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THE DEPTHS OF THE EARTH[1]

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Introduction

Charles Lapworth was one of the intellectual princes of our profession. He it was who discovered the "Secret of the Highlands." In another Scotland Yard he triumphantly illustrated the British genius for detective work. His penetrating eye saw in principle, and later Peach and Horne proved, the majesty of the Scottish thrusts. Thus a revolutionary idea became generally accepted. Yet a still more vital secret underlies the secret explained by the brilliant Lapworth. That further mystery, still unsolved to general satisfaction, is the cause of the clean-cut slicing and plastic shearing of the rocks when mountains are made. Clearly the data for solving the mystery must be sought under all the lands and seas—in the earth's vast interior.

There, too, is the condition for the isostatic rise of the crust when the regional ice-caps melted, or when continents lost weight by denudation and rose to renew their equilibrium. There ultimately is the key to the problem of former land bridges between continents and the key to paleogeographic problems in general. The petrologist also can not escape the necessity of thinking intensely about the third dimension, depth, as well as about the two dimensions of the maps. In short the geologist, however specialized, has to be a courageous soul and venture where angels can not tread. The equipped geologist shall know not only his six continents and the seven seas; he shall faithfully treasure every scrap of information that may come to him regarding the invisible and intangible, where lies the secret of secrets of all geological science.

[1] Presidential address read before the Geological Society of America, December 28, 1932. Somewhat abridged; illustrations and references omitted; to be printed in full in the *Bulletin* of the Geological Society.

To go down in imagination is an adventure, daring but thoroughly essential, for in depth is the seat of dominating energy and the origin of all rocks whatever.

Since the year 1900 discoveries and fertile hypotheses about conditions beneath the accessible part of our planet have been pouring into the literature and summarized in thick handbooks. The young science, geophysics, has become a giant with a giant's power to move, and then in a measure to fix, the foundations of geological thought. This evening I offer a speculative and of course tentative picture of the earth's interior in the light of the new geophysics, checked by facts of geology and by the possibilities of cosmogony.

THE SHELLS AND CORE OF THE EARTH

In principle the picture is given in Tables I and II. The first shows the kinds, states and densities of the materials constituting a typical continental sector.

TABLE I

EARTH-SHELLS IN CONTINENTAL SECTOR

(Crystalline Archean rocks at surface)

Depth (km)	Material, with average density	
0–30	Granite and piezo-granite	SIAL (2.8)
	Discontinuity	
30–40	Granodiorite and piezo-phases	
	Discontinuity	
40–60	Piezo-gabbro.........CRYSTALLIZED SIMA (3.05)	
	Discontinuity	
60–100±	Vitreous basalt (2.8–2.9)	
100±–1200	Vitreous ultrabasic silicate (2.9–4.5)	
	Discontinuity	
1200–2900	Vitreous silicate + oxide + sulphide (5.0–6.5)	
	Discontinuity	
2900–6370	Central core of iron (liquid?; 10.5–12.5)	

TABLE II

EARTH-SHELLS IN SECTOR UNDER DEEP, OPEN PACIFIC

Depth (km)	Material, with average density
0–5.2	Water (1.03)
	Discontinuity
5.2–74.8±	Basalt and piezo-gabbro (CRYSTALLIZED SIMA; 2.95–3.05)
	Discontinuity
74.8±–100±	Vitreous, femic basalt (2.85–2.9)
Below 100	Shells as in Table I

Table II similarly refers to the broad sector underlying the deeper part of the open Pacific Ocean. Somewhat different from either, at levels above the depth of 80 kilometers, would be any of the sectors capped by lofty chains of mountains or by oceans shallower than the deep Pacific.

