root spread out sideways below the crust. In the first case the surface should have risen many kilometers, especially as erosion would immediately begin to carry away the rising materials. There is geological evidence, however, that this did not take place. Although the orogenetic period was followed by rise and denudation, this process soon stopped; too soon to allow of much denudation. This is born out by the fact that only superficial rocks, but not the substratum of the geosyncline were laid bare. The other method by which the negative anomaly could have disappeared, the lateral spreading of the root, is a process of which we know nothing. It could either be supposed, that this takes place in a short time, or that it lasts during geological periods. In our case we are lead to assume that it was quickly accomplished, to account for the cessation of the rising of the surface soon after the upward movement began. But, as I will show presently, the disappearance of the root is not sufficient to annihilate the anomaly. Even if the entire bulge below the crust were carried bodily away, a considerable negative anomaly would remain within the crust. We fail to find a process by which the tertiary anomaly could have disappeared in a short time, as the only possibilities are opposed, by geological evidence or an inference concerning the distribution of mass in the crust.

3. During the successive stages of mountain building one generally observes a gradual wandering of the zone in which the principal movement of each stage takes place. We should therefore expect to find the root, that is forming at present in the Molucca's, to lie beside the tertiary orogenetic belt; but as far as can be ascertained the coïncidence between the negative anomalies and the tertiary orogenesis is perfect.

It would therefore be more satisfactory if the two could be assumed to have originated together.

4. The narrowness of the belt of negative anomalies forces one to assume that the anomalous conditions are restricted to shallow depths, that do not exceed some 50 km. VENING MEINESZ therefore has to assume that the earth's crust is only about 25 km thick and that the root plunges down into the mobile substratum to a depth of about 50 km. Most geophysicists, however, assume that the strong crust is about twice as thick. If a crust of this thickness and of uniform consistency were buckled down, the root would reach to some 75 or 100 km at least and its breadth would be at least 100 km at the top. Under these conditions the belt of anomalies would be almost twice as broad as is actually found to be the case.

5. If the compressive stress had been renewed recently to form the new root, the strips of the crust parallel to the root would thereby be raised a certain amount from their position prior to the onset of the orogenetic phase (B and F, fig. 22 and fig. 19, A). On both sides of the belt with negative anomalies are found deep-sea throughs. There are reasons for supposing that they are of recent origin (see p. 203) and therefore represent movements in the opposite direction to what would be expected on the basis of a renewed diastrophism.

The objections raised against the hypothesis of two major orogenetic phases in the East Indies naturally lead to the supposition that there was only one, tertiary period of diastrophism, and that the negative anomaly dates back to the same time. But this would imply the possibility of the strongest negative anomalies known, to have resisted the general urge towards isostasy during a period of several (5 to 10) millions of years! One cannot accept so bold a conclusion as a possibility without further considerations in its favour. In the following an attempt will be made to show that actually there is much to be said for it.

B. Regional and local isostatic equilibrium.

Before starting on our actual problem some general remarks must be made on the regain of isostasy after the balance has been disturbed by some process. Generally it has not been sufficiently emphasized that there are two independant means by which this can be attained, that must be clearly distinguished. It is agreed that the earth's crust constitutes a layer with a certain degree of strength, that rests on a substratum with almost vanishing strength, but with considerable internal friction. When a load is brought onto or into this crust, the downward pressure will immediately cause an elastic compression of the whole column below it. Besides, the substratum will begin to yield by flowing to almost any amount of extra pressure that comes to bear upon it. The crust, however, has a certain amount of strength and as long as the load is not excessive it will only be bent elastically.

After a laps of time to allow the substratum to flow, the load is carried entirely by the hydrostatic pressure of the substratum, but the strength of the crust causes local divergencies from hydrostatic equilibrium. The downwarped area is larger than the load. The consequence will be a positive anomaly of gravity over the area of the load, a negative anomaly around it that is greatest at the edge of the load and a slight positive anomaly still further out.

The larger the area on which rests a load of a fixed number of kilogrammes per square centimeter (uniform density and thickness), the closer the centre will come to hydrostatic equilibrium. In other words the smaller will be the divergence from isostasy. Thus if an ice sheet of 1000 km diameter and of uniform thickness existed long enough it would be in almost perfect isostatic equilibrium over most of its area (fig. 17, A, left). As an ice sheet thins towards its margins isostasy will reign almost perfectly throughout (fig. 17, A, right).

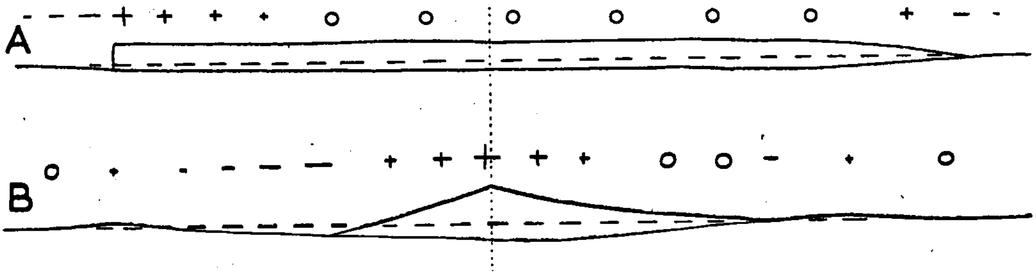

Fig. 17.

A, Anomalies over a covering of land ice. B, Around a volcano.

