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GEOLOGICAL TESTS OF THE HYPOTHESIS OF CONTINENTAL DRIFT IN THE ARCTIC REGIONS

SCIENTIFIC PLANNING

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WITH 2 PLATES

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INTRODUCTION

A steadily increasing number of young men receive a scientific education; at the end of their training some of them still retain their enthusiasm and desire to start work in the polar regions. The recent great progress in the means of transport and in the technique of investigation invites them to study problems which could not hitherto be approached, or the solution of which has so far been impossible. It is not easy to discover which problems will repay the trouble of investigation if one is not well versed in the copious literature on these regions, which is not always easily accessible. A general knowledge of the results of earlier investigators (so far only partially published) is likewise required. And finally, one should have an idea of what is possible under the particular circumstances and how the investigation should be organised. To get acquainted with these facts is a difficult and time-consuming undertaking.

Just as for instance in physics all the aids of theoretical physics are laid under contribution when important experiments are prepared, so also the new tasks to be embarked upon in large geological investigations and expeditions should be prepared as carefully as possible. When such a problem is to be attacked, an outline of its various aspects should be obtained, so that the interrelation of its different parts will be clear as on a map and so as to show where it is best tackled. Such a general view will enable one to make a plan for the investigation and to select the adequate methods and technique. The programme, in its turn, renders it possible to plan the journeys. While previously, when observations were made along the routes, the planning of the journeys was most important, in modern geological research the travelling programme has to be adapted to the programme of investigation. It is necessary, therefore, that this should be as clear as possible, for the solution of the great problems within modern geology is teamwork. Accordingly the investigations to be made by various specialists should be carefully planned, instructions should be worked out, and the work organised in order to ensure the best possible collaboration.

A large expedition is composed of a number of specialists in very different fields, as shown by Dr. LAUGE KOCH's expeditions. In order to be able to cooperate, they must know the aim of the expedition. Otherwise the individual members will very easily regard their speciality as the most important and may try to push it into the foreground at the expense of the others. If all are aware of what is expected of them in order to arrive at a synthesis around the final result, it will be much easier to coordinate the work of investigation, the means of transport, and the journeys. A great organisation comprising many different branches of science may tend to become centrifugal, or the organisation may be made the goal in itself, so that it will function, but will yield no results. The question with which we are here concerned is one of the problems that has been most extensively discussed during the last thirty years, namely the hypothesis of continental drift.

Even at the present day this hypothesis is subject to discussion within many branches of science. The discussion has often been characterised by more passion than argument, but it has lost none of its interest on that account. The attitude taken up by the particular scientist is for the most part a manifestation of his scientific mentality rather than a contribution to the solution of the problem.

Brief survey of the development of the drift hypothesis.

The hypothesis of continental drift may be said to have been the subject of the most heated discussion within geological, geophysical, and biogeographical circles during the last few decades. In the period from 1915 to the present day it has played a similar role to that of DARWIN's works on the origin of species during the last decades of the nineteenth century.

A brief survey of the development of the hypothesis will render the different attitudes towards the problem more easily understandable. Even though it will necessarily be somewhat schematical, it will be useful to divide the development into four, slightly overlapping stages.

During the first stage the hypothesis was put forward several times, in several countries, partly in a rudimentary shape, but it was not taken seriously. DU TOIT mentions as precursors FRANCIS BACON (1620), BUFFON (1780), YOUNG (1810), R. OWEN (1857), A. SNIDER (1859), H. WETTSTEIN (1880), O. FISCHER (1882), C. B. WARRING (1887), D. KREICHGAUER (1902), W. H. PICKERING (1907), and HOWARD B. BAKER (1911). If the literature is studied more carefully, similar ideas may occasionally be found in earlier papers.

The second stage begins with A. WEGENER's papers (1912 a, b, c, 1915). The merit of these accounts is that they have given rise to a serious debate. The ways in which WEGENER's aforementioned hypothesis and DARWIN's theory developed are widely different. While DARWIN gathered arguments during a period of about twenty years until his idea had matured, WEGENER put forward a supposition which afterwards underwent many alterations. Possibly a dissertation may one day be written about all these alterations. Many of them are highly interesting if one knows the background.

In 1912 WEGENER travelled in company with J. P. KOCH via Iceland to East Greenland in order to traverse the inland ice with Icelandic ponies. During his stay in Iceland he realised the great risks of the undertaking and saw that perhaps he might not escape alive. And the last part of the journey was, indeed, beset with very great difficulties (cf. J. P. KOCH 1930). On board the "Godthaab", the ship of the expedition, he therefore, on the way from Iceland to Greenland, wrote down an outline of his ideas and returned the manuscripts with the ship. These preliminary notes (WEGENER 1912 a, b) were published while WEGENER passed the winter in the eastern marginal zone of the inland ice; he received the printed papers in the summer of 1913 on his arrival at Disko after his crossing of the inland ice; they were handed over to him by the young Arctic explorer LAUGE KOCH[1]).

WEGENER himself states that on studying the maps of the world he was struck by the parallelism of the Atlantic coasts, and that the idea of continental drift occurred to him in 1910. Another version is that the idea occurred to him at the sight of the ice-drift in the Greenlandic current. WEGENER's visual impressions of the ice-drift date farther back than 1912, for he was a member of the "Danmark" Expedition to Northeast Greenland 1906—08 (KOCH & WEGENER 1917). The reason why WEGENER's theory was taken seriously was probably not so much his more or less plausible examples of the drift as the circumstance that it was founded on obvious geophysical arguments, which can be easily controlled by most geologists, geographers, and biogeographers. The geological aspect of the case offered many points of attack.

In 1910 F. B. TAYLOR (TAYLOR 1910) published his first papers on continental drift, which, however, did not produce the same effect as those of WEGENER. The idea seems to have occurred to him by a study of SUESS, "Das Antlitz der Erde". It was especially the great Tertiary belt of folding which impressed him; he draws the conclusion that if folding means horizontal movement, the land-masses must slide, and

[1]) I am indebted to Dr. LAUGE KOCH for this information.

must be dragged in the opposite direction of the compression. As such a "disjunctive rift" TAYLOR mentions Baffin Bay, Davis Strait, and the Labrador Sea. In that case the sounds between Smith Sound and Robeson Channel would show traces of flaws (cf. p. 17).

The astronomer W. H. PICKERING arrives at a hypothesis of continental drift on the basis of his views concerning the formation of the moon (PICKERING 1907). He writes as follows (p. 30): "When the moon separated from us, three-quarters of this crust was carried away, and it is suggested that the remainder was torn in two to form the eastern and western continents. These then floated on the liquid surface like two large ice-floes."

Another early representative of the hypothesis of continental drift is H. B. BAKER, who arrived at the idea by an investigation of the continental forms, of which he tried to obtain an explanation. He tried closing the Atlantic and arranging the continents into one mass; the explanation subsequently offered by him is an astronomical theory.

Thus, as shown by DU TOIT, WEGENER had many precursors; but as regards most of them we do not know how the idea occurred to them. As pointed out by BUCHER (1933, p. 45), most students were struck by the parallelism of the west and east coasts of the Atlantic, but only BAKER and WEGENER seem to have used this fact as a starting-point. It is of some importance to call attention to this point; for the parallelism of the coast-lines is considered by most authors, especially the vast army of popular authors, as the most significant argument. It is the starting point of the puzzle-method, which is so simple that almost everybody who possesses an atlas or a globe may try his hand at it.

In the discussion of WEGENER's book of 1915 a multitude of arguments for or against the hypothesis were advanced, and it became apparent that it had a number of weak points in various respects. The attitude for or against is very characteristic of the different scientists. In wide circles it soon became a matter of belief, supported by such phrases as "the struggle of the generations", "the spirit of progress", and "conservatism". Thus the whole discussion was led into channels not always of benefit to science.

Among those who tested the hypothesis there were a number of stratigraphers, chiefly American, first of all C. SCHUCHERT, who rejected it on account of its errors. Other authors, adopting a more positive attitude, tried to modify the hypothesis as first outlined and to adjust it to their knowledge.

The hypothesis now entered its third stage, the stage of modification. WEGENER published a number of new editions of his paper of 1915. His last great work was written in cooperation with his fatherin-law, the climatologist W. KÖPPEN (KÖPPEN & WEGENER 1924). In addition to WEGENER's own modifications a large number of other attempts at syntheses have been made from biogeographical, palæontologic-stratigraphical, tectonic, and geophysical points of view. It is impossible to take a survey of them here, nor would it be of benefit to the present account. Many of the modifications are not particularly novel.

The three modifications best known, which give us a new picture and have had many successors, are probably EMILE ARGAND, "La tectonique de l'Asie" (1924), the excellent work by ARTHUR HOLMES (1933), and "Our Wandering Continents" by the South African geologist ALEX. DU TOIT (1937).

The first of these, taking alpine tectonics as its starting point, treats the whole of Eurasia with its diversified mountain chains of different ages. It is in this paper that ARGAND introduces the concept "plis de fond", crustal folds, which he distinguishes from "chaînes liminaires", marginal folds, "chaînes géosynclinales", geosynclinal folds, and "plis de couverture", mantle folds. HOLMES treats mainly the connection with radioactivity. DU TOIT takes South Africa and the geology of the southern continents as a starting point; his motto (as stated on the title-page) is that "Africa forms the Key".

A number of critical works, discussions, and polemics which followed in the wake of the larger works should also be referred to this stage.

In addition to these modifications dealing with the fundamental features of the hypothesis an immense "jungle" of literature of different kinds has grown up. By many of the authors the drift-hypothesis is regarded as a fact, which they simply use (even in calculations) in order to arrange their material. The hypothesis here assumes the function of an interpretation theory, as for instance gravitation, or of a dogma. This attitude is especially found in zoogeographic, phytogeographic, and palæontological works. But many authors treating general geography, and some tectonic authors, belong to this group.

It is characteristic of the biogeographers that they are mostly specialists who have investigated a limited group of animals or plants. It is impossible to give even an idea of the extent of this literature, to say nothing of a bibliography.

Most biologists assign the separation of Europe from America, or of Scandinavia from Greenland, to the youngest geological epochs, some of them even to Quaternary times. Several biogeographers are not content with a few displacements, but let e. g. the Greenland continental mass move about like a ferry-boat which is to take onboard all the different passengers from the other continents. The structure of the earth's crust would be bound to show traces of such complicated and such young movements. However, so far no such traces have been ascertained; this explains why these large Quaternary movements do not meet with the approval of geologists.

Some biogeographers state that it is impossible to explain certain distributions of plants or animals without the help of the hypothesis of continental drift; this would seem to indicate that either some important facts are lacking or that the possibilities of explanation have been far from exhausted. Possibly some of the biogeographers have failed to take into consideration the milliards of cubic kilometres of rocks which must have been in motion up to some hundred thousand of years ago merely for transporting some small living beings, which might quite well have been transported without such a huge mechanism having to be brought into action.

We will further cite the opinion of the palæontologist who is the most distinguished authority on the relation between the distribution of lower Palæozoic faunas and the drift-hypothesis; thus Øpik (1939, p. 66) writes as follows: "The distribution of Cambrian, Ordovician, and Silurian faunas in America and Northern Europe does not contradict the drift-theory of A. Wegener and may, together with other evidence, testify to the correctness of this theory" (translated from the German). Øpik does not seem to be entirely convinced by the proofs furnished by palæontology.

In addition to these standpoints there is another view which is especially held within the geodetic and oceanographic section. It is characteristic of this view that it concerns itself with those points of the hypothesis which can be checked by observations. Thus the hypothesis becomes a working hypothesis which requires to be tested by the critical application of known methods and the development of new techniques. Here the hypothesis accordingly has a heuristic function.

The drift-hypothesis may thus have a critical, an interpretative, or a heuristic function.

The methods of argumentation.

The methods of demonstrating the hypothesis correspond to the views held. It would be very interesting to compare all the arguments and counter-arguments. Here only the most important arguments will be cited without going into details.

The first group tries to demonstrate the earlier continuity of, and to bring together, some configurations which are to-day fragmentary. As configurations we must regard: the areas of distribution of present plants or animals, or of fossil organisms, and the areas of geological formations; further configurations are: geological structures which have been disrupted, coast-lines which may correspond to each other, and petrographic provinces. All these may be termed discontinuous configurations. The method consists in re-grouping the disjointed members in order to reconstruct the earlier unit. This earlier unit is postulated and is in most cases hypothetical. This procedure, which is that most frequently employed, may be called the "jig-saw puzzle-method".

If this method is employed, it ought first to be proved that the individual areas to be considered as parts of a unit actually are so. Attempts to prove this are very rarely met with in the literature. It should not only be a logical unit (obtained by giving the parts the same denomination as the unit) but its parts must have belonged to one and the same real object. It is, for instance, possible to find a fragment of a fish body and a fish tail; both are parts of a fish, but it cannot be taken for granted that the tail and the other fragment belonged to the same fish. Two fragments of a folded segment may have been folded during the same epoch and accordingly obtain the same name (Caledonian, Hercynian, Alpine); but it is not certain that they can be directly joined together. Thus it should also be proved that the parts can be brought together so as to touch each other, and finally, that the way in which they are put together is the only one possible.

In this connection a very significant point should be mentioned. We may ask: Is it permissible to regard the continental slope as the edge of the continental masses—or of the sial block, in WEGENER's terminology? In the jig-saw puzzle-method, in which the continental outlines are used, it must be assumed to be so, for otherwise it would be impossible to make the margins join; that is why the sial blocks and isostasy play such an important part in WEGENER's theory. One of his most important arguments is the hypsographic curve of the earth's surface with the two maxima corresponding to the continental platforms and to the ocean floors. Thus the hypsographic curve will lend support to the jig-saw puzzle-method. All investigations connected with the structure of the continental edge will therefore also more or less touch upon the puzzle-method, which cannot, of course, be disproved, if it is supple, as ARGAND puts it. Thus the results of the investigations cannot make it lose ground; they can only increase or diminish its prestige and credit.

