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LOG Id: LOG_0011
LOG Titel: Iceland : Evolution, active tectonics, and structure. A preface
LOG Typ: article

Übergeordnetes Werk

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J. Geophys. 47, 1–6, 1980

Journal of Geophysics

Iceland: Evolution, Active Tectonics, and Structure. A Preface

W.R. Jacoby[1], A. Björnsson[2], and D. Möller[3]

[1] Institut für Meteorologie und Geophysik, Universität, Feldbergstr. 47, D-6000 Frankfurt 1, Federal Republic of Germany

[2] Orkustofnun, Grensásvegur 9, Reykjavík, Iceland

[3] Institut für Vermessungskunde, Technische Universität, Pockelsstr. 4, D-3300 Braunschweig, Federal Republic of Germany

Introduction: Iceland and Wegener

Iceland is a unique phenomenon, land raised high where ocean should be. Neither Wegener's concept of continental drift nor its modern version of sea-floor spreading and plate tectonics leave space for a large land mass in the middle of the North Atlantic. This problem and the promise it offers to the study of the earth presented themselves to Wegener (1915, 1920, 1922, 1929) when he attempted to reconstruct the pre-drift positions of the continents, but then his thinking on Iceland was already influenced by the vexing problem of the drift mechanism. Earlier, when he first (1912a and b) published the arguments for drift, he did not mention Iceland, but it seems worthwhile to remember some of his early thinking which, indeed, sounds very modern.

Although Wegener considered the evidence for drift more important than an understanding of the mechanism, which would come with time, he nevertheless allowed himself some speculation on that subject. We cannot resist the temptation to quote a whole paragraph from Wegener's 1912a paper (pp. 305, 306):

> 'Weiter scheint mir aber jetzt eine Möglichkeit vorzuliegen, die Unterschiede der Meerestiefen zu erklären. Da wir für größere Gebiete doch auch am Boden der Tiefsee isostatische Kompensation annehmen müssen, so besagt der Unterschied, daß die nach unserer Auffassung alten Tiefseeböden spezifisch schwerer sind als die jungen. Nun ist der Gedanke wohl nicht von der Hand zu weisen, daß frisch entblößte Simaflächen, wie der Atlantik oder westliche Teil des Indik, noch lange Zeit hindurch nicht nur eine geringere Riegheit, sondern auch eine höhere Temperatur (vielleicht um 100° im Mittel der obersten 100 km) bewahren als die alten, schon stark ausgekühlten Meeresböden. Und eine solche Temperaturdifferenz würde, wenn sie auch, wie früher erwähnt, zur Erklärung der Gewichtsdifferenz zwischen kontinentalem und ozeanischem Material bei weitem nicht ausreicht, doch wahrscheinlich genügen, um die relativ geringfügigen Niveaudifferenzen der großen ozeanischen Becken untereinander zu erklären. Diese scheinen es auch nahezulegen, die mittelatlantische Bodenschwelle als diejenige Zone zu betrachten, in welcher bei der noch immer fortschreitenden Erweiterung des Atlantischen Ozeans der Boden desselben fortwährend aufreißt und frischem, relativ flüssigem und hoch temperiertem Sima aus der Tiefe Platz macht.'

(Freely translated: Further, I now believe I can explain the differences in ocean depth. Since we must assume isostatic compensation for the ocean floors, it follows that old (in our view) sea floor is denser than that which is younger. It is likely that recently uncovered sima, as the Atlantic and the western Indic, long preserves lower rigidity and higher temperatures (perhaps 100° C on the average for the uppermost 100 km) than old, considerably cooled sea floor. Although not sufficient to explain the density difference between continental and oceanic material, such temperature differences could adequately explain the minor differences in depth between the great ocean basins. These differences also seem to indicate that the Mid-Atlantic Ridge is that zone in which the floor of the Atlantic in its progressive spreading is rifting open and making space for fresh, relatively fluid, high-temperature sima rising from depth.)