A volcano on the other hand is of smaller extent and the strength of the crust will have a more marked influence. Apart from a possible influence of light materials in the volcanic chamber, a large positive anomaly will mark the site of a volcano after equilibrium has been established, with a negative area round about, provided the latter is not masked by the gradually thinning out foot of the cone. In fig. 17, B these conditions are represented graphically.

The same effects, but in the opposite sense, will result from a negative load.

If the load is retracted the crust will unbend, because it was only elastically deformed, and after a while the gravity field will return to the original condition.

These types of isostatic equilibrium may be termed regional. The other type of reaction to loading of the crust will set in if the elastic strength of the crust is exceeded. In this case the crust will be deformed beyond the limits of its elastic reaction either by plastic deformation (flow) or by breaking (punching). In both cases when equilibrium has been attained the anomalies will be smaller compared to the load, than when the crust is only elastically deformed and the area over which anomalies are found will not have increased. If the crust actually breaks through it is even conceivable that isostasy is attained entirely. These types of compensation might be termed local.

If this larger load is retracted there will be an urge towards regain of isostatic equilibrium. If the crust had been deformed plastically there will remain a permanent anomaly. The amount of this anomaly will correspond to the degree of the plastic deformation, except if the anomaly in turn exceeds the elastic strength of the crust. In the latter case the crust will be again plastically deformed under the influence of the anomaly until the latter has decreased to the amount that is just balanced by the strength of the crust.

If the conditions during the original loading were such that the crust broke through, the removal of the load may result in a complete return to isostasy. If the crack had been sealed (by magma or recrystallization, etc.) the consequence of unloading would be similar to that described for plastic deformation.

The factor of time cannot be left entirely out of account. It is well known that the strength of the materials of the earth depends to a large degree on the length of the time during which the stress acts. To a certain extent at least, it may be assumed that the longer the stress is continued the smaller becomes the strength. This property may also play an important part in the reactions to gravity anomalies. It is conceivable that an anomaly that is not large enough to cause more than an elastic reaction of the crust during a short geological period, may bring about a very slow plastic deformation in the course of a much longer stretch of time.

C. The nature of the negative anomalies in the East Indies.

(Based partly on the expositions of VENING MEINESZ).

By failing to appreciate the importance of the distinction between the various kinds of reaction to anomalies of gravity, confusion has been caused. Thus the comparative speed with which the crust has reacted to the loading and unloading by ice sheets, has been tacidly assumed to prove, that all anomalies will be compensated with the same alacrity and to a like degree of perfection. Manifestly this is not necessarily the case. The deformation of the earth's crust by a load of ice is either entirely of the regional type, with bending that is only elastic, or it may be partly of the punching type as DALY suggests. The anomaly set up by buckling of the crust cannot be destroyed without plastic deformation. The experience gained by the analysis of the ice-loading is therefore of no direct value for conclusions concerning the influence of a newly formed mountain root. In the latter case the lag between the production of the anomaly and its disappearance might be prolonged or even permanent.

In order to render the difference of these two phenomena more obvious fig. 18 has been drawn. The diagram A shows the deformation of the crust below the last Scandinavian ice sheet, but exaggerated 20 times as the actual amount is far too small to be seen on the scale of the drawing. C represents on the same scale the deformation that would have to take place to annihilate the anomalies in the East Indian

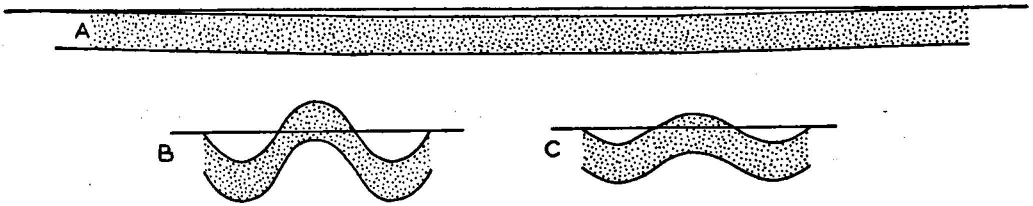

Fig. 18.

Distortions of the crust, with the vertical scale exaggerated 20 times. A, by land ice; B, to form the topographic section through the Outer Banda arc; C, to compensate the present anomalies in the latter region.

arc, and B the deformation that has already occurred, assuming the islands and deep-sea throughs to be of recent formation. It is obvious that the latter two phenomena are not directly comparable to the first.

Evidently it is necessary to gain insight into the nature of the anomaly in the East Indies before we are in a position to discuss its age.

The theory of Vening Meinesz as illustrated by the experiments holds that the crust has been warped and buckled downwards by elastico-plastic deformation. Part of the heavy sub-crustal sima has been pushed away by lighter sial. The negative anomaly thus brought into being, can be destroyed either by disappearance of the root, or by rising of the crust. The latter might be of the regional type only and this must be assumed to have taken place because the crust and substratum react swiftly in this manner. This reaction, however, could only destroy a fraction of the anomaly. We have seen above that the geological history leaves no room for the supposition that there was also a marked plastic recoil of the crust, sufficient to destroy a newly formed mountain root. Evidently the root is still there, or it disappeared by some other process than rising up.