Hence it is necessary to cite the results of some recent investigations which may be dangerous to what might be termed "the rough puzzle-method", as it is employed at the present time, and which may perhaps force its adherents to use a more refined method, the appearance of which has long been a desideratum.

Some geophysical investigations made by MAURICE EWING (1937 a, b, 1939), N. H. HECK, and their collaborators have shown that the wedge of the Cretaceous-Tertiary sediments of the piedmont region in Eastern America thickens rapidly in the direction of the continental slope; there it is approximately 2 miles thick. The old surface exposed on the continent above the Paleozoic folds and the Triassic fillings is at least a mile below the foot-point of the continental slope. So far we do not know its continuation beneath the deeper parts of the sea bottom; but the further results of the investigation will be of the greatest interest.

Investigations carried on in the British waters have shown (BULLARD 1940, JONES 1937) that already at the 200 m contour the solid bed-rock is covered by nearly 3000 m of sediments with other physical properties than the substratum. Thus the bed-rock is situated at just as low a level as the deepest parts of the Scandic. Accordingly part of the British continental shelf is built up of younger sediments and does not, therefore, belong to the "socle" itself[1]). They are partly "foreset beds of the continental slope", partly "topset beds of shelf-seas", as they are called by CHAMBERLIN (CHAMBERLIN 1914). These parts of the continental shelf push their way into the sea owing to abundant sedimentation; accordingly continental slopes with extensive sedimentation differ greatly from sectors with a slight supply of material.

If, now, some portions of a continental slope are sedimentary fillings while others are the edges of an earlier basement, the rough jig-saw puzzle-method can no longer be employed; in that case the margin of the continents must first be subjected to fresh investigations and a very close criticism.

An important aid in this criticism is a study of the magnetic anomalies of the orogenic segments of different ages. According to NIPPOLDT (1935) the anomalies are not influenced by the boundary between the continents and the oceans within the areas studied by him. From this he draws the conclusion that the inhomogeneous magnetisation of the earth's crust in coastal regions, is not influenced by the outermost 3 km. The relation between this conclusion and the thickness of the sediments should be investigated.

Another aspect of the problem of the continental margins is constituted by the submarine canyons, the study of which has given rise to a copious literature and many different hypotheses (cf. p. 40).

Another method is the construction of possible trajectories of

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[1]) The fundamental researches of the Oceanographical Institute of Göteborg (Sweden) carried out by HANS PETTERSSON, B. KULLENBERG, and W. WEIBULL point in the same direction (cf. Geol. För. i Stockholm Förh. vol. 68, 1946, pp. 499—500, and UNDA MARIS, Göteborg 1946, pp. 58—71, 8 fig.).

the movable parts. This method was incidentally tried by TAYLOR, WEGENER, ARGAND, DU TOIT, and other workers without, however, being further developed.—The determination of trajectories by means of the direction of overthrusts must also be referred to this group. Another example will be dealt with in more detail below (p. 22).

A third method, which is mostly employed by geophysicists, is more heuristic. Its aim is to measure the present changes in longitude and latitude. The measurements on Greenland have been the centre of discussion ever since WEGENER called attention to this point. Opinions differ greatly concerning the interpretation of these differences.

The first to deal more intensively with the question is J. P. KOCH (1917). He writes as follows (p. 240): "Wegener gives in Petermann's Mitt. 1912 the difference as 590 m, which information he got from me. The longitude of the observatory in Danmarks Havn was then not finally computed, and the coordinates of the triangulation stations were not determined." And on p. 255 he says: "It cannot be denied that the whole of the research is encumbered with very serious uncertainty, and that the two mean values of the yearly drift, 9 m within the period 1823—1870 and 32 m within the period 1870—1907, do not harmonize very well . . .". "It would, I suppose, in reality seem more natural if the drift did not show itself an even movement, but was subject to considerable variations at different periods." Later on, in 1922, the stations in West Greenland were remeasured by P. F. JENSEN (1923), who assumes that a displacement towards the west has taken place ("dérive de longitude — 4 s. 9 —") between 1863 and 1922.

JELSTRUP (1932) assumes that an actual displacement of 250 m has taken place in the interval between 1870 and 1932. The geodetic station on Sabine Ø is assumed to have moved westward at a rate of c. 10 m annually, or 1500 miles in 600.000 years. EDMOND GUYOT (1935), after closely examining all available data up to 1935, finds no proofs of an actual continental drift between 1870 and 1935. From a geological point of view it must, in addition, be asked:

1) Is it permissible to extrapolate the values, if any, for the period 1870—1932 over geological periods, and to assume that the movement always had the same size and direction?
2) If the station on Sabine Ø has moved, is, then, this movement of a local, a regional, or a planetary nature? Has the Greenland block as a whole been actually displaced?

In order to answer these questions it will be necessary not only to have one, but several points in East Greenland which should be connected by a triangulation network and accompanied by the appurtenant geodetic observations (measurements of gravity, deflection from the vertical, magnetism, etc.) distributed over the geological units, moved each in relation to the other, which the geological investigations have shown to exist in East Greenland.

Thus the land has been found to consist of a number of geologically easily movable blocks. The movements may accordingly be supposed to have taken place between the blocks and to be of a local or a regional nature. The young secular movements between the blocks can be ascertained by a measurement and accurate coordination of the many well developed shore lines; in this way it will be possible to determine the surfaces of movement and the areas which will be most suitable for geodetic measurements.

If the problem is considered on a broader basis than the distance from Greenland to Europe, it may be elucidated from another angle through the works published by the BUREAU INTERNATIONAL DE L'HEURE, Paris (STOYKO 1942, ESCLANGON 1942). The observatory at Paris every day receives radio-electric time signals from more than twenty observatories distributed all over the globe, and several times a day it dispatches time signals the precision of which now exceeds one-hundredth of a second. Thus there is a possibility of determining every day about twenty points distributed over the globe and this determination may be made with great accuracy. The classification and comparison of the determinations made during the last twenty years probably constitute the most important and best material for a study of the variations of longitude.

STOYKO (1942) has worked up the material and has represented a number of the results in a graph. In his opinion two hypotheses may explain the conditions, viz.:

1) pulsations of the earth, considered as an elastic body, and
2) contractions and dilatations of the Earth's superficial layers, the continental drift.

According to STOYKO the drift-hypothesis cannot explain the conditions; whereas the pulsations may do so. Variations of 2.6 m or 2.1 m, respectively, of the radius of the globe for latitude 45° seem to correspond to the observed variations of longitude.

ESCLANGON (1942), the well-known director of the observatory at Paris, points out two items in STOYKO's investigations:

"1° Il n'existe pas de déplacement notable progressif et continu (les astronomes disent variation séculaire), du moins pour les latitudes moyennes, entre le continent américain et l'Europe d'une part, entre l'Europe et le Japon d'autre part.—La moyenne de 20 ans se traduirait en effet par un éloignement annuel de l'Amérique du Nord, par rapport a l'Europe de 15 cm, un éloignement du Japon de 90 cm. Nous sommes loin de la théorie de Wegener, lesquels atteignent des mètres et des dizaines de mètres par an. Ce point parait désormais éclairci en ce qui concerne les déplacements correspondants aux latitudes moyennes.

2° Les observations paraissent donner d'assez importants déplacements relatifs et périodiques pouvant dépasser une dizaine de mètres en amplitude et qui, de plus, seraient en relation avec la période d'activité solaire. La question est ici beaucoup plus délicate." He further says: "Les courbes de la note ci-dessus paraissent comporter des variations de longitude qui ne sont pas toutes en rapport direct avec l'éloignement des points terrestres considérés. Il y a donc lieu également d'envisager la possibilité de déplacements relatifs et localisés de la surface terrestre; déplacements en quelque sorte indépendants les uns des autres et qui suivent comporter, supplémentairement, des variations de niveau."

Here astronomy seems to show us new roads and a new technique. Pulsation hypotheses have previously been put forward within the geologic sector by a number of authors. The well-known American stratigrapher GRABAU (1934) has become its champion in recent times; but according to this author the ocean level is pulsating. BUCHER (1933, pp. 115—124) also advocates a modified pulsation hypothesis concerning the earth's crust. However, this theory has not yet been so fully elaborated that it can be tested by geologic investigations in the field.

For some time to come we must therefore be content with the investigations and results of the astronomers, which in a near future will become still more exact. All important observatories are now equipped with quartz clocks, which permit a precision of 10 microseconds (i. e. 10^{-5} sec.). This new era of chronometry opens up wide perspectives for geophysical research, since a number of phenomena, the annual amount of which previously lay below the threshold of the technique of measurement can now be determined quantitatively. Students of structural geology will follow this evolution with great attention.

This group of methods further includes the exploration of the sea-bottom. If the continents have moved away from each other, there must be a gap between such formations as were ruptured. According to the drift-hypothesis the sea-water fills this gap. According to the more conservative hypotheses the intermediate area has subsided so as to form the substratum of the sea-bottom. If some particular formations could be ascertained there, it would be possible to decide whether a piece of the jig-saw puzzle is lacking between the ocean shores, or whether it is found below the water.

The question at issue between the protagonists and antagonists of the drift-hypothesis is accordingly how the hitherto unknown area of the sea-bottom should be filled in. Both groups fill the *terra incognita* with unproved hypotheses. This is the reason for the emotional undertone. The well-known physicist VENING MEINERZ (1939) seems to arrive at a similar result.

However, this will not always be so. The question may be decided by careful investigations of the geology of the sea-bottom. PIGGOT (1938) has devised a tube-lead by means of which very long samples can be taken and which has already been very helpful towards an understanding of the structure of the sediments in the Atlantic. The geophysicists (EWING 1937 a, b, 1939, JONES 1937, FIELD 1936, 1937) have devised methods for a determination of the structure of the sea-bottom much like the methods employed on land, for instance in the oil fields. These methods have already yielded very promising results on the coast of New England and in the British waters (cf. p. 12). It is probable that they will become more extensively employed and further developed so that they may to some extent contribute to decide the problem of continental drift[1]).

The fact that the proofs of the continental drift are mostly found below the water and that so far no methods have been invented by which we can make a detailed investigation of the structure of these areas, at present leaves the question open, so that everybody may answer it according to his own temper and pleasure. What some are inclined to think, others are disinclined to thinking, and this may perhaps help to explain the intensity, and at times somewhat grotesque forms, of the discussions.

The geometric character of the movements.

The large majority of the drift movements are assumed to have taken place more or less transversely on the fragmentation planes, a gap being found between the fragments. This gap explains the origin of the sea basins. In addition to these gaps (or down-faulted stretches, according to the other hypothesis) displacements resembling flaws must have taken place parallel with one of the fault planes. It must be possible to check such displacements by geological methods if we are able to find the right localities for checking. In such a case there

[1]) Cf. the footnote on p. 12.

is *no intermediate sea-covered area* which has either subsided or is represented by a sea-filled gap. Accordingly the investigation methods of classic geology can be employed. There are two possibilities: either the structure exhibits a certain "décalage" (parallel displacement) on either side of the dislocation, or the earlier connections can be reconstructed in a different way.

Minor displacements have for instance been demonstrated along the San Andreas Fault in California (Gutenberg 1941 a, b). Taylor (1910) and Wegener (1915) regard the straits between Smith Sound and Robeson Channel as the results of a displacement parallel with these straits. Greenland is assumed to have moved northeastward in relation to Ellesmere Land. The geological investigations made by Lauge Koch (1920, 1923 a, b, 1924, 1925, 1928), Wordie, and Bentham (Humphreys 1936) have not hitherto confirmed this assumption. Future investigators should direct their attention towards these conditions, which in fine weather might be photographed directly from the air, especially when taken in the direction of the axis of the folds.

In addition to these, many authors assume that rotating movements have taken place. In that case the centre may lie within or outside the area moved; it may be stationary or be displaced during the movement. These movements are, of course, of another kinematic and dynamic character than the first-mentioned ones.

On a geometrical basis the movements of drift, if any, may be divided into:

1) parallel displacements
   a) with a gap between the fracture planes;
   b) with movements along one fracture plane;
2) rotational displacements
   a) with a stationary centre {(outside) / (inside)} the unit moved;
   b) with a displaced centre;
3) combined movements.

Future presentations should be adapted to the geometric character of the displacements. Thus we obtain one more method for the study of the movements, viz. the *geometric construction of possible traces*. This, of course, is best done on a globe; however, an example will be given below of a study of parallel displacements on a plan which can be represented on a map (p. 22). Altogether, however, it will be difficult to employ this method if we have no indications to follow.

All these considerations have induced the author to present this paper to his colleagues and to future polar explorers; possibly it may help towards the solution of a *much discussed problem*.

The origin of the Scandic and Arctic seas.

The sea between the Norwegian coast, Bear Island, Spitsbergen, Greenland, Iceland, the Faroes, and the Wyville-Thomson threshold was called the Scandic by DE GEER (1912). He regarded it as a structural unit and assumed the existence of a Scandic land-mass rising above sea-level right into Tertiary times (DE GEER 1919). His arguments are as follows: On Spitsbergen there is a synclinorium filled with Mesozoic and Tertiary, largely detrital, sediments, which according to ORVIN (1940) have a total thickness of nearly 4600 m. This material originates from the west, not, as might be assumed, from the east. The area which has supplied these sediments cannot be the comparatively small Prince Charles Foreland, but a much larger area situated farther westward. This land-mass must at times have been much elevated above the synclinorium, so that rather coarse clastics could be transported from it to Spitsbergen. In Tertiary times the zone of western Spitsbergen was folded; the Scandic land-mass subsided, while the surrounding islands were uplifted.