Of course, a large body of new data on bathymetry, magnetics, and heat flow of the oceans was needed to substantiate such speculations (Vine and Matthews, 1963, Sclater and Francheteau, 1970). To be sure, Wegener never quite gave them up (e.g., 1922, p. 96; 1929, p. 211), but he could not see their significance for lack of data and perhaps because he was distracted by the 'new' model of the continents sailing in the sima like icebergs in the sea. His fascination with rheology may have played a role here; consider that the model involved strong (possessing strength), though fragile continents floating in viscous, though hard sima, like cork floating up in cold tar; and remember that Wegener was in need of a convincing model in view of the mounting opposition to continental drift. In contrast to some of his opponents, however, Wegener was never dogmatic about the mechanism.

Returning now to the subject of this volume, we conjecture that Wegener's thinking on mechanisms might have taken a different course, had he known Iceland better. Actually, in 1912 he did travel by pony from Akureyri in the north to, and across, Vatnajökull in the southeast (Koch, 1912), but he probably did not see conspicuous fissures and may have been preoccupied by the preparations for the Greenland expedition (Schwarzbach, 1979). Otherwise his ideas on rifting and spreading might have been strengthened since they are so obviously demonstrated in Iceland as a part of the Mid-Atlantic Ridge. Instead, the ridge more and more became a continental relic from the original splitting (1922, p. 42) and Iceland came to have originated between a double rift (p. 41) or to be held up by molten sial rising beneath it from under the receding continents (p. 40). In this at least there is a surprising affinity with those who contest continental drift altogether and wish Iceland to have a stable continental basement (Belousov, 1970; Belousov and Milanovsky, 1976).

Wegener died in Greenland during the winter 1930/1931, but his ideas remained alive and were heatedly discussed. The majority of earth scientists rejected them, and we must admit today that there were many open questions. The picture of Iceland in particu-

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lar was confusing, indeed; this has, however, not impeded geodynamic research there, and may have rather stimulated it. Probably the first to link the tectonic and volcanic style of Iceland to Wegener's drift hypothesis was the Danish geomorphologist and geologist N. Nielsen (1930, 1933), and S. Thorarinsson (1937) concluded his study of the 1934 Dalvik earthquake in North Iceland by suggesting that also seismotectonics supports the hypothesis.

In this situation several German geodesists, geophysicists, and geologists felt that it was most important to prove or disprove continental drift by direct geodetic observation in Iceland. Wegener (1912, pp. 307–309; 1922, pp. 77–82; 1929, pp. 22–34) himself had often stressed the importance of geodetic proof (but he had thought that the drift between Greenland and Europe is fast enough to be observable, and in fact was successfully measured, by longitude determinations). The choice of Iceland suggests that Wegener's early ideas as quoted above were seriously considered a possibility. The neovolcanic zone in North Iceland was chosen under geologic advice, probably for its simple shape and its conspicuous fissures. The working group consisted of O. Niemczyk, E. Ansel, F. Bernauer, E. Emschermann, and A. Schleusener. An account of their 1938 expedition to Iceland was given by Niemczyk (1943) who argued (p. 1) that even if the inner young volcanic zone 'flows' apart, the behaviour of the rigid Tertiary basalt massifs on both sides must give evidence for or against the expected horizontal drift, and that this evidence could be obtained by setting up a network for repeated measurements of distance, elevation, and gravity. Fortunately, the network could be largely recovered after the war; in 1964/1965 it was remeasured for the first time and extended by K. Gerke (1967, 1974), H. Spickernagel (1966), and A. Schleusener and W Torge (1971) with assistance from E. Tryggvason. Surprisingly, the 1965-1938 comparison demonstrated no significant extension across the rift zone. This might have been taken as evidence against Wegener's hypothesis, but at the time these results became known, sea-floor spreading had already been convincingly inferred from the marine magnetic anomalies (Vine and Matthews, 1963) and the concept of plate tectonics was just being formulated to explain seismological data (McKenzie and Parker, 1967; Morgan, 1968, Isacks et al., 1968). Thus the geodetic observations in North Iceland were most puzzling, raising more questions than they answered.