This sketch of the earth's anatomy is based upon:

(1) The seismologists' determinations of wave velocities and changes of velocity in depth, with corresponding discontinuities of the material;

(2) The known composition of the upper part of the Sial;

(3) The dominantly basaltic quality of lava floods;

(4) The world-wide distribution of basaltic eruptives and the genetic association of basaltic magma with other observed igneous magmas and rocks (eruptive sequence);

(5) The necessity of assuming a distribution of densities to match the earth's moment of inertia and its mean density;

(6) The commonly accepted postulate that the earth's core is composed essentially of metallic iron, which, according to some high authorities, acts like a liquid against the weak, rapidly alternating stresses of earthquake waves;

(7) Experimental and seismological evidence as to the compressibilities of rock matter and iron, and the relation of the compressibilities to the speeds of earthquake waves;

(8) The observed and calculated thermal gradients in the planet;

(9) The probable distribution of radioactivity;

(10) The systematic differences of thermal conductivity among rocks, whether crystalline or vitreous;

(11) The probable origin of the earth;

(12) The composition of meteorites;

(13) The extreme weakness of the material below the depth of a few scores of kilometers (permitting isostatic adjustment);

(14) The facts bearing on orogenesis.

AN EARTH LARGELY VITREOUS

Here again are fourteen points. Not all of them make universal appeal and no treaty of Versailles is likely to bring peace soon in this other war of opinion. Full discussion of any of the points is out of the question, and whole fields of inquiry bearing on the nature of the earth's interior must be left untouched. Our time will be occupied chiefly with one fundamental assumption—that all but a small fraction of the 2,900-kilometer envelope of the planet's core is in the vitreous state. This implies the reality of a true crust, in the sense of a thin crystallized shell resting upon a series of non-crystallized shells; earth temperatures to correspond; strength nearly, if not quite, confined to the crust; and, because of the relations of density, potential instability for limited belts of the crust. The validity of this general idea is one of the most pressing and significant problems of earth science.

Cosmogony and the Vitreous Shells: First we observe that all the modern views about the earth's origin permit the hypothesis described. The erupto-tidal theory of Chamberlin and Moulton, the gas-filament tidal theory of Jeans and Jeffreys, the collision theory of Bickerton and Jeffreys, and the capture-collision theory of Hirayama and Van Anda all start with a gaseous earth. Chamberlin and Moulton argued for the rapid solidification of the young earth and for a tremendous increase of mass by the infall of planetesimals, the earth remaining crystalline except for subordinate, liquid pockets of quasi-eutectic composition. Yet it appears that these authors exaggerated the rôle of planetesimals in the earth's organization. Moreover, in any case the solar belch responsible for our globe may, by the erupto-tidal theory, have been big enough to retain the gaseous condition until the earth had attained practically its present mass. Jeffreys dates the segregation of the iron core at the gaseous stage—its most plausible explanation. He assumes that, after liquefying, the thick envelope about the core was "probably" made homogeneous in a chemical sense by thermal convection, and then by continued stirring was rapidly cooled and crystallized at great depth. However, it seems logical to think that the envelope itself was originally stratified, the intrinsic (chemically determined) density increasing with depth. If so, early thermal or other types of convection would cool only the outer part of the envelope about the core. Thus the initially high temperatures and non-crystalline character of all shells below the depth of a few hundreds of kilometers would be long preserved. Like the others, then, Jeffreys' cosmogonic scheme does not forbid belief that below a crust of moderate thickness the shells of the envelope are still too hot to pass out of the vitreous state.

Seismology and the Earth-shells: By general agreement the upper part of the Basement Complex (Archean terrane) is regarded as chiefly granitic. According to Sederholm's careful measurements in a regional sample (Finland), its average composition lies between those of average granite and average granodiorite. At depths greater than a few kilometers the Sial should be somewhat more basic because of an increased proportion of femic injections, and the lower half of the Sial may, in average, approximate granodiorite. There the density is doubtless increased still further by load metamorphism. Although these deeper rocks of the Sial are probably gneissic, their exact nature remains obscure. Hence there is some advantage in giving them the non-committal names of "piezo-granite" and "piezo-granodiorite."

Similarly, between the 40-kilometer and 60-kilometer levels the crystallized Sima, chiefly basaltic in chemical composition, is not likely to be gabbro or diabase, but denser rock. Its phases may be grouped under the name "piezo-gabbro."[2] The velocities of the earthquake waves are explicable if the Sial and crystallized Sima have the constitution described.