The protagonists of the drift hypothesis give a quite different explanation of the origin of the Scandic, viz. that this basin opened because the coasts of Greenland and Norway slid away from each other. Several modifications of this hypothesis are known. They differ as regards the position which Greenland is assumed to have had in relation to the European block. However, differences do not seem to have impeded the protagonists in their discussions, these being mostly kept on a higher plane.

For a geological checking the assumed earlier position of Greenland is of the greatest importance. This is easily understood if we consider the positions proposed. We may distinguish between three main types:

1) WEGENER and his followers place Greenland in the following position: The region around Independence Fjord and Nordostrundingen is placed near the north-western part of Spitsbergen. The area around the large fjords between Scoresby Sund and Godthaab Gulf lies against the Barents shelf. The Blosseville Coast with a rudiment of Iceland lie off northern Norway (Nordland, Tröndelagen, and Möre). The southernmost point of Greenland is placed outside the shelf near Ireland and Rockall and, on the other side, near the southeastern corner of Labrador. Many authors, i. a. BAILEY and HOLTEDAHL (1938), accept this presentation. It is found several times in DU TOIT (1937, e. g. p. 145, fig. 18).

2) Another position has become better known through the paper of F. E. SUESS (1938, fig. 1, p. 325). The northeast coast of Greenland

here lies along the coast of Norway; Independence Fjord opens towards the Barents shelf, the Nordostrundingen lies near Bear Island. The folds on Peary Land point in the direction between Bear Island and Spitsbergen; that on Grant Land towards Spitsbergen. The Archæan peninsula of South Greenland projects into the Atlantic without falling into line with the general structure. A similar position has previously been proposed by ALDINGER (1937) and by the present author (1935, pp. 47—51).

3) A third position will be mentioned below (p. 23).

Thus we cannot say that there is any very great agreement between the views held by the different authors. The same region, e. g. Scoresby Sund, according to the different authors is situated in places which are nearly 1300 km distant from each other, or about four times the length of the Swiss Alps or the distance from London to Florence. Thus accurate placing does not seem to have been aimed at. Even one and the same author suggests various positions in the different figures. Of the many examples only one outstanding one should be cited here: DU TOIT (1937) in his fig. 18 (p. 145) and fig. 40 (p. 279) places Greenland in position 2), while his fig. 29 (p. 222) suggests solution 3). The paucity of the grounds given in the different papers for assuming the one or the other position is remarkable. It would seem that the protagonists of the drift hypothesis were often satisfied to say as GALILEO GALILEI: "Eppur si muove!"

This shows the difficulties and uncertainties of the jig-saw puzzle-method. Its adherents mostly proceed in the following way: first they *assume the drift hypothesis to be correct*, and then the puzzle begins. ARGAND (1924, p. 292) has described this in the elegant way characteristic of him: the theory is not based on local or regional observations, but on a "vision d'ensemble" which "satisfait à l'exigence de synthèse". But this latter claim for a synthesis does not always seem to have been satisfied, since as yet nobody has been capable of obtaining a general view of the *whole* puzzle and of coordinating *all* the simultaneous movements. On following the movements within a section in the paper of one author, one always comes to a limit where they cannot be traced farther without internal contradictions. In most papers the game is not carried thus far, the author preferring, consciously or unconsciously, to begin in another section. Hence different positions are presented by the same author.

A much smaller number of hypotheses have been advanced concerning the *origin of the Polar Sea*. Most students use Mercator's projection or other projections in which the polar countries are deformed and appear near the margin of the map and thus fail to attract one's attention. If a certain drift is asssumed to have taken place in the Scandic the origin of the Polar Sea should also be taken into consideration. The movements in one sector must, of course, give rise to sliding within the other sectors also.

As far as the Polar Sea is concerned, we may assume that it has arisen as a result of subsidence or it is the opening formed between two land-masses that have moved away from each other. The latter alternative is assumed by DU TOIT (1937, fig. 29, a, b, p. 222). In WEGENER's figures the Polar Sea is mostly found at the margin and is not distinctly visible. A comparison of various representations seem to show that WEGENER is of opinion that the Polar Sea has existed as an open basin ever since the Devonian time (WEGENER 1915, fig. 17, KÖPPEN & WEGENER 1924, fig. 22, p. 155, fig. 19, p. 117, fig. 21, p. 143).

The Polar Sea is regarded as a very old basin by some authors, especially by American stratigraphers. A study of the American Palæozoic faunas has revealed a number of waves of immigration coming from the north. However, these results of the investigations do not answer the question whether it has been a deep sea or merely an epicontinental sea. Corals and their accompanying faunas show that there must have been extensive shelf regions on which this fauna could develop.

Through seismic studies GUTENBERG (1939) arrives at the result that the substratum of the Polar Sea is of about the same nature as that of the Pacific. Those who regard the Pacific as an ocean dating back to the earliest ages will possibly also regard the Polar Sea as a very ancient basin. The substratum of the Atlantic and accordingly also that of the Scandic are assumed by GUTENBERG to be made up of granitic material of a thickness of 12—20 km. The two sea areas should accordingly differ greatly in a geophysical respect. If the Polar Sea resembles the Pacific in a geophysical respect, the structure of the surrounding mainlands is highly different.

A comparison between the Polar Sea, the Pacific, and the Scandic gives noteworthy points of view: the Scandic is surrounded by and probably also filled with basaltic rocks of great thickness. Basalt formations are known from several places around the Polar Sea, but they are not of the same extent everywhere. In the Pacific basaltic rocks are the most widely distributed and they also form huge plateaus in different marginal areas, the largest extending in length from Indochina to the mouth of the Amur River and in breadth from the shore to Mongolia. HUANG (1945) writes as follows (p. 121): "Of the igneous activity in the Himalayan cycle the basaltic flows along the Pacific coast form the most significant part, overshadowing in their importance all other occurrences of either intrusive or extrusive nature." The question is then: Are we to regard the Scandic as the marginal area of the Polar Sea, similarly as the Chinese basalt province is associated with the Pacific?

As first pointed out by De Geer (1926) there is a peculiar relation between the Scandic and the Polar Sea (cf. pl. 1). If a line be drawn northward from Vesteraalen in Norway, following the continental slope off Bear Island and Spitsbergen, it will cut across the Nansen threshold between Spitsbergen and Greenland, and subsequently follow the continental margin north and west of the Arctic archipelago as far as the mouth of Amundsen Sound. This line, which follows a small circle on the globe, is one of the most characteristic, and it is surprising that it has not received more attention from geologists, geophysicists, and geographers.

Looking along this line from Norway, we shall make the following observations: along the first part there lies, to the right, an elevated portion of the earth's crust with Spitsbergen, Bear Island, the Barents Sea, and Franz Josephs Land. To the left lies the sea basin of the Scandic. Beyond the Nansen threshold the high area lies to the left, viz. Peary Land, Grant Land, and the Arctic archipelago; to the right lies the deep basin of the Polar Sea. Within the first part, the heights above the sea are not particularly great (Bear Island 534 m, Spitsbergen 1717 m); the greater part of the area, the Barents Sea, lies some hundred metres below sea-level. The depth of the Scandic generally varies between 2000 and 3000 m. Within the second part very great altitudes are found to the left (Peary Land, highest top 1950 m, along the north coast 12—1400 m; Ellesmere Land nearly 1300 m, rising towards the north, Grant Land 3000—3300 m), which decrease towards the south, though large parts of the surface still lie above sea-level. To the right the surface of the earth descends to great depths, in part more than 4000 m. Thus the differences in height are highly different within the two sectors, being in the first part 4000—4500 m, in the Polar Sea sector of an order of magnitude of 7000 m.

This line, which we call De Geer's line, is regarded by Taylor and Du Toit as a possible path of displacement, along which the land masses may have drifted. However, they have drawn no conclusions which might have rendered checking by geological methods possible.

In the following pages we will now try to outline some consequences and methods by which the drift, if any such has taken place, can be tested.

Consequences of a hypothetical drift along DE GEER's line.

In spite of the many intensive discussions no attempts seem to have been made so far to develop a method by which to construct a track of displacement and its consequences. Below an attempt will be made to follow DE GEER's line.

If we assume that this line marks the track of a displacement, it is fairly easy to study the consequences on a globe. In this case the track of displacement forms a small circle. It can be outlined by placing a thread round the globe.

It will be fairly easy to construct parallels in order to obtain the displacement of each individual point. On a map the construction is a little more difficult. The method which yields the best result is to construct a Mercator's projection whose equator is parallel with DE GEER's line. Professor EDMOND GUYOT, the director of the Astronomic Observatory, Neuchâtel (Switzerland), has been so kind as to compute the coordinates and to draw a skeleton, which forms the basis of the drawing (pl. 1). Professor GUYOT (1943) has published his computations in order that it may be possible to construct skeletons of coordinates for the other supposed tracks of drift[1]).

Mercator's projection has the advantage that the angles are preserved and the contours are not too much contorted in the central zone, a quality which is very valuable in technical discussions. In future such maps will be constructed for the air traffic, for, as shown by GUYOT, they are especially suitable as route-maps, the circle of the route being the equator of the map. Such a map has been published in the Geographical Journal (HINKS 1940, GEOGR. SOC. 1944). As early as 1935 SOLGER constructed a very interesting map on Mercator's projection on a cylinder whose axis was parallel with the perpendicular in 20° E. long, 20° S. lat. The centre of this map lies in Greenland. It is accord-

[1]) On many other discussions the construction of parallel circles would have a beneficial influence, especially if, as is often the case, folds, faults, dikes, etc., are classified over large areas according to their strike. If, for instance, N—S, NE—SW, or NW—SE striking objects in NW-Europe, NE-Europe, and Asia are parallelised, these directions need not be parallel. The direction is determined by the angle they form with the meridians. Thus, if we classify according to angles of strike over large areas, it must be implicitly assumed that the plane of deformation is connected in some way with the present position of the poles. That is to say that a certain group of possibilities of explanation is retained, while others are eliminated. If we do not wish to do so, other zone circles and parallel circles must be constructed.— There seems to be a number of authors who are familiar with the names of minerals, rocks, and fossils, and who use the four points of the compass in the same way, apparently forgetting that these latter are a function of the position on the sphere and accordingly possess other logical properties.

ingly a "greenlandocentric" map, (cf. Solger 1935, p. 2, fig. 1). The constructions may also be extremely useful in discussions of the so-called "lineament tectonics" (Sonder 1938, 1939). Accordingly the use of the method is not limited to the drift-theory.

In pl. 1 the drift can easily be followed in thought, for its direction is parallel with the horizontal plane of the drawing. If the American-Greenlandic block is moved towards the European block along De Geer's line, the third position will be obtained (cf. p. 19). This is characterised by the following features: The highlands Peary Land and Grant Land will come to lie outside Spitsbergen and the Barents Sea. According to the drift hypothesis they should correspond to the Scandic land area of De Geer, i. e. the land which has supplied the clastic rock material for the Mesozoic and Tertiary sediments on Spitsbergen.

This gives us a possibility of geologic checking.

Thus we may try to answer the question: Was it the highland Grant Land—Peary Land which supplied the clastic sediments found in the synclinorium of Spitsbergen, or have the two areas nothing in common?

In such a research the weathering, the transport conditions, and the associated selection should, of course, be taken into consideration. Some of the sediments may have been subjected to several transports and sedimentations. As shown by Orvin (1940), Spitsbergen subsided to the south in Mesozoic times, with a more or less east-west directed axis, while the later movements had an axis more parallel with the meridians. The investigations required if we are to answer the question may be reached through two groups of research.

Geological investigations to be made in order to test the drift hypothesis.

The following conditions should be investigated:

1) The clastic sediments in the synclinorium of Spitsbergen should be studied by petrographic and geochemical methods.
2) A geologic-petrographic and a geochemical investigation should be made on Grant Land and Peary Land.

1) The Mesozoic and Tertiary rocks of Spitsbergen have been studied by Norwegian, Swedish, British, and German scientists and have furnished good clues to the stratigraphy, the thicknesses, the facies, and the general character of the rocks. Orvin has published an excellent survey of these investigations, to which the reader is referred (Orvin 1940). These works may form a good basis for the petrographic studies. So far only scanty information of the petrography is available; it is based on a few casual samples.

DE GEER estimates the thickness of the Tertiary beds at van Mijen Bay at 1200 m, those southwest of Advent Bay at 1000 m; the maximum breadth of the zone is nearly 60 km. During the Tertiary transgression also the western horst was lowered, and the whole subsidence must be estimated at at least 1200 m with a subsequent upheaval. While DE GEER, HOLTEDAHL, and FREBOLD assume a transport from the west, ORVIN also believes in a supply of sediments from the north. In his description of Kings Bay ORVIN states that a large part of the Tertiary sandstones studied exhibit a grain size of 0.2—0.3 mm; some of the grains are rounded, others angular; their size may amount to 0.5 mm. These sediments would probably be well suited for investigations of heavy minerals according to the method of EDELMAN (1939). Some beds contain Permo-Carboniferous pebbles of the size of a hazelnut. HÄGG (1927) writes that quartz occurs in rather large grains. He describes beds of impure limestone rich in fossil wood, of which he gives a chemical analysis. He observed "organic matters with a faint smell of petroleum". He estimates the whole series at 1161 m, and gives detailed sections. KLEIBER (1944) has examined two samples of Tertiary rocks from Kings Bay and found that they belong to a tourmalin-rutile-zircon combination. These results are not very satisfactory. The petrographic and mineralogical characters may, of course, vary greatly within such a thick series.

It will be of importance to look for specially characteristic minerals which may characterise a certain petrographic supply province. In order to solve these questions there should be one or two parties which, in some good sections, should collect samples of the different clastic rocks from well delimited horizons. Such collecting would not be attended with any very great difficulties and should not occasion overwhelming expenses. The travelling conditions are not difficult.