Work Reported

The choice of North Iceland for measuring drift, after all, has turned out to be an extremely fortunate one. It was in this zone that an episode of rifting started in 1975 (Björnsson et al., 1977, 1979) which continues to the day of writing. The 1938 and 1965 measurements cross this zone and now serve as the necessary reference for the horizontal and vertical motions as well as the gravity changes along an E-W line more than 100 km long. Several papers in this volume (Möller and Ritter, Pelzer and Gerstenecker, Tryggvason, Spickernagel, Sigurdsson, Torge and Kanngieser, Johnsen et al.) are devoted to this subject. The rifting event may be the first one to be observed and certainly is the best documented. It is thus extremely important to our understanding of the process. The observation of magma movement through a dyke by its related seismicity (Einarsson and Brandsdóttir) during this event is therefore particularly valuable. Two of the geodetic studies (Tryggvason, Johnsen et al.) focus on the vicinity of the Krafla magma chamber, continuously inflated from below, but occasionally deflated rapidly by outflow into dykes; three (Möller and Ritter, Spickernagel, Torge and Kanngieser) present accurate measurements of the regional deformation; two (Sigurdsson, Pelzer and Gerstenecker) look at the effects of the rifting on the fissure swarm some 50 km north of Krafla where seismicity has also been studied (Einarsson and Brandsdóttir).

Although less spectacular than in N-Iceland, activity in the southwest is obvious and manifests itself in earthquakes (Foulger and Einarsson). Their relation with the state of stress is, however, complicated (Voight et al.). For the region of current rifting in N-Iceland, strain and stress measurements are not yet available, but such measurements are in progress. The comparison with large-scale deformation (Möller and Ritter, Spickernagel, Torge and Kanngieser) would shed light on the driving forces (long-distance tension versus push from dyke intrusions).

Another important question is that of the history of tectonicsm and magmaticsm activity in Iceland. The very different appearance of the Tertiary flood basalt regions (Fig. 1) and that of the neovolcanic zones (Fig. 2) made many people believe that there had been a hiatus between Tertiary and Pleistocene volcanic activity. Statistics offers no evidence for any variation during historical time (Gudmundsson and Saemundsson) and theoretical modelling of crustal generation (Pálmason) suggests that the different character of old and young regions is not in conflict with a continuing, more or less steady process. This is supported by work on paleomagnetism (Kristjansson et al., Schweitzer and Soffel) and magnetic anomalies (Becker); a side-line is the clarification of the history of glaciations. Even more directly revealing the tectonic history are the marine magnetic anomalies from the regions south (Voppel and Rudloff) and north of Iceland (Vogt et al.); these studies continue a well known tradition in the region (Vine and Matthews, 1963; Heirtzler et al., 1966) and pose new questions as to the interaction of 'normal' sea-floor spreading and the action of a deep 'Iceland plume' In discussing models of such an interaction it is helpful to compare Iceland with related geotectonic phenomena such as Hawaii (Wyss).

There is no doubt that Iceland is part of the Mid-Atlantic Ridge and that it is an anomalous part. Iceland cannot be understood without looking at the surrounding sea floor (Johnson and Pálmason, Jacoby, Bram), but we can also learn much about the oceans by studying Iceland, which is a much more convenient place for many kinds of observations than the sea floor. We must, however, use caution before making any generalization one way or the other. Detailed data on sea-floor morphology (Vogt et al., Jacoby) and heat flow (Bram) north and south of Iceland will be important information on the transition from the Iceland anomaly to normal ocean. Some of the differences between Iceland and the surrounding sea floor, to be sure, are simply the expression of the different environments (submarine versus subaerial volcanism). The Icelandic plateau basalts and ash layers smear out the magnetic anomaly strips so typical for the ocean, where dyke injection dominates the magmatic processes. For the same reasons the bulk mechanical properties of Iceland and ocean crust may be different, Iceland being more ductile; the lack of clear transform faults, expected from the spreading geometry, may be explained this way, but also simply by frequent burial under flood basalts and ash layers. Thus, it is not easy to distinguish direct and secondary effects of the anticipated cause in the deep mantle.

