the rabbits, rats and mice and similar other creatures, is nearing the saturation point; we are beginning keenly to feel the competition of the insects. Our further increase in the not distant future will be measured by the success we may attain in the displacement of the insects from their normal food which we shall ourselves consume.

Our future increase is dependent on the ability we may show to cut deeper and deeper into the ranks of those plant-feeding things that are competing with us for our food supply. The ability of our grandchildren to live will be measured by their ability to suppress their equivalent in insects.

What has the sea to offer for the future? Much less than is commonly supposed. In the sea the huge annual surplus of vegetable material so characteristic of the land does not exist. Except for a few flowering plants growing in shoal water near the coasts and the abundant sexual products of some algae, no sea plant bears special organs like the leaves of land plants which are discarded at the end of the growing season. There is no need for them to do so, as the humidity does not vary and the changes in the temperatures, if any, are very slight and gradual.

Life in the sea is a continuous cycle without the enormous annual waste of organic substance that characterizes life on land. In the sea when the minute plants called diatoms increase in numbers this phenomenon is promptly followed by an increase in those small creatures, especially the copepods, that feed on them. Increase in the copepods is followed by an increase in the number of the copepod consuming fishes, and these serve to attract predaceous fish and mammals that feed on them. When, owing to changed conditions, the numbers of the diatoms fall off, the copepods in their turn become less numerous. The fishes, however, swim away and seek their food elsewhere, followed by the predaceous fishes, dolphins, porpoises, etc. The annual cycle in the sea is a cycle of continuous, though changing, life, and there is almost no organic waste. If a large creature dies, it is soon consumed by the other creatures of the sea. On the bottom muds live many scavengers that feed upon whatever falls down from above, many swallowing the mud and digesting out of it the half-decayed remains of diatoms and copepods and other things.

Probably the extensive ooze deposits on the bottom of the seas mostly represent the shells of shell-bearing animals, diatoms, etc., from which the organic matter largely had been eaten or digested before they fell. Were this not so we should expect to find upon the oozes much more abundant life than there exists.

Of the products of the sea we eat plant feeders, predators of plant feeders, scavengers and detritus feeders. We eat sea plants only in wholly negligible quantities. Our relations with the sea to-day are just the same as those of the most primitive of mankind were with the land. Just as primitive man found the uninhabited lands the most productive, so we find the uninhabited coasts the most productive. Any large increase in sea products must come mostly from the exploitation of new areas. By the elimination of the present waste and by proper conservation the amount of food drawn from the sea can be more or less increased in the populous regions, but the possibility of increasing the productivity of sea areas already being utilized is infinitely less than the possibilities of similar efforts on the land.

The interrelationships of the land and sea as set forth above are not yet proved, but all the evidence seems to point in that direction. On our utilization of the land depends the productivity of our adjacent seas. For instance the unrestricted use of streams for industry by the pollution of the water not only kills off such aquatic life as they contain, but, more important still, decreases the value of the substances brought down for the support of marine life.

Intensive cultivation of extensive areas on land not only greatly lessens the amount of vegetable detritus washed into the sea, but also permits the washing off from the land surface of quantities of mud and sand. Such detritus as goes down a very muddy river is largely buried when the sand and mud sinks to the bottom on the ocean floor, or is so diluted with mud particles as to make it unavailable for use by detritus feeding animals. Furthermore, in a muddy river mouth only a small part of the available material can be used by plants, since the sunlight in effective quantities can only penetrate for a few feet through the clouds of mud.

For centuries the land areas have been subject to intensive study. The similarly intensive study of the seas is yet to come. We see our way to an increase in the products of the land. We see at present no such simple way of increasing the products of the sea.

We can not escape the inference that at the present time and in the future we should especially devote ourselves to the problem of displacing the competing insects on the land, and to an intensive study of the seas, especially with a view to ascertaining what it is that feeds the life in them, the nature, extent and quantity of that life and how we may conserve and utilize it to the best advantage.