That the material just below the 60-kilometer discontinuity is vitreous basalt is an assumption at least permitted by studies of compressibility. Bridgman's pressure-volume curve for tachylite at 75° is relevant. It gives a rather close approximation to the compressibility of basaltic glass at pressures corresponding to depths of 60, 70 and 80 kilometers below the earth's surface. The density of the glass under these conditions is known within narrow limits. Taking the accepted value of Poisson's ratio at 0.27, the closely approximate velocity (V_p) of the longitudinal wave in tachylite at 75° and at the depths stated is found. Table III (col. 4) shows the result of such computation as well as the densities and elastic data. The expected high temperature of the glass at these levels is not allowed for in the calculation of wave velocity. Heating tends slightly to reduce compressibility and therefore wave velocity, but the correction required for this reason may be more than offset by proper correction for the difference between the high-pressure or static modulus of elasticity and the somewhat higher, seismically-effective or dynamic modulus for the same rock. Now the actual velocities of the longitudinal wave at the depths of 60, 70 and 80 kilometers within the earth have, according to the seismologists, the values shown in the last column of Table III. Comparison of this and the preceding column shows how well vitreous basalt meets the needs of the case. The agreement would be much poorer if crystalline peridotite were assumed at these depths, and is probably better than if vitreous peridotite be assumed.

TABLE III

DATA REGARDING A TACHYLITIC EARTH-SHELL

Depth (km)	Volume compressibility	Density	Computed V_p (km/sec)	"Observed" V_p (km/sec)
60	1.13×10^{-6}	2.80	7.4	7.8
70	1.07×10^{-6}	2.82	7.6	7.9
80	1.00×10^{-6}	2.85	7.8	8.0

[2] No important phase of this layer seems to be as dense and incompressible as eclogite.

Gutenberg's depth-velocity curve suggests only moderate chemical change between the 100-kilometer level and the discontinuity at 1,200 kilometers. Vitreous peridotite, perhaps merging into material like the average stony meteorite, would give the corresponding densities and wave velocities of Tables I and II, at these greater depths.

The meaning of each of the discontinuities, tentatively located by Gutenberg at 1,200, 1,700 and 2,450 kilometers of depth, is an unsolved problem. Goldschmidt supposes the changes of material to be in the direction from dominant silicate to dominant sulphide or oxide. The "observed" wave velocities seem to permit, though they do not compel, the idea of a vitreous condition for what Gutenberg calls the "intermediate layer," that between the 1,200-kilometer and 2,900-kilometer levels.

In conclusion, seismology has given results not unfavorable to our main thesis—non-crystallinity for all earth-shells below the thin crust. Experiments like those of Bridgman on liquids suggest that pressure alone can give the "glass" effective "solidity" in its reaction to the weak, short-lived stresses of earthquake waves. That the temperatures are high enough to prevent crystallization is a premise which would be strengthened if, as Oldham, Knott, Visser, Jeffreys and Gutenberg think, the earth's core acts as a true liquid against the stresses of earthquake waves; for presumably the temperature of the core would have to be exceedingly high to counteract the "solidifying" effect of the one to three millions of atmospheres of pressure upon the core material.

Evidence from Thermal Gradients: A review of the best data gives 1° C. per 36 meters or 28° per kilometer of depth as a good average gradient of temperature at the surface of the Archean complex. Extrapolation to depth is famously difficult. On any reasonable set of assumptions the gradient must, with increasing depth, become less steep: slowly near the surface and then faster to some moderate depth, where the rate of change becomes lower again. The troubles in calculating the gradients at depths greater than about 25 kilometers are numerous and of differing importance. There are two major troubles. One is connected with uncertainty regarding the distribution of radioactivity in the rocks; the other with uncertainty regarding the content of primitive, nonradioactive heat in those earth-shells which, through geological time, have actually felt the cold of outer space. Minor difficulties relate to the proper values to be assigned to the age of the earth and to the conductivity and specific heat of each of the outer earth-shells.