The question what Iceland really is, has been controversial and is still in the very centre of current geodynamic research it is that of vertical versus horizontal (plate) tectonics and also that of deep-mantle plumes. The question, of course, cannot be answered without some more direct information on the structure, state, and composition of the crust and upper mantle beneath Iceland and the North Atlantic. Seismic methods using earth-

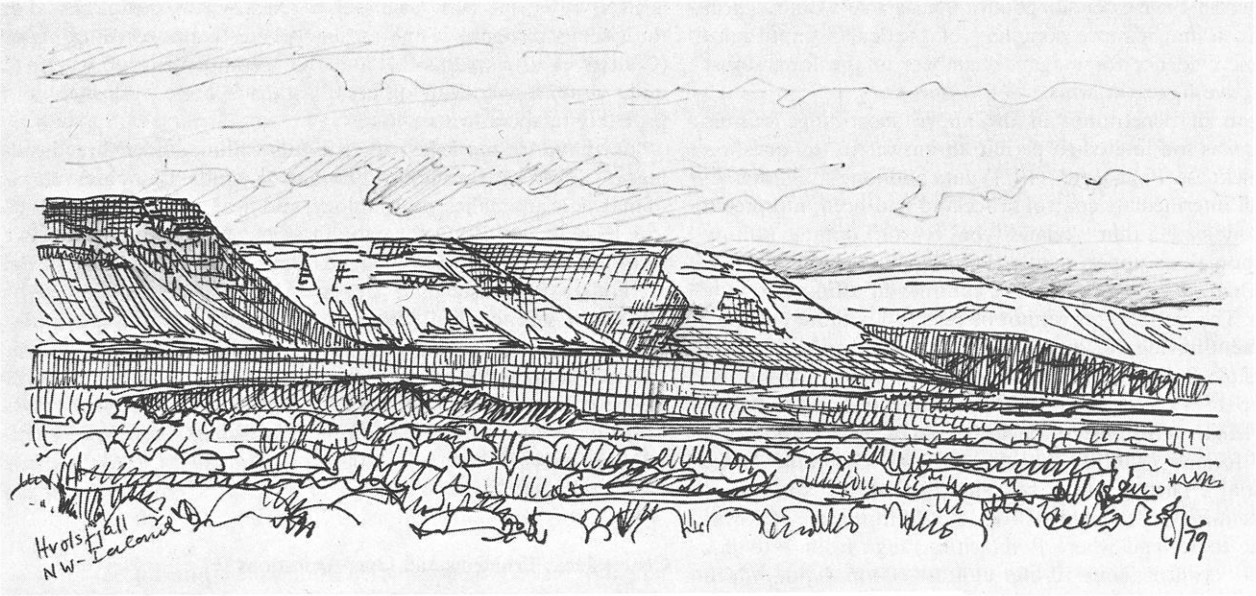

Fig. 1. Flood basalts of NW-Iceland: Hvolsfjall across Gilsfjördur in September 1979