An important point must be made here. Even if we were to suppose that the root had been cut clean off the crust and carried bodily away, or that there is no difference in density between the substratum and the lower part of the crust, there would still remain a negative anomaly. This follows from the inhomogeneous nature of the crust as it is imagined by practically all geophysicists. The crust namely is generally supposed to consist of materials with a higher specific gravity the further down we go. For a few recent opinions see Daly (bibl. 5), Cloos (bibl. 3), Eskola (bibl. 7), Gutenberg (bibl. 9). Hereby a complication is involved when the crust is buckled downwards. Not only is a bulge formed at the lower surface of the crust, but a wall of the lighter upper crust is forced down into the level of the heavier lower crust, as Bucher already pointed out. After complete removal of the bulging root there would still remain a difference in weight between the deformed zone and its neighbourhood. The anomaly caused by this phenomenon cannot be destroyed by the combined action of elastic deformation of the crust and plastic flow of the substratum (regional compensation). Either plastic deformation or breaking of the crust or some type of regional metamorphism is needed besides.

D. Can the anomalies have been formed as long ago as the Upper-Tertiary?

We are now in a better position to tackle the problem of whether the present negative anomaly may date back to the period of the upper-miocene diastrophism.

1. In the first place we must bear in mind, that the swift reaction of the crust to ice-loading and unloading in no way proves that the anomaly caused by buckling of the crust will also disappear in a short time. The chief argument against a greater age of the anomaly is hereby refuted.

2. A considerable portion of the weight of the volcanic islands of the Pacific constitutes an extra load on the earth's crust (bibl. 8, bibl. 17, II, p. 106). For the last couple of million years they have not sunk away to any appreciable extent. The case is less clear for delta's, but here also there are indications, that the crust bears part of the extra load elastically through long periods (bibl. 14). These cases imply that a considerable anomaly may last through millions of years, without any (or at least with only very slow) plastic deformation of the crust. (There must be an elastic deformation that brings about regional compensation). The mountain root in the East Indies is restricted to a belt that is no broader than either delta's or the Hawai volcanoes and one is therefore strengthened in the opinion, that the anomalies may have lasted since the Upper-Tertiary.

3. The chief difficulty is met with when we compare the East Indian case with that of the Alps. Below the Alps there also occurs a root of lighter materials, but although it is narrower than the mountain chain it is considerably broader than the East-Indian root. Moreover the whole system comes much closer to isostatic equilibrium. How can this difference be explained if it is not the consequence of greater youth of the movements in the East Indies?

In the first place there still remains a difference in age, even if the root in the Molucca's is of upper-miocene age. The Alps underwent serious compression repeatedly before the chief paroxism set in. The latter took place in middle-oligocene times according to most investigators. Although tectonic movements occurred in the Moluccan arc before the Upper-Miocene, these can hardly be looked upon as sufficiently intensive to have been accompanied by the formation of a root. Speaking in a general sense, therefore, the Alpine root is twice as old as the East Indian equivalent. There has been double the time for compensation to have been brought about in the Alps.

In the second place the Alpine geosyncline was very broad before the compression; according to CADISH 650 km (bibl. 2, p. 215—216). We have little to go upon in estimating the equivalent breadth for the East Indian belt, as so much less of the structure is exposed. But as 650 km is equivalent to the distance from the Sahul-shelf to Celebes, or from Malacca to the islands west of Sumatra at the present stage, it is reasonable to suppose, that the Alpine geosyncline was considerably broader than the East Indian geosyncline.

Now the experiments have tought us, that when a broad geosyncline is compressed, the central parts do not partake in the formation of the root, they remain at the surface as a more or less disturbed and squeezed zone, while the lower crust of the two sides, the fore- and hinter-land, push under this central zone to meet in a root at the centre. By this process a broader and less intensive negative anomaly will be produced. It appears to the author that this mental picture of the Alps fits the known facts of structural geology and geophysics very well, much better in fact than the supposition of a narrow crustal fold of the type assumed for the East Indies.

Another probable difference between the Alps and the East Indies is the greater amount of compression likely to have taken place in the former. We will return to this subject on p. 208.

E. Interpretation of the recent geological history.

We will now attempt to explain the known facts of the geological history of the East Indian orogenetic belt in terms of the theory of crustal buckling. As Timor is the only island within the zone of negative anomalies of which we possess sufficient data, it will be taken as starting point.

During mesozoic and early tertiary times a deep geosyncline occupied the present site of Timor. Later the region remained close to sealevel.

After a post-eocene period of unimportant diastrophism an intensive orogenetic compression took place during the Miocene. *The crust is assumed to have warped down and buckled into the substratum during this phase.* There must have followed a short period of elevation during which erosion cut the upper elements away. *This is assumed to represent the regional compensation of the root* by which a broad belt was slightly bulged upwards. Then followed subsidence and block faulting during upper tertiary times accompanied by very slight compression or tilting of the strata. *This is assumed to mark the stage of melting and or spreading of the root* after it had been gradually heated up and rendered plastic.

We learnt (p. 201) that the negative anomaly was not destroyed by this process, only weakened to some extent.