The samples should be subjected to thorough petrographic and mineralogical examination; characteristic minerals should, in addition, be studied by geochemical methods because several accidental elements have proved to be characteristic of certain petrographic provinces.

2) Until the recent development of air transport Peary Land and Grant Land were among the most inaccessible tracts of the world. It will therefore be understood that an examination of the few specimens which were brought home could give no adequate idea of the conditions. Hence we know very little about the petrography of these regions (cf. KOCH 1920, 1923 a, b, 1924, 1925, 1928, 1940). New and careful collecting is accordingly required. To carry out this task at least two parties with a highly rationalised technique of collecting would be required; one party would have to collect samples along the coasts of Peary Land, the other along the coasts of Grant Land.

Two types of programme may be worked out, a minimum programme and a more comprehensive programme. Both may, of course, be varied. An example of each type will be given below, centred round the problem under discussion.

a) The one-season-programme

aims at providing the materials required for a comparison with the sediments collected on Spitsbergen and at supplying us with a more extensive knowledge of De Geer's line.

In the collecting work according to the minimum programme the following technique may therefore be used: Travelling along the shore, one may collect samples from moraines and other Quaternary deposits. In this way a general view of larger areas will be obtained in a very short time than if samples are only taken from the nearest mountains[1]). In addition to this collecting work it will be necessary to photograph the regions from the air in order to determine the basins of supply of present and earlier glaciers or water courses; in this way it is possible to limit the area of occurrence of the minerals and rocks to the basins and to draw a sketch map of the regions of supply, similar to those of East Greenland published by Hübscher and Kleiber (Hübscher 1943, Kleiber 1944). The sedimentary-petrographical investigations in East Greenland and South Greenland combined with air photography have already given very valuable information about the distribution of the clastic rocks under Greenlandic conditions. Through collections and studies we have gained an idea of the glacial, glaciomarine, fluvioglacial, fluviatile, and æolian possibilities of dispersal (Bøggild 1904, 1907, Crommelin 1936, Vroman 1942, Hübscher 1943, Kleiber 1944, von Moos 1938, von Moos & Müller 1939, Stewart 1930, Edelman 1939). In the present case the investigations should not be limited to the sands, but should also include all other rock fragments (as to another aspect of the problem, cf. p. 27). Thus we would not only obtain information about which materials Peary Land and Grant Land may supply, but also learn from which hydrographic basins they are derived.

If the main object of the expedition is to answer the question of the connection between Spitsbergen and Peary Land—Grant Land, some other information for a judgment of the conditions prevailing along De Geer's line must be secured. Some of the most important

[1]) Journey and transport of the samples will be much facilitated by the many practical devices which were invented and tested during the war, e. g. boats of plastic, which are so light that they can easily be dragged across the ice, and in addition so strong that they can be let down on to the sea by parachutes. It is not intended to treat the practical aspect of the investigation here.

points are the depth conditions in Lincoln Sea, that is, the depth of the sea between Peary Land and Grant Land. So far only very little information is available about this sea area and still less about the continental margin within this sector. Are there any fissures in the shelf? Are there any submarine canyons in the continental slope?

The submarine morphology may give much valuable information. By means of a modern echo sounding apparatus a good general idea of the conditions might be obtained in a short time either from the ice or, by the aid of a small seaplane, from holes in the ice. If work from the ice is preferred, the journey should probably be made in the period in which it is possible to travel by sledge. The technique within the different sectors should be adapted to the prevailing conditions as we know them now (Koch 1940, 1945).

While the echo sounding technique is still comparatively simple, the next step, the collecting of bottom samples, involves greater difficulties, though it will be possible in shallow water even for a small party.

The most interesting point for an estimation of the continental margin, the seismic investigation of the shelf (Ewing 1937 a, b, 1939), is probably not yet so well developed instrumentally that it can be used on polar expeditions of this type. Such methods should first be tested on the coast of East Greenland where the conditions for transport by ship are much easier; the results obtained there might give some indications as to the conditions in other sectors.

The minimum programme may be carried out by two to four collecting parties and one or two sounding parties. The parties should be transported to and from their fields of investigation by aeroplanes. It seems most practical to have a base which might be pushed far northward through Smith Sound. The distance from the base to the north coast of Grant Land would then be about 500 km, while the distance to Peary Land would be 800—900 km. A base on Spitsbergen would lie at a much greater distance, but would have the advantage that it could be reached much earlier by ship.

The minimum programme may be carried out by well equipped and trained parties on one summer expedition and would present the advantage that a stay during the winter would not be required.

b) A more comprehensive programme.

A more comprehensive programme requires wintering[1]) of the parties in the regions to be investigated. In addition to the aforementioned

[1]) In the last few weeks newspapers report on a new network of weather observation stations to be established by the American Weather Bureau in the regions dealt with here. A main station with an aerodrome will be situated at

collecting work such an expedition should undertake tectonic and stratigraphic investigations. These would be of the greatest significance in these remote parts and are necessary for an understanding of the structural conditions around the Polar Sea. A knowledge of the Palæozoic and older formations and their tectonics and metamorphoses would enable us to make a more detailed comparison with the conditions on Spitsbergen.

In such a case the wintering station or stations should be so placed that the journeys and the transport of the collections would be as easy as possible.

In the investigations of the structure the following procedure would be very useful: During the first year the minimum programme should be carried through, i. e. the collecting of samples and photographing from the air. The samples collected should be transported home in the autumn and in the course of the winter be subjected to a first examination in the laboratories; the result of this examination should be conveyed over the wireless to the wintering teams, which may then plan the journeys to be made in the spring. Air photographs should be in the possession of the wintering teams already in the autumn. The maps constructed on the basis of the air photographs should be available before the time of travel. The most important visible landmarks should be inserted on the maps; these are of invaluable help in these tracts where the compass cannot be used to the same extent as farther south. Without fairly exact determinations of the direction it is impossible to obtain reliable tectonic results.

The division of the work into two seasons along this line has many advantages: As long as the geologic work in the polar regions consisted in collecting fossils and rock samples, and the stratigraphic determinations were made on the basis of the relation to the overlying and underlying rocks, a fairly simple travelling technique was possible. It is less simple when the tectonics are to be studied where the spacial conditions and directions over vast stretches are to be observed, and when it is in most cases necessary to look for key-areas. In areas previously investigated we too often had to grope our way towards the key-areas, whose topography had first to be cleared up.

Etah, with substations in western Peary Land and the eastern and western parts of Grant Land. The travelling technique will be much simplified if it is possible to use these stations.

As a result of the development of the flying technique it will probably no longer be necessary to pass the winter in the Arctic. The travelling parties may go home by airplane in the autumn and be transported up again in the spring. In that case the members must pass through a thorough training during part of the winter, since otherwise they will be unable to carry out the journeys in the spring under the very exacting conditions.

By this procedure not only the planning of the journeys is simplified, the work is rendered more rational, and less time is lost, but there is also the great advantage that one will be sure to bring home some connected results. If conditions permit the minimum programme to be carried out during the first year, a completed work will have been secured, even if the conditions should prove less favourable in the second year and the whole programme cannot be carried through. If the programme of the first year is not completed, owing to the natural conditions, it may be completed next year. If, however, both kinds of investigations are made in both seasons, one runs the risk of not completing any of them, and it is very doubtful whether tectonic investigations made without any plan will reach the key-areas which are characteristic of each orogeny.

A more detailed working plan must, of course, be adapted to the scope and organisation of the expedition. If the expedition is to be joined by other scientists (geophysicists, meteorologists, botanists, zoologists, etc.), a compromise between the various requirements must be found, which will mainly affect the distribution of the means of transport and the position of the wintering station. We shall not, therefore, enter further into the projects, but answer some more questions which will, no doubt, be put before a large and costly expedition is equipped. Intending members will, no doubt, wish to know whether the programme proposed will give results of general importance beyond the local conditions. A discussion will show us which points deserve special attention and may accordingly be helpful in the preparation of the more detailed working plans. Hence it would seem useful to put forward some conclusions which may possibly be drawn from the investigations. It is obvious that the results cannot be foretold; but if we ask a question of nature, the answers may be divided into two categories:

a) the two petrographic provinces correspond to each other;
b) the two petrographic provinces do not correspond to each other.

a) The two petrographic provinces correspond to each other.

If Peary Land and Grant Land and the synclinorium of Spitsbergen exhibit mineral assemblages which correspond to each other, there is a great probability that one of them has been the area of supply to the other. We may, of course, continue to have some doubts, maintaining that the Scandic land-mass was composed in the same way as the above-mentioned ones. It is therefore necessary to mention this possibility: If a Scandic land area existed, part of it must have been made up of a Caledonian substratum, probably covered with some younger formations. It must accordingly resemble the Hecla Hook formation and its metamorphic correlate. For the Caledonian orogeny is not limited to Spitsbergen, no boundary being found on Prince Charles Foreland. On the other hand the land (if it did exist) must have been built up of some of the East Greenlandic Caledonides, this whole orogenic zone striking seawards and northwards from Christian X's Land. If we like, we may assume, within these Caledonian zones, the presence of an older nucleus, but this will be absolutely hypothetical. It is probable, therefore, that not only the Caledonian substratum will be decisive, but also younger eruptives, if such are present, and older massifs. The sediment-petrographic investigation must accordingly be as differentiated as possible. Geochemical methods and researches on the radioactivity should be combined with the mineralogical technique.

HOLTEDAHL and FREBOLD have repeatedly pointed out that a bed-rock like that of East Greenland cannot have supplied the clastic materials found on Spitsbergen; unfortunately, however, these authors do not state the grounds on which their assumption is based.

Most authors, geologists as well as biologists, assume a young Tertiary or early Pleistocene age for the separation of Greenland and Europe. The majority of them do not give their reasons; it would seem as if they assumed it intuitively. Within the biological section the botanist P. GELTING (1941) is one of the few exceptions, as he puts forward very detailed arguments. It would be highly desirable to obtain some facts which would enable us to establish the age of the movements. There is a possibility of obtaining some indications, if the mineral composition of the Tertiary deposits of Spitsbergen varies in a certain way, permitting us to follow the sliding away of the area of supply.

Other significant chronological data can be established in another way: If the Caledonides on Spitsbergen and in Northern Greenland form a unit, the well-known geological development of Spitsbergen (ORVIN 1940) may be used in a more detailed investigation of the little known, but no doubt rather complicated, history of the first-mentioned areas. Notably in the polar tracts it is very useful to have some guidance when we make the preparations for the investigation. The conjecture of a single-phased tectonic structure of Peary Land, still adhered to in some circles (i. e. only Caledonian or only younger), must be dropped.

Let us look at some of the connections that would be established if a connection between North Greenland and Spitsbergen can be ascertained on the basis of the drift theory:

The shallow water at the mouth of Independence Fjord and Danmarks Fjord would correspond to the depression of the Barents Sea north of Norway. The key area within the Scandic, the area around the large fjords in East Greenland, would lie against the shelf outside the North Sea. This position might provide some clues for unravelling some very intricate problems[1]).

One of the most important problems is the connection between the various fragments of the Caledonian folding within this sector. The Peary Land—Grant Land part of the Caledonian folding zone is undoubtedly only a fragment with an organic connection with the foreland, but with younger boundaries towards the sea which cut across the ancient structures. How closely this fragment stands in relation to the orogenic organism, it is impossible to say, too little of its structure being known. As repeatedly pointed out (WEGMANN 1935, 1939 a, b), the Caledonian district in East Greenland is no complete orogenic unit, but only part of such a unit. The visible part is obliquely cut off in such a way that to the south there is a zone nearly 350 km broad, it grows narrower towards the north and according to the investigations of EIGIL NIELSEN (1941) is barely 100 km broad at Ingolfs Fjord. If we assume (it has not, of course, been proved) that the zone of deformation to the north was just as broad, the belt corresponding to Liverpool Land (at present the innermost visible area within this sector) would be situated midway between Nordostrundingen and Spitsbergen. If, with some authors, we would connect the Caledonides of Northeast Greenland with those of Spitsbergen, the curvature of the connecting link would be fairly sharp.

If, however, we accept the drift hypothesis, this sharp curvature is avoided. The innermost Caledonian fragment (if we consider the mountain range and not the present topography) is Liverpool Land. There is nothing to indicate that we are here near the eastern margin of the deformation segment, on the contrary there is every indication of its being a fragment of the central zone (cf. KRANCK 1935). The actual margin of the orogenic zone must accordingly have been situated much farther eastward. The southern and western boundaries are not yet known. The zone disappears beneath the basalts of Blosseville Kyst towards the south, below the inland ice towards the west.

The Scandinavian Caledonides likewise only represent a fragment. They are also cut off obliquely, but in a different way. The various folding and thrust masses form large festoons, which relieve one another. Their southern branches, in part complicated by an axial culmination, strike towards the continental margin. The margin of the northern zone between Folla (in the region of Namdal) and the region of Grong is the trace left by an axial upheaval. The western margin of the Trond-

[1]) This is, of course, no argument in favour of the said connections; it is only one more reason for our endeavours to get the whole connection cleared up.

heim field between Surnadal and Vaage is likewise the vestige of an axial upheaval; thus the folds do not run along this margin but intersect it obliquely or at right angles. The thrust masses of the Sogne-Jotunheimen region are likewise bounded on the west by the Vaksdal culmination before descending towards the west, just like the Trondheim zones on the west coast after passing the large crystalline area representing a broad axial culmination. These culminations, disposed more or less north-south, constitute one of the most characteristic features within the Scandinavian Caledonides (WEGMANN 1929). They are more or less parallel with the Svalbard branch on the one hand and with very important features in the ancient basement of Fennoscandia.

The zones mentioned above have not the same direction as the whole of the Scandinavian Caledonides. The symmetry of the Caledonian segment is therefore a very marked triclinic one, especially south of the Lierne. This is partly due to the interference of the Caledonian deformations with the structures of the older basement, partly to the different movements corresponding to different phases, partly to the intersection of the folds with the metamorphic fronts.