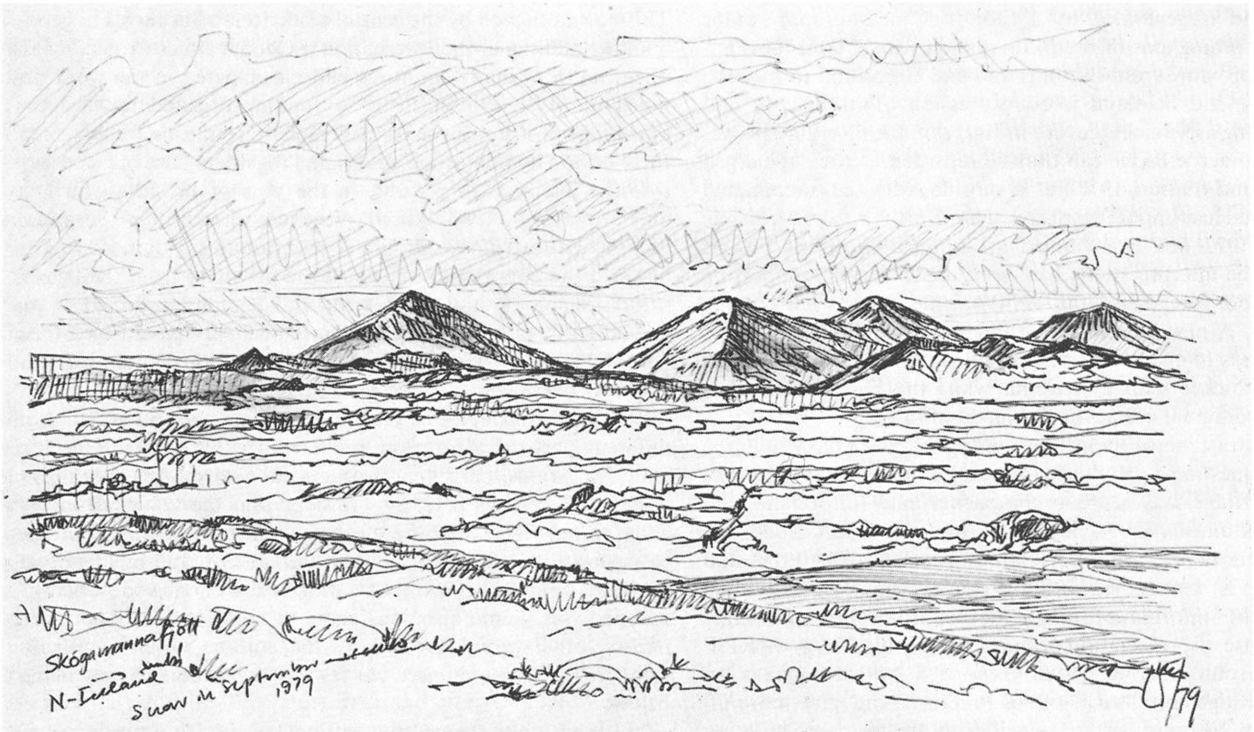

Fig. 2. The neovolcanic zone of N-Iceland: Skógarmannafjöll east of Mývatn across post-Pleistocene flood basalts (Búrfellshraun) under partial snow cover (September 1979)

quakes and/or explosions yield the most detailed and least ambiguous information on structure, but less on temperature and composition; with magneto-tellurics the opposite is true, thus complementing the former; gravity is useful in giving a broad picture of the effects of the dynamic processes on mass balance and isostasy.

Considerable efforts have been made in all these fields, and one of the purposes of the present volume is to assemble many of the results. Earlier crustal refraction profiles of Pálmason (1971) have been re-interpreted with the aid of synthetic seismograms (Flóvenz), with the result that the Icelandic crust now looks more like oceanic crust (Bunch, Goldflam et al.). Two extensive wide-angle reflection surveys have been carried out recently in the active rift zones and the adjacent plateau basalts of Iceland (2 papers by Zverev et al.) resulting in the most detailed picture of the crust so far obtained in such zones; Pálmason's model predictions are confirmed in their essential points, but as one would expect the structure is much more complex; of particular importance is the seismic evidence for magma chambers in the lower crust under the active fissure swarms.

The depth of penetration in the above short-range seismic experiments was too limited to permit an answer to the question of crustal thickness. Pálmason's (1971) data and long-range refraction, without intermediate control in Iceland had been interpreted as to either indicate a thin 'Iceland-type' crust of oceanic affinity above an anomalous upper mantle (Pálmason, 1971, Bott, 1974) or a more than 50-km-thick crust of continental affinity (Zverev et al., 1976). The controversy cannot be resolved without a refraction experiment having the longest range and tightest intermediate control possible. Such an experiment was carried out by an international group in 1977; it involved a multiple refraction profile of 800 km length along the southeast flank of Reykjanes Ridge and across Iceland (RRISP Working Group; Gebrande et al., Goldflam et al.). There seems to be a sharp transition from typical oceanic crust and upper mantle of 10 Ma old lithosphere at Reykjanes Ridge, to Iceland where P velocities range from 7 to less than 8 km/s between about 10 and at least 60 km depth, which is atypical for both continental and oceanic upper mantle; it may be termed 'Iceland-type upper mantle' which according to magneto-telluric observations (Beblo and Björnsson) is hot and probably partially molten.