Finally during the Pleistocene and Recent the present island was created by elevation of several hundreds of meters. MOLENGRAAFF supposed the adjoining deep throughs to have warped down in conjunction with the elevation of the islands and UMBGROVE confirmed this opinion from geological data. The author came to the same conclusion on the strength of the profile of the troughs as revealed by the recent echo-

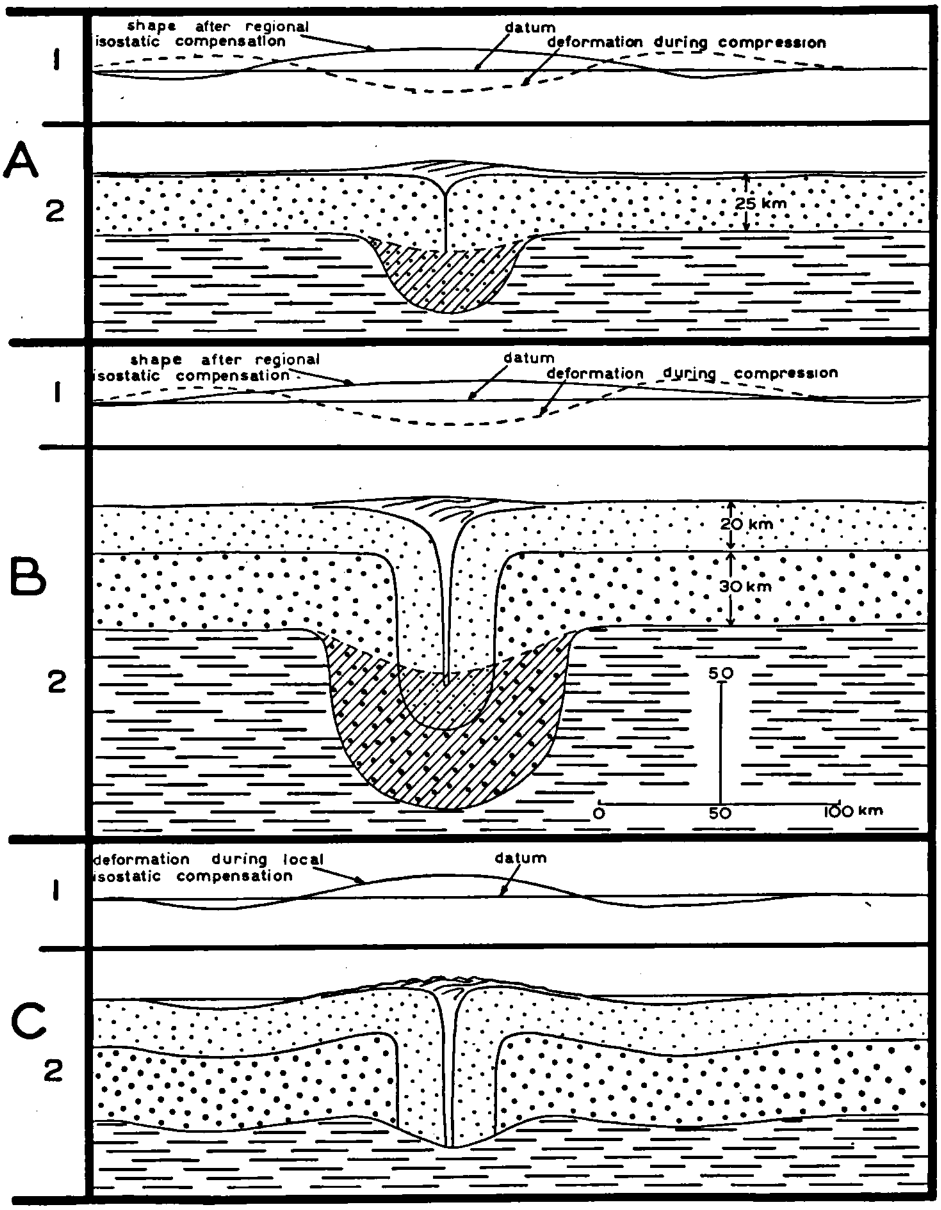

Fig. 19.

A^2: Tectogene with root according to the views of Vening Meinesz. The shaded part spreads and disappears. A^1: nature of the warping of a flat plane at the surface of the crust (apart from local buckling into the root). B: When taking into account the probable greater thickness and inhomogeneous nature of the crust. C: Slow, local deformation after disappearance of the root, in consequence of remaining wall of light materials in lower part of the crust.

sounding survey. This part of the geological history also fits into our conception; *it represents the plastic deformation of the crust by which the wall of lighter materials in the crust is gradually raised.* Parallel with this elevation goes a sinking of the adjoining area's. These had been held above their isostatic level by the boyency of the central zone of negative anomalies and showed a positive anomaly. *They will sink while the negative zone floats upwards* to its position of equilibrium. The rise of the central belt is in full swing, and probably therefore the sinking of the throughs also. That is why we are still able to detect a negative anomaly, weaker than at the close of the Tertiary but still prominent. The central belt still has to rize about 1000 meters to establish equilibrium. If erosion continues the amount is increased. To show the relations more clearly fig. 21 and 22 are given in which the vertical and horizontal scales are the same.

The anomaly-sections drawn by Vening Meinesz practically all show the upward bulge at both sides of the negative zone that proclaim the lifting of the crust at both sides of this zone by way of the strength of the crust.

It may be objected that the Outer Banda arc is narrower than the belt of negative anomalies and that the parallel strips that are bulged upwards are situated further away than the troughs. It should be born in mind, however, that a deficiency of mass is felt over a wider area than it actually occupies (fig. 20). The belt of negative anomalies is 100 km broad, but is caused by a light mass that is only about half this width. The largest amount of gravity is measured some 150 km from the centre of the negative zone, but the heaviest column lies considerably closer; in other words just about in the centre of the trough (fig. 21 and 22).