The structure of the zones is likewise triclinic. The two wings of the axial depressions and culminations are different, hence the geometrical analysis is not so easy that the tectonic coordination is evident at first sight. The clue of many problems of the Scandinavian Caledonides will be obtained by surveying in more detail the spatial relations of the axes, not only of the zones but also of the elements composing them.

The interference between the large axial culminations (in addition there are a number of minor ones) and the relieving arcs also characterises part of the morphology of the shelf, as is evident from HOLTEDAHL's excellent bathymetrical chart (HOLTEDAHL 1940).

Thus the East Greenlandic and the Scandinavian Caledonides are obliquely cut off fragments. In East Greenland the outer zones (nearest the foreland) extend right out to the sea-shore in the north; in Scandinavia towards the southwest. The axial depression on Christian X's Land may correspond to that of the North Sea. The whole of Northeast Greenland, Bear Island, the northern zone of Scandinavia, Smölen outside Tröndelag, and Northwestern Scotland belong to the Arctic-Scandinavian facies district (ØPIK 1939). However, this division into facies can give no certain clues for a judgment of the drift hypothesis, facies limits being very "elastic" configurations. It is probably advisable not to indicate their earlier course until the position of the land masses has been determined by other methods.

The great West Norwegian culmination is characterised by eclogites and related rocks. The same is the case with Liverpool Land (BACKLUND 1936, KRANCK 1935, SAHLSTEIN 1935). In East Greenland structural conditions have been observed which in direction and external shape resemble the large culminations in Norway (WEGMANN 1933, 1939 b). Unfortunately their internal structure in Scandinavia, though well investigated petrographically, is very little known so far.

From a fixistic point of view as well as from that of the drift hypothesis, the connection with the British fragment will not be easy to make out as long as we have no guiding lines in the intermediate sea-covered area[1]). In contrast to the other fragments the British fragment is cut off more transversely to the strike. The so-called northwestern foreland in Scotland and on the Hebrides possibly constitutes the margin of an intermontane area (Zwischengebirge) and need not be the definitive margin of the whole Caledonian folding zone. Such intermontane areas must also be assumed to have existed in other parts of the Scandic.

The structure of the Caledonian mountain range is, no doubt, not simpler than that of the Altaides in Eurasia and America, which has gradually proved to be very complicated. Evidence of this is supplied i. a. by the intermontane molasse basins which were already compared by BUBNOFF with the Saar basin and similar formations within the Hercynian orogenic zone.

It is very instructive to compare orogenic zones of different ages, but the result should not be that everything is forced into the same alpine scheme, as is often done, especially by Alpine geologists. Our preference of the methods which were best developed in the Alps, should not make us assume that the structure is the same everywhere. Each investigation may yield new models and logic possibilities; their number is increased every year, so new structural classifications will soon be required. The greater the number of models and logical possibilities at our disposal, the easier will it be, by approximation and intersection, to grasp the special structural features. One of the shrewdest structural geologists, M. FALLOT, has recently warned against too extensive generalisations with regard to orogenic segments (FALLOT 1944); he writes "Au lieu de vouloir chercher dans les Altaides une réplique, ou mieux une préfiguration des Alpes, le tectonicien objectif doit au contraire, constater que les deux édifices diffèrent par leur essence même."

According to the drift hypothesis the fragments of the Caledonides will show the following relations: The fragment of Peary Land—Grant Land in connection with the fragment of Spitsbergen forms a segment which is situated between the Greenlandic shield and the Barents shelf; it continues towards the south in another branch formed by the Scandinavian, British, and Greenlandic fragments, which form

[1]) It is to be hoped that geophysical investigations in the North-Sea will determine the anisotropy-axes of the underground.

a segment between the Greenlandic shield and Fennoscandia. Minor branches spring off in several places, thus, for instance, in northern Norway the Varanger-Murman branch, which is probably a mantle fold (plis de couverture) which has been sheared from its basement (WEGMANN 1929).

Investigations of the relationship between Peary Land—Grant Land and Spitsbergen will accordingly also be of great importance for a coordination of the other Caledonides and their covering formations. The fragmentation of the ancient structures through oblique intersections seems to be a very common phenomenon which may be found in younger chains also. It is a very important point for the understanding of the relation between older and younger structures.—According to the fixistic hypothesis the whole distribution of the Caledonian fragments is more complicated, but there are also some good grounds for this interpretation. In that case the presence of several intermontane massifs must be assumed. Even according to the drift theory the Caledonian deformations constitute a broad and complicated zone which can be compared with the whole breadth of the Mediterranean orogenic zone from the northern edge of the Alps to the African marginal zone. This zone is of another order of magnitude than the Alps. For the interpretation of the drift hypothesis, also, the whole Caledonian deformation segment around the Scandic will be broader than the Tertiary Mediterranean orogenic zone. For the fixism it has a breadth which may amount to 2000 km. In land areas a breadth of only some few hundred kilometres is known, i. e. less than one-fourth. Fortunately the fragments are often cut off obliquely. But even if we take this fact into consideration, we know comparatively little, so far, about this large deformation zone. A summary of our present knowledge of this question will be given in another paper.

Although some of these remarks would seem to fall beyond the scope of the present exposé, it seems necessary to call attention again to the large lacunæ which are bound to occur in our knowledge of the Caledonian orogenesis. To many authors the puzzle with the Caledonian fragments is one of the proofs of the correctness of the drift theory. It is therefore of importance to show that this method cannot be regarded as a proof; on the contrary, we must later on draw on the drift hypothesis or the fixism theory if we are to put together the Caledonian fragments. According to sound logic the proposed investigation will be the first step towards the elucidation of the Caledonian deformations. The results in the Scandic region will also be of the greatest importance for the North American sector.

The Devonian areas of Greenland (BÜTLER 1935 a, b, 1938, 1939, 1940), of Norway (KOLDERUP 1928), and of Scotland (READ

1935, FLETT 1935) would touch each other and thus would have been a unit during the Devonian period, for which the tectonic analyses might be synchronised in a more rational way (BÜTLER 1939, 1940). The Mesozoic area of depression in Northeast Greenland then corresponds to that of the North Sea and the adjacent seas (Anglo-Parisian basin, Belgic, N. German, Danish, and Scanian Mesozoic and Tertiary). The agreement between the Scanian and the East Greenland Rhætic floras already pointed out by HARRIS (1937) would then be more easily understandable[1]). The basalt area south of Scoresby Sund, Iceland, and the younger volcanic and subvolcanic rocks in Scotland and North Ireland form a very compact region. The highest peaks of Greenland, found south of Scoresby Sund, and the vast highland of Jotunheimen would be placed each on its side of the depression represented by the North Sea and Christian X's Land.

The parallelism of some important guiding lines is very remarkable: The two shores of the North Sea are parallel with the St. George's Channel on one side and the Swedish west coast on the other; they are all parallel with the Wyville-Thomson and Iceland threshold and DE GEER's line. The Reykjanes ridge, which extends far southward, bends in about 56° N. lat., 36° W. long.; beyond this bend it is again parallel with DE GEER's line and some of the important tectonic elements on the east coast of America. The lines which are parallel with the east and west coasts of the North Sea were regarded by BUBNOFF (1942) as traces of displacements. In each of these displacements the western block is suppcsed to have drifted southwards in relation to the eastern block.

How the Alpine folds are to be associated with slidings parallel with DE GEER's line, is one of the very interesting chapters, for these deformations more than any others have been studied in detail and have often been interpreted with the drift hypothesis as a starting point (cf. ARGAND 1924). However a discussion of this question would take us too far afield, so it will not be dealt with here.

The most peculiar feature shown by the projection is that the mainland of North America moves southward; such a movement has been demonstrated in California along the San Andreas Rift and other similar dislocations. The land bordering the Pacific moves northward or remains stationary, as shown by REID, BAILEY WILLIS, LAWSON, GUTENBERG, CLOOS, and many others who have studied these move-

[1]) BROTZEN (1945) has shown that the Cretaceous marine ingression into Scania carried with it the same species as are found in the Cretaceous beds on Spitsbergen. The sea came from the north, invading the west side of Fennoscandia and issuing a branch into the depression zone of Scania and Denmark which gradually advanced towards the southeast.

ments. Recent investigations have shown that the movements can be traced back to Tertiary times, possibly to the Cretaceous period, and that the deformation itself has extended over a much longer space of time. Farther northward similar displacements have been ascertained in connection with earthquakes.

Farther southward, in Central America, the folding segment and the volcanic zone present a curve. The North American Pacific ranges and the Andes do not lie on the same meridian. If the Central American arcs are the results of a southward drift (along De Geer's line), it would be possible to coordinate the orogenic movements over greater distances. Movements which might possibly be connected with De Geer's line in the polar regions may be dated by means of the very detailed Neogene and Pleistocene stratigraphy within the Central American sector. If folding, the formation of chains of arcuate shape, and tear faults (Bartlett trough, etc.) follow upon each other, the later phases would correspond to the sliding along De Geer's line. Investigations in the polar tracts and in the tropics would therefore supply indications for stratigraphic coordinations and diastrophisms, if we assume that the drift hypothesis is correct (cf. pl. 2) and that the whole of the continent has been displaced (cf. Staub 1939, 1942).

Within the Asiatic sector the Japanese geologist Tsuboi assumes that the continental (western) side of Japan moves southward relative to the Pacific underground. Bailey Willis also assumed, for the Philippines, movements along a major fault; the continental side moves southward relative to the Pacific side.

Whether these movements should be interpreted as connected, so that both the Eurasiatic continent and the American continent move southward (cf. pl. 2), or whether thay are regional phenomena which cannot be combined, the future will show. It is important to point out possible connections. In all these problems De Geer's line forms a key point. The demonstration of drift along this line will throw fresh light on a number of earlier observations and warrant new stratigraphic and tectonic (diastrophic) correlations.

2) The provinces do not correspond to each other.

It might perhaps be observed that the whole programme has been prepared for the solution of one question only and that a negative result will not be very satisfactory. To this we may answer that if the two provinces do not correspond to each other, it is no negative result, but, on the contrary, something positive to the adherents of fixistic hypotheses. A decision for or against the drift hypothesis will be one of the greatest advances within geology. *In the present state of affairs all conscientious geologists will always follow two* lines of interpretation, a drift-hypothetical and one or more others. If, now, one of these hypotheses can be eliminated, it must be regarded as a step forward.

We are therefore compelled to choose among the many fixistic hypotheses. A critical review of all of them would be beyond the scope of the present paper. A large number of them have not been fully considered and contain inconsistencies. They may be divided according to several principles. Beginning with the geometrical, we obtain:

1) Hypotheses which explain the deformations as being due to more or less regional conditions;
2) Hypotheses comprising the deformations all over the globe; the movements are supposed to be almost contemporary and to have taken place within strictly limited periods of time.

The latter group, again, comprises very different types:

a) Hypotheses which imply the unchangeableness of the position of the poles (and accordingly the present skeleton of the map). These hypotheses are characterised by their classification of the deformations all over the globe according to the angles which they form with the local meridians. The position of the poles and the Equator must accordingly also play a dynamic role.

b) Hypotheses for which the present skeletons do not play the same role, e. g. pulsation hypotheses (GRABAN 1934, BUCHER 1933, and others) or lineament tectonics (HOBBS 1921, SONDER 1938, 1939, 1941). SONDER constructs large circles on the sphere which he call "zonals" (e. g. the Red Sea—Iceland zonal). DE GEER's line would then be a very important "zonal". If we glance at our projections, it is easy to ascertain that a direction which in a certain latitude is east-west, in another latitude is parallel with a NE—SW direction or with a NW—SE direction on the opposite side of the sphere. For lineament tectonics our projections would accordingly be of great value.

If we classify the hypotheses according to these and some other principles, it will be possible to arrive at the points which may be verified in future investigations. As long as the hypotheses are metaphysic, that is, with as little and as remote a contact with observable phenomena as possible, they can neither be proved nor disproved, and they will have to be trailed along in the baggage of science from year to year. Only by, so to speak, mapping them and developing them into their concrete consequences can we either show them to be absurd or verify them.

Hence an investigation of DE GEER's line (or "zonals", cf. SONDER) will be of great value because it will bring out the metaphysical hypotheses to the light of day.

But apart from the fundamental questions, information about a great many questions of general and regional significance will be obtained. It will therefore be superfluous to deal with any of these points: If there is a fundamental difference between the petrographies of the two areas, the drift hypothesis in position 3 (cf. p. 23) will be rejected. If (according to the drift hypothesis) we accept position 1 (cf. p. 18), East Greenland would be the area of supply of the clastic material. This region is known from a number of investigations (BØGGILD 1904, 1907, HÜBSCHER 1943, KLEIBER 1944, VON MOOS 1938, and MÜLLER 1939). Thus it should be possible to compare clastics from this area with the sediments from Spitsbergen. But there will be an even greater number of new hypotheses. This is the great difference between the two cases. The many hypotheses which do not assume a continental drift will not be mentioned here, but only some branches of geology within which the observations may be of importance.

In the case just mentioned it must be assumed that the Scandic land subsided below sea-level. If this is possible within this sector, it will, of course, also be possible in other sectors in spite of the many theories according to which it seems impossible. The scientists who maintain that they cannot imagine the subsidence of such large land surfaces must then try to to get accustomed to another explanation, if they do not prefer, for the present, to record the fact without any explanation. It will also be necessary to explain DE GEER's line. Here we are confronted with a new case of the phenomenon called by CHAMBERLIN a "four corners of the earth segmentation".