If Iceland is generated by a 'hot' or 'melting spot' at or near the spreading axis of the Mid-Atlantic Ridge, then the Thulean basalt province from Baffin Island and Greenland to Iceland, the Faeroes, and Scotland is an expression of its activity and the connecting aseismic ridges are its traces on the diverging plates; the Iceland-Faeroe Ridge can thus be regarded as the submerged part of Iceland (Nilsen, 1978), as it subsides with the surrounding sea floor and cooling lithosphere. The North Atlantic Seismic Project in 1972 had been aimed at studying the crust of this ridge, but the interpretations had been controversial, supporting either sea-floor spreading (Bott, 1974) or a more fixist view (Zverev et al., 1976). A new more detailed interpretation (Bott and Gunnarsson) leads to the conclusion that the crust is of the 'Iceland type', but thicker than in Iceland, while the Faeroe block has a distinctly different crust of continental affinity.

The contrast between Iceland and the adjacent ocean is an important question in studying the crust and upper mantle. The Reykjanes Ridge may serve as the background for Iceland, but this ridge is unusual in having a conspicuous crestal block and shallow water depth, probably related to Iceland. The crust and its evolution is best studied with explosion seismology (Goldflam et al., Bunch); information on the deeper lithosphere is meager partly because of propagation problems of seismic energy (RRISP Working Group). Surface waves generated by earthquakes on Reykjanes Ridge and Charlie-Gibbs Fracture Zone give more information on average seismic velocities at greater depth than on detailed vertical structure (Keen et al., Jacoby and Girardin). Nevertheless, a rather detailed picture of lithospheric evolution has come forth from the above studies: very thin lithosphere near the crest (Keen et al.) traps rising melt below 20 km depth after a few million years of existence; this is evident in an upper low-velocity layer, before the lithosphere-asthenosphere transition at 60 km depth is clearly established at 10 Ma age, or so (Jacoby and Girardin). By this time the crust is fully developed (Bunch, Goldflam et al.).

Finally, there is the question why slow spreading ridges generally have a crestal rift while Reykjanes Ridge, similar to the fast spreading East Pacific Rise, has a positive crestal block which is itself rifted, at least near 62° N (Jacoby). A model study of the viscous response of the asthenosphere to the receding plates (Collette et al.) explains this and the accompanying gravity field quite convincingly with different asthenospheric viscosities and probably temperatures.

We have arranged the papers in this volume under three headings: (1) Tectonic Framework, Evolution (studied by observations of magnetic anomalies, morphology, and heat flow and by theoretical modelling); (2) Deformation, Stress, Seismicity, i.e., active tectonics (studied by geodetic, gravity, and seismological methods); (3) Crustal and Upper Mantle Structure (studied by seismic or seismological, magneto-telluric, and gravity methods). Not all papers have found an entirely satisfactory place in this scheme. This introductory paper is meant to serve as a guide to the contents of the volume. Finally, the most recent reviews of geophysical work on Iceland include: Björnsson (1967), Kristjansson (1974), Pálmason and Saemundsson (1974), Jacoby (1979).

Conclusions: Problems and Open Questions

When the idea was formed to publish an Iceland Volume, it was intended as a common platform for new results from the large amount of work done in and around Iceland in recent years. This was motivated by the central place Iceland occupies in geodynamics and thus in the International Geodynamics Project. Many scientists from many countries had participated in the work and much money has been spent by funding organizations; it thus appeared most appropriate to demonstrate the usefulness of all these efforts by publishing the results together. Last but not least, it is the intention to honour, in the year of his 100th birthday on November 1, 1980, Alfred Wegener, who died in the middle of his work nearly 50 years ago. We venture to say that, if Wegener had had the opportunity to see all the new data from the oceans, from seismology, and from structural geology collected in the second half of this century, he most probably would have been one of the first to understand the New Global Tectonics and to throw overboard his old idea of the sailing continents.