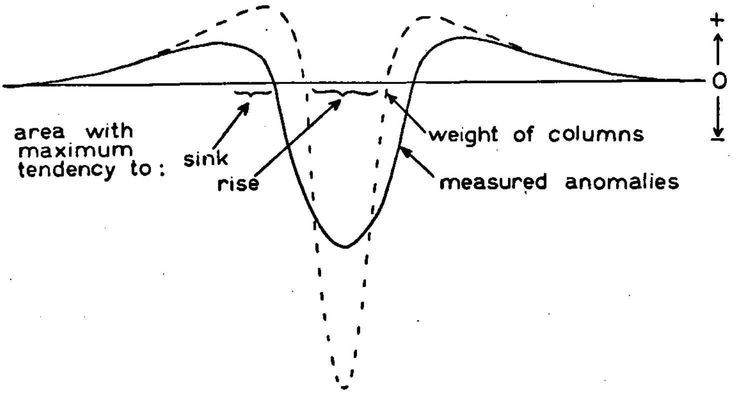

Fig. 20.

Section showing the relations between the weight of vertical columns and the anomalies that would be measured over this section.

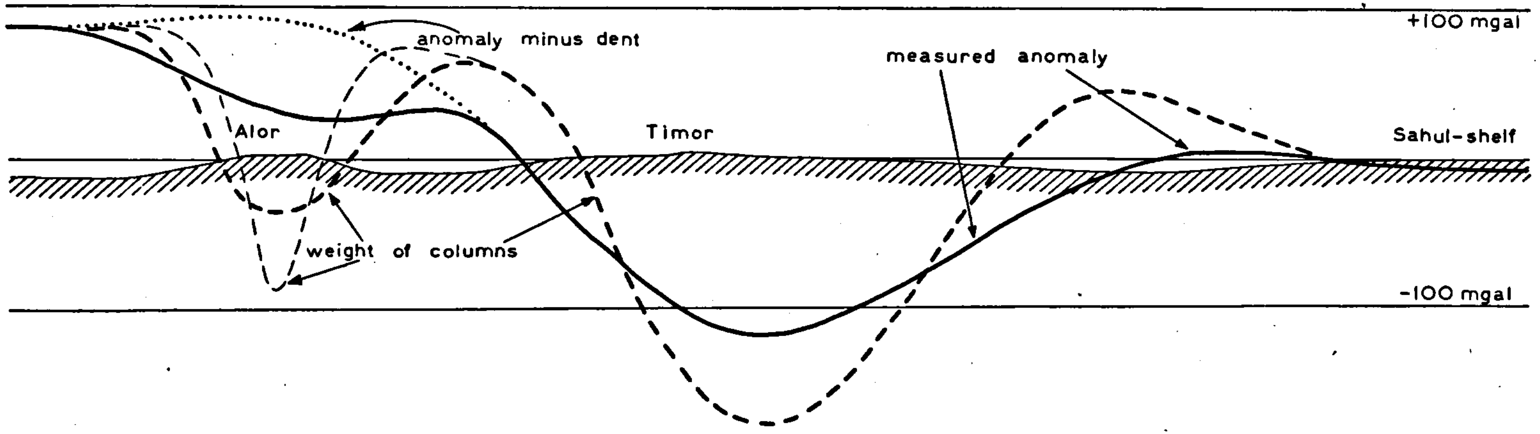

Fig. 21.

Topographic section through Timor and surroundings (scale 1: 2.000.000) with measured force of gravity and possible corresponding weight of columns.

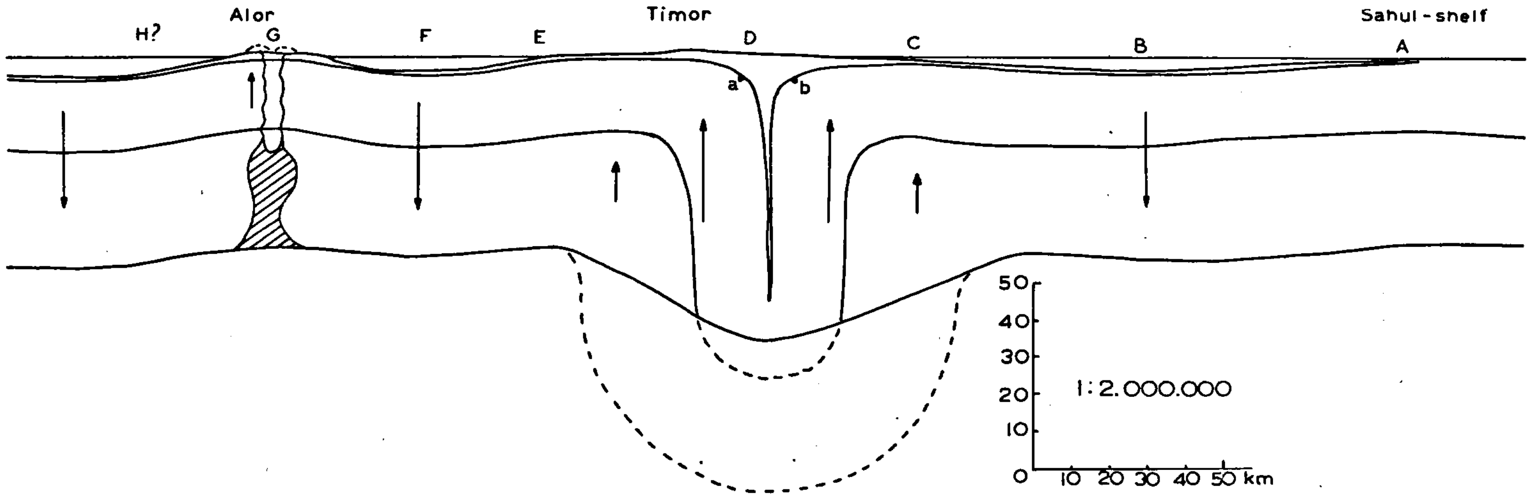

Fig. 22

Topographic section of fig. 21 with crust and root. Arrows show the gradual movements in consequence of urge towards local compensation.