Because the crust has existed continuously since the Early Archean, all authorities assume strong concentration of radioactivity in the superficial shells. Jeffreys and others believe that the thermal output of the radioactive furnace is less than the heat lost by radiation from the surface, a large fraction of this squandered energy being an original endowment of the planet. Joly and Holmes, though also assuming much concentration of radioactivity in the crust, find the radioactive furnace efficient enough to endanger the continued existence of the crust, if unmoved.

According to Joly's well-known theory the crust persists, because periodically thinned by melting and then dragged over the earth's body; for both reasons the excess heat is supposed to have been rapidly conducted and radiated away. To this theory there are physical and geological objections, apparently fatal.

Holmes appeals to continental migration as the leading cause for the dissipation of the excess heat. The migration is thought to have been compelled by periodic convection and overturn of the whole 2,900-kilometer envelope about the earth's core. This bold suggestion implies that compression alone accounts for the downward increase of density in the envelope. Yet, as Williamson and Adams showed, mere compression of either basalt or peridotite could not give densities matching the earth's moment of inertia and mean density. There are other formidable objections to Holmes's theory, but here too time fails for their discussion.

Considering all the facts of the case, I think it most probable that no more than half of the heat radiated from the globe is of radioactive origin, the rest being original heat.

Holmes in 1915 and later Adams and then Jeffreys computed the temperatures to depths of 100 or more kilometers, on two assumptions: moderate heat of radioactivity, and little or no secular loss of heat from depths greater than a few hundreds of kilometers. Holmes deduced temperatures of 600°, 1204° and 1575° at the respective depths of 20, 60 and 100 kilometers. Both Adams and Jeffreys found considerably lower temperatures for the same depths.

All three sets of calculations are affected by the uncertainty as to the law of diminution of radioactivity with increasing depth, and all three ignore the possibility of thermal convection in the earth's body. The latter process would seriously affect the quantity of original heat to be dissipated in the course of time. While the single-step convection through the 2,900-kilometer envelope can not be assumed, Holmes has recently done good service in once more emphasizing (with Jeffreys) the possibility of convective transfer of primitive heat. If hot enough to be vitreous, the earth-shells at depth may reasonably be considered as infinitely weak (liquevitreous), whatever rigidity they exhibit against seismic and tidal stresses. In any of these shells convection is possible, provided: (1) the thermal gradient is steep enough; (2) the intrinsic (chemically determined) density is not far from uniform; (3) the shell is at least a few hundreds of kilometers thick; and (4) the viscosity is not many times higher than that of steel. Surface cooling establishes a thermal gradient in the topmost liquevitreous layer. Suppose this layer to be convectively overturned. The next shell beneath is chilled and thus has its own gradient steepened. Suppose it also to be convectively overturned. A third layer is then liable to the same change; and so on. These changes take much time, but it is conceivable that at long intervals original heat is brought from great depth to the base of the earth's crust. This speculative process, involving a slow, downward transfer of cold from shell to shell, may be briefly described as *delayed, tandem convection.* If such a mechanism has been at work, it is obviously wrong to postulate a fixed "initial" temperature for the outer earth, a temperature controlled wholly or essentially by the relation of melting temperature to pressure. For this reason the gradients calculated by Holmes, Adams and Jeffreys may be less steep than the true gradient.

Their assumptions of minor importance should also be examined. In each of the three calculations the thermal conductivity of crust rocks is taken at too high a value. Not enough allowance is made for the decrease of conductivity with increase of temperature; nor for the fact that the statically metamorphosed rocks of the crust, with the expected flat-lying schistosity, conduct heat in a vertical direction more slowly than the chemically equivalent but unlayered rocks (granite to gabbro) conduct heat. Moreover, Holmes (since 1929) and Jeffreys assume a vitreous state for earth-shells well within the layer affected by secular cooling, and should in their computations have allowed for the conductivity of vitreous rock, which, at ordinary temperature at least, is much lower than that of the equivalent crystalline rock. Again, all three calculations were based upon a too-low value of the specific heat, a quantity increasing with temperature and also with the change of state from crystalline to vitreous.

The net result of these minor errors also is to find the earth cooler in depth than it really is.