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NOTES AND REFLECTIONS ON ISOSTASY

IF the earth's materials were sufficiently plastic to allow of a condition of perfect equilibrium, these materials would arrange themselves in concentric strata according to density, and there would be no surface irregularities. But in the actual earth, owing to the resistance to flow of the crustal materials, perfect equilibrium is not reached. Wherever denser material remains at the same level as lighter material, and wherever portions of the crust are not in complete equilibrium, there will be stresses and a tendency to readjustment.

It is reasonable, as well as convenient, to assume that there is some uniform depth of equal pressure, below which there is perfect equilibrium, and above which there is that close but imperfect condition of adjustment called isostasy. Isostasy might exist in the earth's crust with an indefinite number of combinations of varying crustal densities and depths, falling within two extremes. At one extreme is the theory that the depth of crustal effect is uniform, and that the unit columns above that depth have equal weights, with densities differing according to the height of the columns, thus with less density beneath mountains and greater density beneath ocean beds, but with the density in any one column uniform. This is the hypothesis used, for convenience of computation, in the Hayford gravity reduction method, excepting that the depth of compensation was taken as being uniform from the earth's surface, therefore having the same irregularities as that surface. The other extreme theory assumes that the crustal material is of uniform density, and does not extend to a uniform depth but projects into a heavier substratum sufficiently to maintain the elevated regions in equilibrium; the depth of equal pressure then becomes the depth of the deepest projection, and the unit column of equal weight above that depth includes more or less of the subcrustal material. In the former, but not in the latter, the depth of equal pressure and the depth of crustal effect would be identical. These have been referred to respectively, as the Pratt and the Airy theories, though not proposed so definitely by either. Pratt recognized horizontal compression due to a contracting crust, as well as vertical contraction and expansion, as explanatory causes of crustal conditions, and followers of Airy have recognized greater density beneath ocean beds as a factor in equilibrium.

That the actual crust does not conform to either of these extreme theories is evident from the known differences in the density of crustal materials. As regards the second theory, local crustal rigidity makes it unnecessary to assume that there are "roots" having sufficient depth to support mountain peaks separately. There are several factors which would tend to reduce considerable protuberances, if they ever existed, as, for example, the lateral pressure of the adjacent denser materials of the substratum, and the horizontal movement beneath the outer crust. As regards the first theory, the magnitude of the forces, whatever their character, necessary to have caused existing surface irregularities and subsurface flow would be likely to cause material departures from uniformity in the depth of crustal effect. It may be that the actual condition of the crust is well within either of these extreme limits, but with the probabilities against considerable protuberances into the substratum, and also against a uniform or a well defined depth of crustal effect.

It should be noted that the meaning of isostasy is broad enough to cover any condition of crustal equilibrium within these extremes, and there is no ground on which the use of the word can properly be restricted to some particular theory of equilibrium, at least until such theory is confirmed.

Isostasy is now a much discussed subject, but thirty-three years ago there was quite "low visibility" as to the interpretation of gravity results with respect to this theory. In applying the old methods of reduction to the important series of gravity determinations made in 1894 and 1895, I found the results so discordant that there appeared to be no clear proof of any theory as to the condition of the earth's crust. Acting on a long neglected suggestion, I then developed a reduction method which in effect applied approximately a compensation for the elevation of the region about a station as though it were leveled off, instead of a compensation for the station elevation itself. When this method was applied to a considerable number of stations well located and distributed, it showed uniformly good results, and for the first time in gravity reductions the large residuals disappeared, and all the gravity results became consistent with isostasy. I used for this test 67 stations, including besides the transcontinental series, determinations in several continents, and island stations in two oceans, and every group of these showed consistently small residuals with this average elevation reduction.

At the time, I referred to these results as leading "to the conclusion that general continental elevations are compensated by a deficiency of density in the matter below sea level," and the only limitation I put on the closeness of compensation was that "local topographical irregularities, whether elevations or depressions, are not compensated for," and the meaning of this limitation was explained in another place; "as, for instance, the attraction of a mountain on a station at its summit or of an isolated island." Referring to a group of stations near the Gulf coast I said that "the smallness of the differences found indicates a close approach to the condition of hydrostatic equilibrium in this region," and referred to this reduction method "as further confirming the validity . . . of the equilibrium or isostatic theory."