The two illustrations bring out the importance of making our drawings to scale. Escher already showed the importance of this principle when dealing with the anomalies of gravity in the East Indies (bibl. 6). In our case the close fit between the anomalies, the section and the thickness of the crust is very satisfactory and lends considerable support to the interpretations given in this paper.

It cannot be denied, however, that the correlation between topography and anomalies is not everywhere possible. More particulars of the field of gravity would have to be ascertained before an attempt could be made to enter into details.

The question can be raised why the spreading of the bulge is not supposed to be a slow process, for then the broader belt in the Alps could be attributed to the greater length of time since it came into being. This, however, would give no explanation of the history of Timor between the post-orogenetic denudation and the present period of elevation. But there are two other objections. If the bulge still existed below the negative zone and were gradually spreading, then the island would have to be sinking, not rising. The adjoining belts would have to be rising in consequence of the increasing thickness of the light crust below them. In other words the recent movements would be opposite to what they appear actually to be. Furthermore the present anomaly is restricted to depths that hardly exceed the probable thickness of the crust; they do not reach down far into the substratum.

As was shown above the different shape of the alpine root may be explained in part by other means than a more advanced spreading.

Tabel 1. Distribution of specific gravity in the crust.

	Tams-Eskola	Gutenberg	Daly	Cloos
surface	2.7	2.75	2.7	2.7
lower limit crust ...	3.2	2.9	2.9	3.0
below (> 50 km) ...	3.4	3.2	3.0	3.5

Tabel 2. Average specific gravities of a few common rocks.

granite	2.67	upper layer
grano-diorite	2.72	
quartzdiorite	2.80	
diorite	2.87	
gabbro, basalt	2.97	lower layer
ultra basic rocks	3.27	substratum
eclogite	3.4	

Another question is: can the amount of the anomaly be accounted for on the basis only of different specific gravities *in* the crust? The answer depends upon the degree of inhomogenety that one is willing to admit for the crust. Taking a rough average of what various author's have assumed (Tabel 1 and 2) and supposing for simplicity that there is no compression and that the crust consists of two layers, we may draw the section of fig. 23. Vening Meinesz states that if the area of the cross-section of the root is 1200 km² the difference in specific gravity should be 0.6 to explain the observed deficiency of mass. In our section the deficiency in the crust (I, I) corresponds to 1250 km², and 0.2 difference in s. g. By adding the remains of the root inside the triangle A B C, the total is increased to the correct amount, but the depth is too large. If the upper crust is made thinner and the difference with the lower crust larger, then a more favourable distribution of the deficiency of mass is obtained.

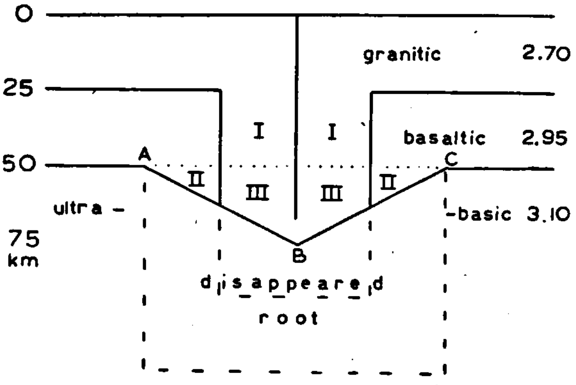

Fig. 23.

Simplified section through buckled crust.

Evidently we cannot do entirely without the remains of a root, but it need not be larger than a slight bulge.

One could also follow Bucher and suppose a more gradual chemical transition below the upper, granitic crust. According to this author the term „the earth's crust'' should be taken in a physical sense only, meaning the outher shell of the earth that has strength, and reaches uniformly to a depth of 50 or more kilometers, whatever the chemical composition. It is merely because this condition is more difficult to represent graphically or to reproduce in experiments that the writer has adhered to the original hypothesis of Vening Meinesz. The gravimetrical results of buckling and the resultant distribution in section of the materials (that is: the chemical composition of the orogene) remain practically the same with both theories. Bucher's view, however, has the advantage that the „disappearance'' of the root is a simpler process: the physical condition of the lower part of the root changes and leaves only a slight chemical difference.

We must return here for a moment to the difference between the Alps and the East Indies. It is highly probable that the amount of compression in the former was considerably larger and therefore that the root was deeper. The shaded part of the root in fig. 19, B becomes plastic merely through rise of hydrostatic pressure and temperature in Bucher's conception. If the root is so shallow, that the upper crust does not reach this depth, the root will loose practically all its influence on the field of gravity and the anomaly is then restricted to the crust. If the root is deeper, that is: if the compression is more than about 50 km, the upper crust will reach down into the root. This part will spread out and form a broader field of negative anomalies (fig. 24). This may be one of the causes for the greater breadth of the Alpine root. It does of course not explain the greater amount of compensation by rising of that region.