The sediment-petrographical investigations will enable us to form a fairly concrete picture of the petrographic composition of the Scandic land. What the seismic investigations of the sea-bottom cannot give, viz. the qualitative (mineralogic-petrographical) composition, will be elucidated. The seismic studies will give the structural features. These investigations are accordingly the first part of the investigation of the intermediate area between the Scandinavian and the Greenlandic Caledonian segment. Probably the studies will also supply information about the weathering and transport conditions on the Scandic land. The flora of the land has been known since the time of OSWALD HEER and has often been treated since then (NATHORST 1912).

In many cases the mineral composition can also tell us whether the material is derived directly from a crystalline area or whether it has been redeposited one or several times. Interstratified volcanic pyroclasts in the younger series of Spitzbergen are recorded by GRIPP (1937). An investigation of its character would help to complete the picture of the periscandic volcanism.

The history of the Scandic land, as to which numerous hypotheses have been advanced, and which plays a great role on palæogeographical maps, would appear in a new light. Hitherto not many investigations of vanished land-areas of this size (which have supplied sediments of a thickness of several thousand metres) are on record, though studies have been commenced in several places, e. g. Schuchert's studies of the Appalachians, or studies of the Tyrrhenic land-mass which is assumed by many geologists to be situated between Italy, Sardinia, and Sicily.

The question becomes even more interesting on considering the Polar Sea. Many of its coasts are very little known and the discussions on this subject have not been particularly lively. The investigations proposed above (even the minimum programme) would permit a study of many interesting questions. These problems may be grouped under two headings:

a) geological observations;
b) morphological observations.

a) Geological observations.

Owing to its collections of material and its localisation to basins of supply the minimum programme will render it possible to outline a first picture of Peary Land and Grant Land. It will be a mineralogic-petrographic catalogue like many petrographic descriptions. By means of air photographs it will be possible to distinguish various zones which extend through the area and to form an opinion of its structure. It is on this first picture that the tectonic investigation should be based (cf. p. 28).

The results may also be used for another purpose: The present author has previously (Wegmann 1935, pp. 45—46) called attention to an interesting comparison between the clastic material supplied by the Caledonian areas at the present day and that supplied by them during the Palæozoic and Mesozoic times. The programme was partially carried out in East Greenland by Hübscher (1943), Kleiber (1944), von Moos (1938), and Müller (1939). A synthesis of the results obtained has not yet been published. Similar comparisons may be made in the Peary Land—Grant Land sector, in which both Palæozoic, Mesozoic, and Tertiary clastic sediments occur. A comparison with the late-Silurian sandstones (Koch 1937) would be exceedingly interesting as it might help to determine the period during which the metamorphosis of the mountain chain was taking place. Are these sands derived from the interior of the mountain range, and where is it situated? Did metamorphosis in the feeding zone go on prior to the erosion? Another problem is whether any sedimentary formations existing in previous times have now disappeared. Thus an investigation of the present regions supplying clastic material should form the basis of all future studies of the structural development of the sector. To each warping corresponds an extensive transport with subsequent sedimentation. A large warping of this kind must have taken place during the Triassic time, whose deposits are rather thick in the Heiberg Land zone.

Previously folds running NNW—SSE have been observed near Eureka Sound in connection with several faults. These folds are more or less parallel with the Tertiary folds on Spitsbergen if we draw parallel circles along De Geer's line. Thus they form "zonals", according to the nomenclature of Sonder (1939, 1938).

Here special stress has been laid on the petrographic and tectonic problems and their solution; but, of course, a number of stratigraphic problems of great interest also present themselves. The programmes may easily be combined with fossil collecting and stratigraphic studies. The technique is well known and is not, therefore, mentioned here. The hypothesis suggesting that a great many organisms had their places of origin and development in the Polar regions has been mentioned above (p. 20). The Permian problem in the Arctic tracts is still subject to discussion, and each observation may throw new light on this interesting formation, especially in areas situated between the classic occurrences of India, the Ural, and western America. It is a curious fact that the classic regions for the Permian stratigraphy seem to be situated almost parallel with Greenland (see pl. 2), on one side the Urals, on the other Texas. Several other curious symmetrical conditions will appear from the plate, some of which have already been pointed out by Solger (1935). The Triassic and Tertiary deposits probably allow of many new discoveries. It will therefore be possible to insert a great many new stones in the mosaic constituting the geologic history of this region. The post-Eocene development should mainly be reconstructed from

b) morphological observations.

This observational material may be obtained on land areas by direct observations, observations from the air, air photographs, and maps. However, this does not suffice for more detailed studies, for *important phenomena must be found below the level of the sea.*

The northernmost part of Greenland, Grant Land—Grinnell Land, Ellesmere Land, and the adjacent parts of the Arctic archipelage seem to exhibit traces of extensive peneplanation. As in East Greenland and on Spitsbergen it comprises a number of heterogeneous surfaces, the youngest of which are younger than the Eocene. This complex ancient land surface was elevated and again became the subject of intensive erosion. In the western and southern parts of the Arctic archipelago the land surface is not at present elevated very much above sea-level. However, it rises towards the east, to attain altitudes of 1000—2000 m on Ellesmere Land and 3000 m farther northward on Grant Land. The same ancient land surface seems to occur on Heiberg Land at an altitude of about 2000 m. In the highest regions the peneplain is resolved into isolated peaks, while the more extensive portions of the ancient land surface are preserved in the lower areas. Most authors assume that not only the valleys, but also sounds and fjords are old river valleys which have been more or less transformed by glacial erosion. These sounds form a network of waterways between the islands. Lauge Koch (1925, 1928, 1940) is of opinion that Robeson and Kennedy Channels are also ancient valleys within an earlier drainage system which was later submerged.

No direct evidence of the time at which they were formed is on record. The land was elevated some hundred metres above the level of the sea. A similar upheaval and subsidence have been observed within the Siberian sector, where it has been possible (Obrutschew) to determine one phase as Upper Pliocene and early Pleistocene. At that time the Siberian islands were connected with the mainland. If the deep valleys correspond to the Upper Pliocene, the youngest peneplanations and the phases of large upheaval will be of Oligocene, Miocene, and early Pliocene age.

If the old valleys were carved out during a period with a lower sea-level, they may be traced right back to the continental slope. Some of them would perhaps be more or less filled with erratic material, but they should be traceable in broad features just as they have been traced in Norway (Holtedahl 1940).

The outlines of the continental margin will be an important new feature in the history of the Polar Sea. A number of questions of general importance at once arise: Are there submarine canyons on this coast? Are they situated in continuation of the ancient valleys? The discussions on submarine canyons are very lively at present, and a number of hypotheses of their origin have been put forward.

In the areas investigated so far it will perhaps be impossible to distinguish between the factors which are of importance for the genesis of submarine canyons so that none of the hypotheses can be eliminated. Possibly this can be done more successfully in Arctic regions; a comparison between Arctic and other conditions may, no doubt, yield new results.

If we accept the drift hypothesis, the tectonic origin of submarine canyons cannot be directly rejected. If, however, we assume that a warping of the mainland has taken place simultaneously with the subsidence of the ocean basins, their genesis should be explained in quite a different way. The valleys previously cut into a not very sloping coastland may subside between the continents and the deeper basins, assuming an oblique position in the hinge-zone. How they are later preserved, whether by the action of heavy water masses or in other ways, will probably be shown by more detailed investigations.

An investigation which may without any great difficulty be combined with this collecting work is a study of the magnetisation of the basalts. SCHEI found a number of diabases which were post-Triassic, but pre-Tertiary. These diabases are probably also represented on Peary Land; they may possibly correspond to some of the basalts on Spitsbergen. However, they should not be confounded with the diabases of the Thule Formation.

MERCANTON (1926, 1931, 1932) has observed the direction of the magnetism in basalts derived from Iceland, the Faroes, East Greenland, and Spitsbergen and found that it does not correspond to the present magnetic conditions of the earth. The collecting of magnetically orientated and locally determined samples might supply interesting new information. It may perhaps be possible, on the basis of the changing magnetic orientation, to establish a certain sequence in age for the Arctic basalts, for basalts of the same age would have a corresponding orientation. It would be exceedingly useful for the study of the thick basaltic series in East and West Greenland as well as within the whole Arctic region and would enable us to make a more detailed division of the diastrophism.

It is very interesting that the seismic activity is not concentrated along DE GEER's line, but in a zone extending from Spitsbergen over Jan Mayen, Iceland, and the Reykjanes Ridge (GUTENBERG and RICHTER 1941, p. 71, fig. 11).

Summary.

The great problems within modern geology should be solved by team work. This work should be carried out according to well prepared programmes which entrust to the individual specialists their part of the work in order that the object may be attained through a well organised collaboration.

One of the most extensively discussed problems within geology is the reality or non-reality of continental drift, that is, the justification of the drift hypothesis. All possibilities of an effective verification should therefore be regarded as a welcome opportunity to pass from discussion to observation. On that account a research programme is put forward here which should render testing possible in a particularly suitable sector.

The development of the drift hypothesis is briefly outlined (pp. 6—10) and a distinction is made between the different standpoints

possible. Quite schematically they may be divided into critical, interpretative, and heuristic attitudes. The attitudes correspond to the methods of argumentation. The method most commonly employed is the jig-saw puzzle-method, by which areas now separated are regarded as parts of an earlier unit and are put together like the pieces in a jigsaw puzzle. The gap between the pieces is the present sea bottom. The fact that the substratum of the sea bottom is almost a *terra incognita* plays the most important role in the puzzle-method. All new investigations of the shelves, the continental slopes, and the sea bottom must accordingly influence the puzzle-method (pp. 11—12).

The change in the position of astronomically determined points (if they are real) cannot decide the problem, for at present we cannot be certain whether the changes are of a local, a regional, or a planetary character (pp. 13—16). The investigation of the sea bottom by modern methods will possibly enable us to decide whether the bottom of the sea basins is a gap in relation to many geological formations and structures, or whether it consists of down-faulted blocks.

In a geometrical respect the movements may be divided into 1) parallel displacements a) with an opening between the fracture surfaces or b) along parallel fracture surfaces; 2) rotational displacements of different characters; 3) a combination of the two categories.

If an attempt is made to verify the drift by geologic methods, a displacement parallel with the fracture surface must be found in which the two adjoining parts are above sea-level, that is to say that they are accessible to investigation by the usual geologic methods. Such a case seems to exist in the Arctic, and plans for its investigation form the subject of the second part of the paper.

The hypotheses concerning the genesis of the Scandic and the Polar Sea are divided into two groups: De Geer and his followers assume the existence of a Scandic land-mass which subsided in Tertiary times. The adherents of the drift hypothesis regard the Scandic as a gap between Greenland and the European land-mass. They do not agree as regard the earlier position of Greenland in relation to the European block; we may distinguish between three types of position (pp. 18—19). The hypotheses concerning the Polar Sea are less explicit.

The most peculiar feature in Arctic physiography is De Geer's line, which extends northward from Vesteraalen in Norway along the west of Spitsbergen to the Nansen threshold, continuing north of Peary Land, Lincoln Sea, Grant Land and along the continental margin outside the Arctic archipelago to the mouth of Amundsen Sound. If we consider this line, the first part to the left, the bottom of the Scandic, lies at a low level, while the right side is more elevated (Spitsbergen, Bear Island, the Barents Sea), in the second section, however, the right side is low (the Polar Sea) and the left side has become elevated (Peary Land, Grant Land). If we try to interpret the conditions according to the drift hypothesis, De Geer's line will be the vestige of a displacement parallel with the fracture plane.

A method for the construction of a map of such parallel displacements has been put forward (pp. 22—23, pls. 1—2).

Thus according to the drift hypothesis Peary Land and Grant Land play the role of the Scandic land, as they come to lie outside Spitsbergen. It is the Scandic land which has supplied the Mesozoic and Tertiary sediments. The sediment-petrographic and geochemical examination of these sediments would enable us to form an idea of the qualitative composition of the rocks of the Scandic land. The question would then be whether or not this composition corresponds to that of the rocks of Peary Land and Grant Land, so that these lands may have supplied them? There are two possibilities: 1) The petrographic provinces correspond to each other; 2) the petrographic provinces are different.

A plan for the investigation of this question is put forward; it comprises the collecting of samples and investigations on Spitsbergen, on Peary Land, in Lincoln Sea, and on Grant Land. For the latter investigations a minimum programme (one summer expedition) and a more comprehensive programme (extending over two years or more) are given.

The aims of the investigations are incorporated in a larger scheme in order to show which points of the programme deserve special attention; at the same time the connection between the investigated regions and a number of other regions is pointed out and it is thus shown that the solutions of the problems may have far-reaching consequences of a fundamental character. The results will be of importance far beyond the Arctic region, both in the American and in the Eurasiatic sector.

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LITERATURE

ALDINGER, H. 1937. Das ältere Mesozoikum Ostgrönlands. — Geol. Rundschau, Bd. 28, pp. 124—127.

ARGAND, EMILE. 1924. La tectonique de l'Asie. — C. R. XIIIme Congr. géol. internat. 1922, pp. 171—372, 27 figs., 1 map.

BACKLUND, H. G. 1936. Zur genetischen Deutung der Eklogite. Geol. Rundschau, Bd. 27, pp. 47—61.

BAILEY, E. B. and HOLTEDAHL, O. 1938. Regionale Geologie der Erde, Bd. 2, II, 76 pp., 16 figs., 2 pls.

BROTZEN, F. 1945. De geologiska resultaten från borrningarna vid Höllviken. Preliminär Rapport; Del I: Kritan. S. G. U., ser. C. No. 465, Årsbok 38 (1944), No. 7, 64 pp., 10 figs., 4 pls.

BRÆNDEGAARD, JENS. 1946. The Spiders (Araneina) of East Greenland. A faunistic and zoogeographical investigation. M. o. G. Bd. 121, No. 15, 128 pp., 49 figs.

BUBNOFF, S. VON. 1943. Das Südrand Skandinaviens. Geol. Rundschau, Bd. 34 (Backlund-vol.), pp. 197—208, 1 fig.