The results of the extensive investigations following the development by Hayford in 1909 of a more rigorous method of gravity reductions permit additional light to be thrown on this earlier work. It has now been shown that there is substantially no difference between the gravity results on an assumption of perfect local isostasy and those on an assumption of regional compensation to some radius perhaps approaching 100 miles from the station. Therefore the average elevation method of 1895 should give similar results, which it does. It gave average anomalies substantially as small as the later results by the Hayford method, and it eliminated the large residuals as completely. It did not, however, as was then thought, prove that local features are not compensated. But the inference at the time that features of the order of a single mountain are not locally compensated is perhaps near the truth. A mountain is in general probably compensated, but it is evident that through partial rigidity the compensation is distributed beyond the area of the base, as a part of the compensation of the surrounding region; the method of distribution is, for obvious reasons, difficult or impossible to detect with the pendulum.

Hayford in his gravity work adopted a rather extreme view in favor of local compensation, as is shown by his prediction of complete adjustment to some limit between one square mile and one square degree, by his conclusion that the area that may be regionally compensated is probably less than 12 miles in radius, and by his selection of zones for the gravity reductions, the first zone having a radius of only 2 meters, and there being 11 zones within a radius of 12 miles from the station.

The 1895 work has further significance in that a different computation method leads to similar general results as to isostasy, and also in that it shows that close average results may be obtained by an approximate method which makes no assumption as to the depth, thickness or density arrangement of the compensation. Incidentally a gravity reduction method was developed which gives close approximate results by a very short computation, the correction for attraction and compensation being simply 0.0001059 multiplied by the difference in meters, between the average elevation within 100 miles and the station elevation (this constant being for an average surface density of 2.56). A subsequent result has been an explanation of wherein the so-called "free air" reduction method failed.

The reduction and discussion of the 1894 and 1895 results were done in a few months of individual work. At the time, an eminent geologist made independent computations, and drew his own conclusions as to isostasy being limited by very broad areas of rigidity, conclusions which he later abandoned, and to which I gave little heed. But as a consequence, views which I have not held have inadvertently been attributed to me. I did not conclude that mountain ranges or systems were not in equilibrium, nor that isostatic adjustment was limited to broad areas such as that of the United States as a whole. In fact I did not consider that the 1895 work gave evidence as to the limiting size of areas regionally compensated.

In the brief references to the 1895 work in the official publication of 1912 there are also some misunderstandings. As stated above, the average elevation reduction was applied at 67 stations, instead of only at the one group of 14 stations as mentioned. I did attach importance to the smallness of the anomalies derived by this method, and gave numerical and graphical comparisons with older methods showing the large reduction accomplished in their average size for each group of stations, whether on continents or on oceanic islands. My statement was only to the effect that the remaining anomalies would not have particular significance as respects the individual stations, because of the approximations used.

In this latter respect the results of the Hayford reduction method made an advance over any previous work. But it must be remembered that even this reduction, the most rigorous that has been developed, is based on a number of assumptions and approximations, and is not free from the possibility of appreciable systematic error; an indication of this is found in the comparison of anomalies for pairs of stations of considerable difference of elevation, where in practically every case subtraction of the anomaly of the lower from that of the higher station gives a positive difference, the average difference for the 16 available pairs of stations being + 0.022 dyne, and the maximum (Mauna Kea-Honolulu) being + 0.131 dyne. Omitting this maximum, the average difference is still + 0.014 dyne, an appreciable uncertainty, since the average gravity anomaly in mountainous regions is about this amount. Mauna Kea is one of the most notable gravity determinations of the world, as the station is about 18,500 feet above the average elevation of the surrounding region, allowing for the density of sea water. While there is incompleteness in the topographic data, this large difference may have some real significance as respects reduction methods, as it is fairly consistent with the other pair differences, considering the difference in elevation. There is a group of other Hawaiian determinations by Preston which should have similar interest.