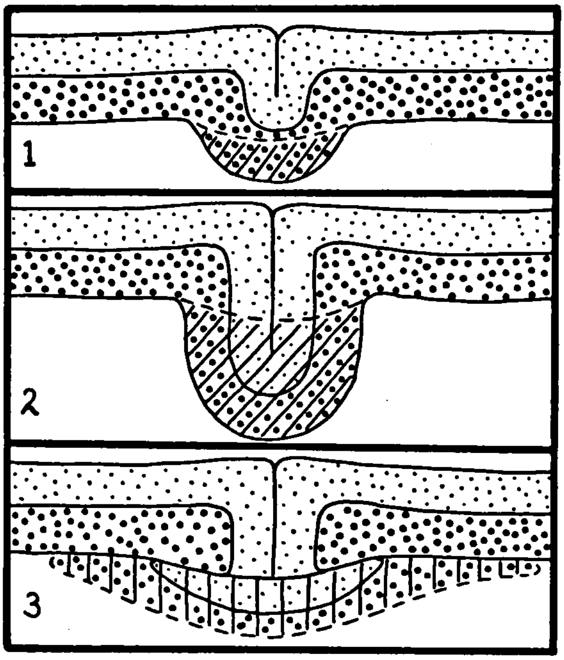

Fig. 24.

Influence of the depth of the root on the materials that spread out below the crust. With a deep root (2) the light, upper crust partakes in this movement (3).

There is also the question: by what mechanism is the present deformation going on? The crust has evidently not broken right through, for in that case the movements would be much swifter than they actually are. There is also no indication in the morphology of the island or of the sea floor that the movements are of the nature of block-faulting of the *whole crust.* The possibility of local faulting on a smaller scale is not denied and the frequent earthquakes even point to the probability of this mechanism playing a part. Metamorphism at depths and transport of materials is doubtlessly also going on. These, however, cannot explain the major movements as they would have the same opposite effects as spreading of the root.

This leads us to the conclusion that a slow plastic deformation of the crust as a whole is the chief mechanism by which isostasy is gradually being reestablished.

The breadth of the whole belt that was originally lifted by the root in the centre was of the order of 300 km (see fig. 22, A—H); it reached to a distance of 100 km from the edge of the root (A—C). The same 100 km is found between the centre of the now rising and sinking area's. These amounts appear to be of the correct order of magnitude. Thus according to BUCHER and GORANSON the radius of the area over which isostatic equilibrium is generally found to reign is of the order of 150 km or more. This type of adjustment is regional and should be compared to the breadth of the originally lifted belt beside the root (100 km). The present deformation in the East Indies shows a distance of 50 km from hinge-line to maximum (A—B, fig. 22).

F. Some Objections to the interpretation offered.

In conclusion some objections to the above interpretation of the tectonic history of the East Indies must be discussed.

1. (pointed out to the author by VENING MEINESZ). There is a distinct concentration of the epicentres of earthquakes along the belt of negative anomalies. As long as this feature of the gravity field is looked upon as the result of active diastrophism, this observation fits neatly into the picture. But if we attribute to this belt only vertical movements in the process of regaining isostatic equilibrium, it is surprising that many of these earthquakes belong to the severest known.

This objection looses much of its force when we consider the fact that tectonic movements are of a plastic type. There appears to be less cause for a sudden slip with consequent vibration of the earth, than under the influence of vertical movements that take place along faults. The frequent association of fault movements and earthquakes is a well known fact, although it remains enigmatic why an epicentre is ascertained as a mathematical point, not as a plane with large dimensions. Several of the hypocentres lie far below the crust and between Soemba and Boeroe the Outer Banda arc has hardly any epicentres (see fig. 26, bibl. 17, vol. II). The relations between earthquakes and the anomalies are therefore too vague to allow any definite opinion on the nature of this relation.

2. (pointed out to the author by UMBGROVE). There are many parts in the East Indies that have been elevated considerable amounts, although they are not situated along the belt of negative anomalies. Chief among these islands are those of the Inner Banda arc, especially in the region of Wetar. As they are not being lifted by the negative anomalies, the elevation above the latter may also be caused by some other mechanism.

An answer to this objection is found when we look closer at the gravity sections given by VENING MEINESZ. As he already pointed out (Vol. II, p. 113), practically all of these show a marked dent over the course of the Inner Banda arc and the volcanic belt to the west, even at Krakatoa. This is brought out especially in the line representing the regional reduction method. In fact it is one of the most important features of the gravity field.

The reason for this dent is not clear. One is tempted to attribute it to the magma in the volcanic chamber below the volcanoes of the Inner arc. In the opinion of the author it is perhaps too narrow and too shallow to represent a second buckle in the crust, as VENING MEINESZ believes. Whatever its cause, it must result in a tendency for the zone to be raised relative to the adjoining area's.

The dent is small and seldom reaches negative values. If the cause is situated at some depths, it may be a narrow and considerably lighter portion of the crust, so much lighter in fact that there is a marked upward hydrostatic pressure. The principle has already been illustrated in fig. 20; it is the weight of the columns, not the anomaly measured at the surface, that determines which areas must rise and which must sink. In fig. 21 two possible shapes are given of the line representing the actual weight of the vertical columns below the Inner Banda arc. It will be seen that the dent is considerably larger than appears at first sight, because without it, there would be an extra upward bulge (see for instance VENING MEINESZ' section no. 4, where the dent is absent).