BUCHER, WALTER, H. 1933. The Deformation of the Earth's Crust. Princeton 1933. XIII + 518 pp., 100 figs.

BULLARD, E. C. 1940. Geophysical study of submarine geology. Nature 145, 1940, pp. 764.

BÜTLER, H. 1935 a. Some new investigations of the Devonian stratigraphy and tectonics of East Greenland. M. o. G. 103, Nr. 2, 35 pp., 17 figs.

— 1935 b. Die Mächtigkeit der kaledonischen Molasse in Ostgrönland. Mitt. Naturf. Ges. Schaffhausen, 12, 3, pp. 11—33, 2 figs., 4 pls.

— 1938. Die tektonischen Strukturelemente des östlichen Moschusochsenfjordes. Vorläufige Mitteilung. M. o. G. 103, Nr. 5, 8 pp, 1 fig.

— 1939. Übersicht der devonischen Bildungen nördlich des Davysunds in Ostgrönland. Mitt. Naturf. Ges. Schaffhausen, 16 No. 5, pp. 105—131, 8 figs.

— 1940. Das devonische Faltungsgebiet nördlich des Moskusoksefjordes in Ostgrönland. M. o. G. 114, Nr. 3, 32 pp., 10 pls.

BØGGILD, O. B. 1904. Samples of the sea-floor along the coast of East-Greenland. M. o. G. Bd. 28, Nr. 2, pp. 18—95, 9 pls.

— 1907. Sédiments sous-marins recueillis dans la Mer du Groenland (in) Duc d'Orléans: Croisière océanographique accomplie à bord de la Belgica dans la Mer du Groenland 1905; pp. 85—98, pls. 60, tables I—IV.

CHAMBERLIN, T. C. 1914. Diastrophism and the formative processes. Part VI. Foreset beds and slope deposits. Journal of Geology, vol. 22, pp. 268—274, 1 fig.

CROMMELIN, R. D. 1937. A sedimentary petrological investigation of a number of sand samples from the south coast of Greenland between Ivigtut and Frederiksdal. M. o. G. 113, Nr. 1, 32 pp., 4 figs., 1 map.

DALY, REGINALD A. 1916. Petrography of the Pacific Islands. Bull. Geol. Soc. Amer., vol. 27, pp. 325—344, 3 tables.

De Geer, Gerald. 1912. Kontinentale Niveauveränderungen im Norden Europas. C. R. XIme Congr. géol. intern. Stockholm 1910, pp. 849—860, 1 map.
— 1919. On the physiographical evolution of Spitzbergen, explaining the present attitude of the coal-horizons. Geogr. Ann. 1, pp. 161—192, 9 figs., 2 pls.
— 1926. Om de geografiska huvudproblemen i nordpolsområdet. Ymer 46, pp. 133—145, 1 map.
Du Toit, Alex. L. 1937. Our wandering continents. An hypothesis of continental drifting. XIII + 366 pp., 48 figs.
Edelman, C. H. 1939. Das Ziel der Sanduntersuchungen auf Grönland. Mitt. Naturf. Ges. Schaffhausen, Bd. 16, pp. 217—220.
Esclangon, Ernest. 1942. Remarques au sujet de la note précédente. C. R. Ac. Sc., t. 214, pp. 559—561.
Ewing, Maurice, Crary, A. P., and Rutherford, H. M. 1937 a. Geophysical investigations in the emerged and submerged Atlantic coastal plain. Part I. Methods and results. Bull. Geol. Soc. Amer., vol. 48, pp. 753—802.
— and Miller, Benjamin. 1937 b. Geophysical investigations in the emerged and submerged Atlantic coastal plain. Part II: Geological significance of the geophysical data. Bull. Geol. Soc. Amer., Vol. 48, 6, pp. 803—812, 1 fig.
— Woollard, George P., and Vine, A. C. 1939. Geophysical investigations in the emerged and submerged Atlantic coastal plain. Bull. Geol. Soc. Amer., vol. 50, pp. 257—296, 16 figs.
Fallot, P. 1944. Tectonique hercynienne et tectonique alpine. Bull. Soc. géol. de France (5) t. 14, 1944, pp. 99—102.
Field, Richard M. 1936. Recent developments in the geophysical study of ocean basins. Amer. Geophys. Union Transactions 1936, pp. 20—23.
— 1937. Structure of continents and ocean basins. J. of the Washington Academy of Sc., vol. 27, 5, pp. 181—195.
Flett, Sir John. 1935. The geology of the Orkneys. Quart. G. S. London, vol. 91, pp. I—II.
Frebold, Hans. 1930. Verbreitung und Ausbildung des Mesozoikums in Spitzbergen nebst einer Revision der Stratigraphie des Jura und der Unterkreide in Nowaya Semlja und ein Entwurf der mesozoischen Entwicklungsgeschichte des Barentsschelfes. Skr. o. Svalbard og Ishavet, No. 31, 126 pp., 8 figs., 33 pls.
Gelting, Paul. 1941. Über pleistozäne Pflanzenrefugien in Grönland. Mitt. Naturf. Ges. Schaffhausen. Bd. 17, 4, pp. 74—96, 5 figs.
Geographical Society. 1944. The geography of post-war air routes. Evening meeting of the Society, 6. December 1943. Geograph. Journ., vol. 103, pp. 89—100, 1 fig., 2 pls.
Grabau, A. W. 1934. Oscillation or pulsation. Rep. XVI Intern. Geol. Congr., Washington, 1933 (preprint), 15 pp., 1 fig.
Gripp, Karl. 1927. Beiträge zur Geologie von Spitzbergen. Abh. des Naturw. Vereins zu Hamburg, Bd. 21, 3, 38 pp., 13 figs., 7 pls.
Gutenberg, B. 1939. The structure of the Pacific Basin as indicated by earthquakes. Science, 90, pp. 456—458.
— 1941 a. Tectonic processes in action. U.S.A. transactions of 1941 of the American Geophysical Union, pp. 556—558.
— 1941 b. Mechanism of faulting in southern California indicated by seismograms. Bull. of the seismological Soc. of America, vol. 31, pp. 263—302, 6 figs.
— and Richter, C. F. 1941. Seismicity of the Earth. Geol. Soc. of America, Special Papers No. 34, VII + 131 pp., 17 figs. (Arctic belt pp. 70—73, fig. 11).
Guyot, Edmond. 1935. La théorie des translations continentales et l'astronomie. Annales Guébhard-Sévérine, 11me année, pp. 37—73, 1 fig.
— 1943. L'emploi d'une projection cylindrique oblique pour l'étude de la dérive

des continents. Bull. Soc. Neuchâteloise des Sciences Nat. t. 68, pp. 105—111, 3 figs.

HÄGG, R. 1927. A new Tertiary fauna from Spitzbergen. Bull. Geol. Inst. Upsala, vol. 20, pp. 39—53, 4 pls.

HAIG-THOMAS, DAVID. 1940. Expedition to Ellesmere Island, 1937—1938. Geogr. Journ. 95, 4, pp. 265—277, 10 figs.

HARRIS, T. M. 1938. The fossil flora of Scoresby Sound, East Greenland. Part 5: Stratigraphical relations of the plant beds. M. o. G. 112, 2, 114 pp., 5 figs., 1 pl., 3 tables.

HINKS, A. R. 1940. Maps of the world on an oblique Mercator projection. Geogr. Journ. 95, 5, pp. 381—383, 1 map.

HOBBS, WILLIAM HERBERT, 1921. Earth evolution and its facial expression. XVII + 178 pp., 84 figs., 6 pls.

HOLMES, ARTHUR. 1933. The thermal history of the Earth. Journ. of the Washington Acad. of Sc. 23, 4, pp. 169—195, 15 figs.

HOLTEDAHL, OLAF. 1940. The submarine relief off the Norwegian coast. With bathymetrical map in 7 sheets of the Norwegian coastal waters and adjoining seas. Det Norske Vid.-Akad. i Oslo.

HUANG, T. K. 1945. On major tectonic forms of China. Geol. Memoirs, Ser. A No. 20, IV + 165 pp., 8 maps.

HÜBSCHER, HANS. 1943. Zur Petrologie junger Sande aus Nordostgrönland. M. o. G. 115, 3, 108 pp., 9 figs., 1 pl.

HUMPHREYS, NOEL, SHAKLETON, EDWARD, and MOORE, A. W. 1936. Oxford University Ellesmere Land Expedition. Geogr. Journ. 87, 5, pp. 385—443, ill., 1 map. (Geology, App. I, pp. 427—431, by BENTHAM, R.).

JELSTRUP, HANS J. 1933. Détermination astronomique à Sabine-Øya au Groenland oriental, avec discussion touchant une dérive continentale possible. Skr. om Svalbard og Ishavet, 58, 27 pp., 1 fig.

JENSEN, P. F. 1923. Ekspeditionen til Vestgrønland Sommeren 1922.
1) Stedsbestemmelser ved: Godhavn (Egedesminde, Holstensborg), Kornok, Julianehaab og Godthaab. 2) Grønlands Forskydning. 3) Résumé du traitement des travaux astronomiques-géodésiques effectués au Groenland, au cours de l'été 1922 et décrit dans le présent fascimile, ainsi qu'une réproduction in extenso du chapitre relatif au déplacement du Groenland. M. o. G. Bd. 63, Nr. 3, pp. 205—283, 12 figs.

JONES, THOMAS OWEN. 1937. The exploration of the earth crust. Quart. Geol. Soc. London, vol. 93, pp. LI—XCII.

KLEIBER, KARL. 1944. Beitrag zur Geologie und Sedimentspetrographie Ostgrönlands. M. o. G. 115, 4, 148 pp., 17 figs., 6 pls., 13 tables.

KOCH, J. P. 1917. Survey of North-East Greenland. M. o. G. Bd. 46, Nr. 2, pp. 79—468, 67 figs., 6 pls. (The drift of Greenland in a westerly direction, pp. 240—256, figs. 33—34).

— & WEGENER, A. 1917. Die glaciologischen Beobachtungen der Danmark-Expedition. M. o. G. Bd. 46, Nr. 1, (1911), pp. 1—77, 38 figs. 1 pl.

— & WEGENER, A. 1930. Die wissenschaftlichen Ergebnisse der dänischen Expedition nach Dronning Louises-Land und quer über das Inlandeis von Nordgrönland 1912—13 unter Leitung von Hauptman J. P. Koch. M. o. G. Bd. 75, 676 pp., 389 figs., 8 pls.

KOCH, LAUGE. 1920. Stratigraphy of Northwest Greenland. Medd. fra Dansk Geol. Foren., Bd. 5, Nr. 17, 78 pp., 12 figs., 1 pl., 1 map.

— 1923 a. Resultaterne af Jubilæumsexpeditionen Nord om Grønland i 1921. Naturens Verden, Febr. 1923, pp. 49—67, 4 figs.

KOCH, LAUGE. 1923 b. Preliminary report upon the geology of Peary Land, Arctic Greenland. Amer. Journ. of Sci., vol. 5, pp. 189—199, 2 figs.

— 1924. Rapport I: Kartografi og Geologi (De videnskabelige Resultater af Jubilæumsekspeditionen Nord om Grønland. Geogr. Tidsskr. Bd. 27, 4, pp. 208—218, 5 figs.

— 1925. The geology of North Greenland. Amer. Journ. of Sci. (5), vol. 9, pp. 271—285, 5 figs.

— 1928. The physiography of North Greenland. Greenland, vol. 1, Copenhagen 1928, pp. 491—518, 13 figs.

— 1937. Sur la question de la chaine calédonienne au Groenland septentrional. C. R. Ac. Ac. t. 204, pp. 1299—1301.

— 1940. Survey of North Greenland. M. o. G. 130, 1—2, 364 pp, 56 figs., 21 pls.

— 1945. The East Greenland Ice. M. o. G. 130, 3, 373 pp., 140 figs., 1 pl.

KOLDERUP, CARL FRED. 1928. Das Old Red im westlichen Norwegen. Fennia 50, 23, 17 pp., 7 figs.

KÖPPEN, W. and WEGENER, A. 1924. Die Klimate der geologischen Vorzeit. IV + 255 pp., 41 figs., 1 pl.

MALMQVIST, DAVID. 1932. Zur Kenntnis der oberkarbonischen Sedimente der westlichen Clavering Insel, Ostgrönland. M. o. G. 94, 6, 28 pp., 3 figs., 1 pl.

MERCANTON, P.-L. 1926. Inversion de l'inclinaison magnétique terrestre aux âges géologiques. Soc. Suisse de Géophys. Mét. et Astr. Fribourg 1926, pp. 345—349.

— 1931. Inversion de l'inclinaison magnétique aux âges géologiques. Nouvelles observations I. C. R. Ac. Ac. t. 102, pp. 978—979.

— 1932. Inversion de l'inclination magnétique aux âges géologiques. Nouvelles constatations II. C. R. Ac. Sc. t. 194, pp. 1371—1372.

MOOS, ARMIN VON. 1938. Sedimentpetrographische Untersuchungen in Ostgrönland, M. o. G. Bd. 103, 4, 76 pp., 4 figs.

— and MÜLLER, A. 1939. Sedimentpetrographische Untersuchungen im Devon von Kongeborgen in Ostgrönland. Mitt. Nat. Ges. Schaffhausen, Bd. 16, pp. 138—145.

MURRAY, HAROLD W. 1941. Submarine mountains in the Gulf of Alaska. Bull. Geol. Soc. Amer. 52, p. 333.

NATHORST, A. G. 1912. Sur la valeur des flores fossiles des régions arctiques comme preuve des climats géologiques. C. R. XIe Congr. géol. intern., Stockholm 1910, pp. 743—756, 4 figs.

NIPPOLDT, A. 1935. Die Magnetisierung der Erde unter den Ozeanen. Terrestrial Magnetism, 40, pp. 129—138, i fig.