A gravity measurement of geological interest was made by me in 1896 at Umanak, Greenland, in latitude 70° 40′. This station, deep in the fjords and near to the edge of the ice cap, is about 1,000 feet below the average elevation of the surrounding region. Using the incomplete topographic data and an approximate reduction, the small anomaly indicates fairly normal gravity here. Another determination of special interest, two years later, at St. Michael, Alaska, in latitude 63° 28′ N., adjacent to the great Yukon delta, also shows approximately normal gravity.[1]

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SCIENTIFIC EVENTS

CONCILIUM BIBLIOGRAPHICUM

THE arrangement whereby the Concilium Bibliographicum (Zurich) has received certain financial support during the last five years from the Rockefeller Foundation through the National Research Council, and whereby the council participated in the management of the concilium, terminated with the end of 1926, a termination provided for by the terms of the arrangement as originally made.

This termination of the arrangement referred to in no way indicates a disapprobation of the concilium's service either on the part of the Rockefeller Foundation, the National Research Council or the American users of the service.

This service, it may be briefly explained to those readers of SCIENCE not already familiar with it, is the preparation and distribution of bibliographic references in current zoology and certain allied fields on cards giving author and title references arranged according to a convenient subject classification. These cards are sent to subscribers at a reasonable rate. Subscription may be for the whole series of cards or for parts of the series referring to particular subjects.

Despite the cessation of the American subsidy, the concilium expects to continue its work, as it has been able to find some special financial support in Switzerland and Germany. It needs, however, more support than it has yet found and would be glad if its American friends could give it further financial aid.

American subscribers who have been paying their subscriptions through the National Research Council are requested to make payments hereafter to the Equitable Trust Company, 77 Wall Street, New York City, "for Concilium Bibliographicum Account, Len & Company, Zurich," and to address all inquiries and other correspondence directly to Concilium Bibliographicum, 49, Hofstrasse, Zurich.

Concilium Bibliographicum was founded in 1895 by Dr. H. H. Field, of Harvard, and has now for director Professor J. Strohl, of the University of Zurich. The difficulties created by the great war and by the death of Dr. Field soon after the close of the war nearly overwhelmed the Concilium, but the cooperation of the National Research Council, with the financial assistance of the Rockefeller Foundation, and the vigorous and devoted efforts of Director Strohl, saved the situation, and the Concilium was enabled to catch up and go on with its work.

Despite the recent establishment of *Biological Abstracts*, a periodical form of biological bibliographic service urgently recommended by the Union of American Biological Societies and by the National Research Council, and established by the financial aid of the Rockefeller Foundation, there will probably always be a considerable number of American zoologists, especially taxonomic workers, who will find the concilium cards convenient and useful. These zoologists will be glad to learn that the concilium expects to continue its service.

VERNON KELLOGG

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THE BOTANY SCHOOL OF THE UNIVERSITY OF SYDNEY

THE opening of the new botany school in the University of Sydney is an event not only important for the British Empire, but also for the world at large. The building is in modernized perpendicular Gothic and harmonizes with the main structure of the University of Sydney, which presents some interesting resemblances both in its architecture and its origin to the well-known main building of the University of Toronto. The construction is in stone and the building is so arranged that it will be an ornament to the university for many years. Although architecturally attractive, it does not represent the petrification of the science in the Pierian springs of architecture rightly dreaded by Thomas Huxley, for it is thoroughly well lighted, spacious and in every way practical. The entrance is adorned by representations of some of the great masters in the science. The idea of commemorating the great, however, is not confined to the exterior of the building, for the laboratories and other workrooms are named after distinguished botanists. The botanical museum bears the name of Bentham and Hooker and its windows show the portraits of such outstanding botanists as Hofmeister, Grew, Sachs, Nageli, Hooker, Bentham and others. The herbarium, which is spacious and well equipped, is

[1] These notes are a summary, with additions, of paper "The Equilibrium Theory of the Earth's Crust," in the *Journal* of the Washington Academy of Sciences, June 4, 1926, where detailed explanations and references are given.