Before turning to the next point the author wishes to point out, that a dent need not be expected in the gravity section over all rising area's. If for instance the rising is caused by compressive stress the region would show a positive divergence of the anomaly. Over a rising area of great extent (hundreds of kilometers) there need be hardly any anomaly at all (compare the section on regional isostatic equilibrium).

3. (pointed out to the author by MAC GILLAVRY). The most serious objection of all is bound up with the general level of the whole area. During the mesozoic era there were deep troughs, shallow seas and land area's in the region of the negative anomalies and most of the tertiary sediments were deposited in shallow water.

The orogenetic activity is supposed to have thickened the salic crust and should have resulted in an elevation of the region as a whole. Even the sinking troughs should still be above sealevel. In other words there should be found a fairly high land area along the belt of negative anomalies. Actually the average level in the neighbourhood of the Outer Banda arc is some 1 to 2000 meters below sealevel.

The explanation cannot be sought in a warping down of the crust by compressive stress. In that case a large negative anomaly over an extensive field would have been the result, instead of the slightly positive average that is found for the East Indies as a whole.

Neither does it appear permissible to suppose that convection currents have carried away the light material of the root. The velocity is estimated by VENING MEINESZ at 1 cm per year, giving only ten kilometers per million years. Perhaps this amount is too small, but even if the whole root had disappeared, the region should still lie well above sea level.

Part of the root may have changed by metamorphism to a more dense facies, but this again is insufficient to explain the present general level.

Is it possible then, that the substratum being molten has a lower specific gravity than the crust? This possibility has been suggested by DALY (bibl. 4). In that case the first result of thickening would be increase of density and rising would not set in until the root had gradually melted. Although this point of view has its merits it would also imply an unstable condition for the earth's crust. The author does not believe many geophysicists would admit this possibility.

If we turn to the Alps in the hope of finding illuminating data the difficulty is only emphasized. Here too the compression must have increased the average thickness of the crust considerably long before elevation set in. When at last the crust did begin to rise there were prolonged periods of stagnation during which erosion base-leveled the region. The same kind of difficulty is therefore found when we attempt to visualize the isostatic history of the Alps.

A possible explanation could be the thinning of the crust under the influence of tensional stress (BUCHER). Many geologists are of opinion that under those conditions the crust is not thinned out, but cracks and splits open (Red Sea, East African fault zone, etc.). The author does not wish to express a positive opinion, but he has not succeeded in finding any arguments confirming the stretching hypothesis for the case under consideration.

Another possibility is the following. The positive anomalies that reign throughout the deep basins of the Banda sea and Celebes sea and the oceans have so far only found a tentative explanation by VENING MEINESZ in the convection current hypothesis. It might be assumed that the cause of these anomalies, whatever its nature, extends also below the area of the mobile belt, thus pulling it below its natural level.

It is admitted that these suggestions for explaining the general level of the mobile belt, are only vague. But the alternative hypothesis expressed by VENING MEINESZ and UMBGROVE meets with the same difficulty. It is therefore still a problem, but not an argument against the views expressed in this paper on the age of the anomalies.

4. We have only considered the history of the island Timor. Many of the islands along the belt show hardly any elevation, and there is even one atoll, Meaty Miarang to the East of Timor. These variations cannot be attributed to differences in the size of the anomaly. We are forced to assume that for unknown reasons the recompensation varies in efficiency although the acting force is fairly uniform.

This is especially evident south of Java, where the belt lies much deeper than elsewhere, although the anomaly is as large as over the Outer Banda arc. In fact the field of gravity and the morphology are remarkably independent of the passage from the region of the Banda arcs to that of the Indian Ocean. For this reason it can hardly be assumed, as ESCHER did (bibl. 6), that south of Java the granitic upper-crust is absent and that the continent ends at the coast of this island. If that were the case the deficiency of gravity would be smaller and narrower than further along. If the floor of the ocean is formed by crystalline sima, buckling would not produce an appreciable negative anomaly, neither in the crust nor below it. Here again is one of the

„loose ends'' of our quotation from Longwell. Possibly it may be found to join up with that of the problem stated above concerning the general level, when in future we are able to answer the question: What is the nature of the difference between continents and ocean floors?

V. SUMMARY.

In this paper a series of experiments is described relating to the deformation of the earth's crust by horizontal compressive stress. A floating model crust is compressed in most cases, after a slight dent has been run across it to represent a geosyncline. When materials of the correct order of strength are used, the crust buckles down below the model geosyncline forming a root at the lower surface of the crust. At no time in this process is a topographic depression formed at the surface that exceeds the depth of deep-sea troughs as compared to the thickness of the earth's crust.

Other possibilities suggested by the experiments are that the crust may break through and overthrust and that a broad geosyncline will surmount a more complicated form of root. It was also found that the direction of compressive stress need not be at right angles to the course of the geosyncline to produce a root and that an orogenetic belt need not be more plastic than the remainder of the crust, but that the contents of the geosyncline must have less aggregate strength than a layer of the crust itself of the same thickness, as they would otherwise disappear down into the root. These experiments illustrate and clarify the theories of crustal buckling evolved by **Vening Meinesz** to account for the anomalies of gravity in the East Indies and by **Bucher** from geological data.

In the second part of this paper an attempt is made to explain the recent isostatic history of the East Indies on the basis of the buckling hypothesis. The chief aim is to show that the anomalies may date back to the miocene orogenetic phase, and that the recent vertical movements can be looked upon as a slow adjustment to regain isostatic equilibrium.

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