ÖPIK, A. 1939. Paläontologie, Arctisforschung und Kontinentalverschiebung. Mitt. Naturf. Ges. Schaffhausen, Bd. 16, pp. 47—69, 2 figs.

ORVIN, ANDERS K. 1940. Outline of the geological history of Spitsbergen. Skr. om Svalbard og Ishavet, No. 78, 57 pp., 12 figs., 3 pls.

PICKERING, W. H. 1907. The place of origin of the moon. — The volcanic problem. Journ. of Geol., vol. 15, pp. 23—38, 4 figs.

PIGGOT, CHARLES SNOWDEN. 1938. Core samples of the ocean bottom and their significance. The Scient. Monthly, vol. 46, pp. 201—217, 9 figs.

READ, H. H. 1935. The Grampian Highlands. Geol. Survey and Museum. Brit. Regional Geology, VI + 81 pp., 25 figs., 10 pls.

SAHLSTEIN, TH. G. 1935. Petrographie der Eklogiteinschlüsse in den Gneisen des südwestlichen Liverpool-Landes in Ost-Grönland, nebst Anhang: Granulitartiger Gneis nordöstlich von Kap Hope. M. o. G. Bd. 95, Nr. 5, 43 pp., 5 figs., 1 pl.

SOLGER, F. 1935. Grossformen in Norddeutschland. Zschr. d. deutsch. Geol. Ges., Bd. 87, 1, pp. 1—21, 9 figs.

Sonder, Richard A. 1938. Die Lineamenttektonik und ihre Probleme. Ecl. geol. Helv., vol. 31, 1, pp. 199—238, 12 figs.

— 1939. Zur Tektonik des Atlantischen Ozeans. Geol. Rundschau, Bd. 30, pp. 28—51, 1 fig.

— 1941. Studien über heisse Quellen und Tektonik in Island. Publ. herausgegeben von der Stiftung "Vulkaninstitut Immanuel Friedländer", No. 2, 132 pp., 2 figs., 13 pls.

Staub, Walther. 1939. Ost-Mexiko, das Nord-West-Ende der mediterranen, orogenen Zone. Geol. Rundschau, Bd. 30, pp. 346—351, 2 pls.

— 1942. Nord- und Südamerika als Westteile von Eurasien und Afrika. Mitt. Naturf. Ges., Bern, 1941, pp. 104—110, 3 figs.

Stewart, D. 1930. Analyses and derivation of two beach sands from Holstensborg district of Greenland. American Mineralogist 15.

Stoyko, Nicolas. 1942. Sur les variations séculaires et périodiques des longitudes. C. R. Ac. Sc. t. 214, pp. 558—559, 1 fig.

Suess, Franz Eduard. 1938. Der Bau der Kaledoniden und Wegener's Hypothese. Zentralbl. f. Min. usw., Jahrg. 1938, Abt. B, pp. 321—337, 1 fig.

Taylor, F. B. 1910. Bearing of Tertiary mountain belt on the origin of the earth's plan. Bull. Geol. Soc. Amer., vol. 21, pp. 179—226, 7 figs., 1 pl.

Teall, J. J. H. and Newton, E. T. 1897. Notes on a collection of rocks and fossils from Franz Joseph Land, made by the Jackson-Harmsworth Expedition during 1894—1896. Q. Geol. Soc., London, vol. 53, pp. 477—519, 4 figs., 5 pls.

Vening Meinesz, F. A. 1939. De Theorie van Wegener. Tijdschr. v. Aardr. Gen., Amsterdam (2), 56, 4, pp. 453—457.

Wegener, Alfred. 1912 a. Die Entstehung der Kontinente. Petermann's Mitt. Jahrg. 58, 1, pp. 185—195, 253—256, 305—309, 7 figs.

— 1912 b. Die Entstehung der Kontinente. Geol. Rundschau, Bd. 3, pp. 276—292, 3 figs.

— 1915. Die Entstehung der Kontinente und Ozeane. Sammlung Vieweg 23. IV + 94 pp., 20 figs.

Wegmann, C. E. 1925. Note préliminaire sur le profil en long de la chaîne calédonienne scandinave. C. R. somm. Soc. géol. Fr., pp. 225—226.

— 1929. Zur Kenntnis der tektonischen Beziehungen metallogenetischer Provinzen in der nördlichsten Fennoskandia. Zschr. f. prakt. Geol., Jahrg. 37, pp. 193—208, 3 figs.

— 1935. Preliminary report on the Caledonian orogeny in Christian X's Land. M. o. G., Bd. 103, 3, 59 pp., 11 figs., 3 pls.

— 1938. On the structural divisions of Southern Greenland. M. o. G., Bd. 113, 2, 148 pp., 70 figs., 7 pls.

— 1943 a. Sur un contrôle géologique de la dérive des continents. Bull. Soc. Neuchâteloise des Sc. Nat. t. 68, pp. 97—104, 1 fig.

— 1943 b. Zur Feststellung von Landverschiebungen in der Arktis. Geol. Rundschau, Bd. 34 (Backlund-vol.), pp. 236—242, 1 pl.

Wright, John. 1940. South-East Ellesmere Island. Geogr. Journ. 95, 4, pp. 278—291, 6 figs., 1 map.

Wüst, Georg. 1941. Relief und Bodenwasser im Nordpolarbecken. Zschr. f. Erdkde, pp. 163—180, figs. 17—20, 1 map.

Færdig fra Trykkeriet den 25. November 1948.

Plate 1.

Ocean depths: 1000 m, 2000 m, 3000 m, and 4000 m.

Map of the arctic regions. European and American sectors.
Oblique cylindrical projection (cf. GUYOT 1943).
Horizontal lines are parallel with DE GEER's line.

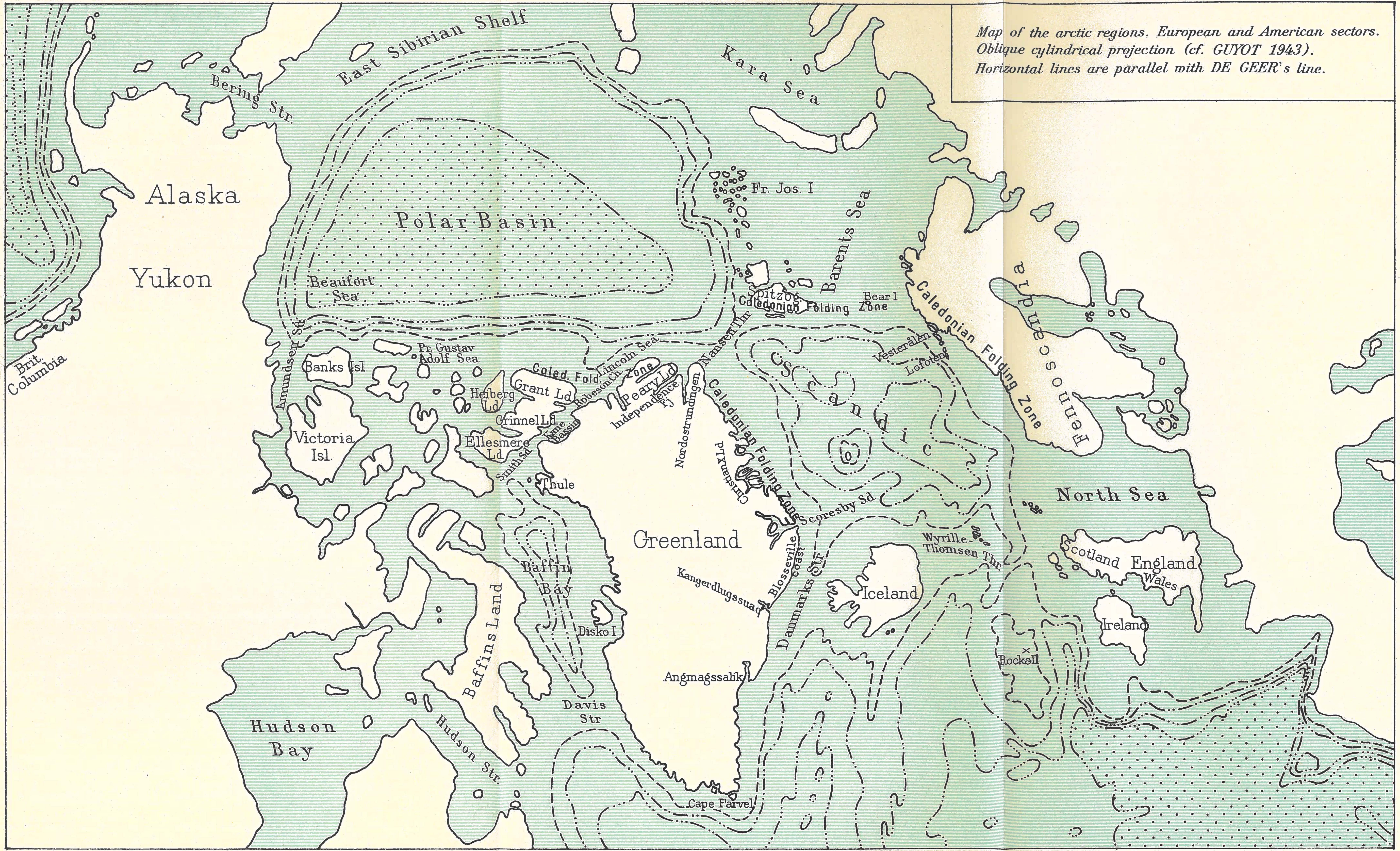

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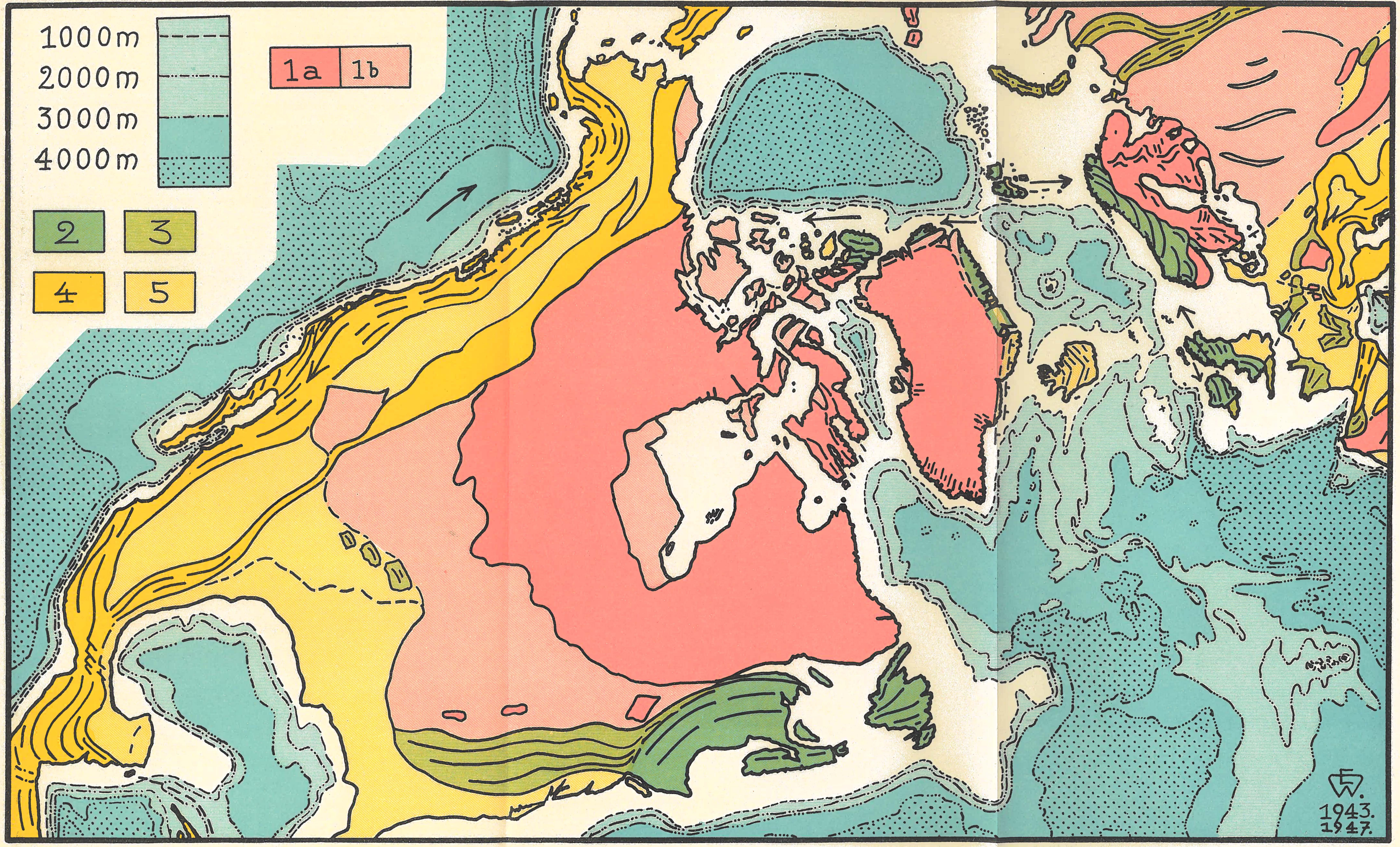
Plate 2.

Geological Map based on the oblique cylindrical projection (cf. GUYOT 1943). Horizontal lines are parallel with DE GEER's line.

1 a = orogenic zones of pre-Cambrian age levelled at different times of the pre-Cambrian period.
1 b = tabular sediments covering levelled pre-Cambrian chains.
2 = orogenic zones of the Caledonian cycle.
3 = orogenic zones of the Hercynian cycle.
4 = orogenic zones of Mesozoic and Tertiary age.
5 = tabular sedimentary formations on levelled chains of Caledonian, Hercynian, and younger age.

Ocean depths in metres.



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