Most importantly, this volume is to bring together results from different parts of the region and obtained by different methods with the various, partly conflicting ideas and models. We do not attempt a synthesis here, but we hope that the volume itself will advance our understanding of geodynamics by opening our eyes to aspects and relationships hitherto unnoticed, and by identifying problems and questions to be attacked with new observations and analyses. Some questions come to mind by just reading the papers, others are expressed by the authors taking conflicting views of the same subject. A few of the problems are named below.

1. How does the current rifting episode fit into long-term spreading? A synthesis of the various geodetic and other observations, both local and regional is called for with information on inter-episode deformation. It is also most important to continue the local and regional observation of deformation through the whole rifting event and beyond to study how the deformation spreads through the lithosphere.

2. What is the driving mechanism of the rifting event? Is magma squeezed in gravitationally (buoyantly) pushing the sides into compression or is regional tension from plate divergence released in fissures tearing open and making space for the magma? The regional deformation of the area (Möller and Ritter) can be interpreted either way. Strain/stress measurements by overcor-

ing or hydrofracturing in North Iceland could discriminate between the two models by means of the 'absolute' stress.

3. How does the suspected Iceland plume interact with sea-floor spreading? Two somewhat conflicting views are expressed, one concerning the regional stress field in and around Iceland (Wyss), the other concerning the obliquely spreading Reykjanes Ridge subject to both regional (plate) tectonic and thermal stresses (Jacoby).

4. Several seismic models of the crust and upper mantle of Reykjanes Ridge are presented (Bunch, RRISP Working Group, Goldflam et al., Keen et al., Jacoby and Girardin) which are not in perfect agreement with each other. The lithospheric low-velocity layer is clearly evident only in one surface wave study (Jacoby and Girardin). Is it real? If so, is it a special feature of Reykjanes Ridge close to Iceland or a property of all spreading ridges? In the first case, does it record a transient influence of the 'Iceland plume' or a steady-state process of lithosphere evolution? A promising exercise for the immediate future is to synthesize all the new and old data to establish a consistent model of Reykjanes Ridge.

5. For Iceland and the Iceland-Faeroe Ridge a special 'Iceland-type' crust has been demonstrated (RRISP Working Group, Bott and Gunnarsson), but at the Iceland-Faeroe Ridge it is distinctly thicker. Why is this so? Is it the result of aging or of temporal variation in crustal generation?

6. Will the controversy about the nature of the Iceland crust and upper mantle be finally settled or shall we continue to disagree?

7. Several different seismic anisotropy models are discussed in this volume (e.g., RRISP Working Group, Goldflam et al., Keen et al.). Each is connected to different dynamic processes under Reykjanes Ridge. A discrimination between the models therefore has interesting consequences. Which model is correct?

These are only some of the questions which will be asked.

At this point we wish to thank all those whose work and efforts have brought about this volume: the authors, the referees, the Journal editor, the publisher, and Deutsche Forschungsgemeinschaft. Several referees reviewed more than one manuscript. R.I. Walcott, C. Kisslinger, H. Illies, and J. Untiedt gave their advice on such editorial questions as to how to formulate the title of the volume and many others. B. Geidel patiently put up with author's special requests and the inadequacies of the coordinating editor of this volume. G. Jung-Jacoby supported him in this work actively. Professor H. Illies first proposed to the National Committee of the International Geodynamics Commission and to the editor of the Journal of Geophysics that an Iceland Volume should be put together.

This is also the proper place to remind the reader that the original work reported here was made possible by many funding organizations and by the cooperation and efforts of many scientists, technicians, ships' crews, administrators and others, not named anywhere in the volume. One organisation, however, should be named here: the National Research Council of Iceland, representing the Iceland science community, that has given permission and support to the work in Icelandic Territory. Last but not least, a large unnamed group often forgotten are the scientists' companions who put up with them going to the field and writing papers till late at night.

M. Schwarzbach, D. Voppel, and K. Gerke supplied important information on Alfred Wegener and the early studies; H. Illies, J. Untiedt, B. Higgs, G. Ranalli, and J.G. Dennis assisted in clarifying the contents and the English style of this article. We thank all of them.

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