Until more is known about the cause of the distribution of radioactivity in the rocks and about the rôle of delayed convection, it is impossible for the mathematician to declare finally the existing thermal gradient in depth. My own tentative estimates of temperature give 760° and 1330° at the respective depths of 30 kilometers and 60 kilometers; below the 60-kilometer discontinuity, temperatures everywhere so high as to prevent crystallization. While these estimates appear compatible with the principles of earth physics, I dare to put them before you, primarily because they portray a vital part of a theory of the earth which seems best adapted to account for the facts of geology.

Isostasy and the Vitreous Shells: If the earth has a true crust resting on a succession of vitreous, because hot, shells, the weakness of the glassy material automatically explains the sensitiveness of the crust to glacial loads, and in general accounts for the condition of isostatic balance among the larger topographic features of the globe. Supposing radioactivity to be proportioned to the acidity of the crust, the thickness of the crust and the densities of the outer segments of the earth should vary systematically. The result is a scheme of densities different from those of the Pratt and Airy explanations of isostasy. The preferred scheme can not, of course, be described in exact figures; an indefinite range of choices within the limits set by my theory is admissible. Nevertheless, a reasonable set of choices seems to be indicated, and I have found that the corresponding arrangement of densities reduces the gravity anomalies about as well as the Airy hypothesis does and better than the Pratt hypothesis, so commonly used by geödesists. (Illustrative diagrams of the address here omitted). Hence the explanation of isostasy implied by the crust-substratum idea seems to have some support from the pendulum studies so far made.

Evidence from Petrology: Gravitative differentiation of magma is evident in certain intrusive sheets, laccoliths, lopoliths, dikes, thick extrusive flows, and probably in visible parts of a few batholiths. Year by year new examples are being discovered. It is surely not a wild idea to think that the earth, from the bottom of the crust to the top of the iron core, is also stratified according to intrinsic (chemically determined) density. As in differentiated sheets, the density of the envelope of the core should not be expected to increase uniformly with depth. The analogies mentioned suggest rapid changes of density at more or less widely spaced levels. Between any pair of those levels the intrinsic density may be nearly constant.

Because of their high density the deeper shells should not be eruptible into or through the crust. Normally the one layer that does erupt, as if emanating from a continuous earth-shell, is the basaltic. From the Pre-Cambrian to the present day it has delivered at the surface great masses of basaltic liquid in every continental and oceanic sector. This liquid issues alone or in direct association (inside the time limits of the petrogenetic cycle) with magmas of different composition. Thirty years ago the hypothesis that such basaltic magmas have come from local pockets of liquid in an essentially crystalline earth was held by some petrologists. But there is manifest difficulty in accounting for the pockets, whether as residuals of a planet once liquid, as locally developed eutectic solutions, or as local, radiothermal fusions. An increasing number of petrologists and geologists prefer the postulate that the erupted basalts originated in a world-circling, vitreous substratum.

Although not to be directly verified, this hypothesis has the advantage of explaining petrological and geodynamical facts more simply than any other yet published.

(1) In the first place, it enables us to understand why lava rises so high. The density relations expressed in Tables I and II are such that, if the crust is opened by a through-going fissure, the dead weight of the crust would tend to push up the substratum material at volcanic pipes to heights from three to six kilometers above sea-level.

(2) The hypothesis automatically accounts for the latent heat of the erupted, fluid basalts.

(3) Rising from a minimum depth of 60 kilometers, the basaltic liquid should be superheated. In fact the plateau-basalts, even after prolonged running and rapid radiation under the air, are still liquid to a degree apparently impossible if the flows started without superheat. If not superheated, could thin basaltic (diabasic, gabbroid) sills spread underground in their astonishing way?

(4) Without exception known to me, the earth's crust is basined where large volumes of basaltic magma were erupted at or near the surface, in the form of basaltic plateaus, or major cones, or lopoliths. The diameters of such structural basins measure hundreds of kilometers, a size suggesting that each erupted mass was transferred from a single, continuous earth-shell.

(5) The hypothesis seems to be the most adequate as a basis for deriving the origin of the 700 other species of post-Archean igneous rocks. Endowed with both latent heat and superheat, the erupted basaltic liquid is capable of melting and assimilating crust rocks, and these reactions, followed by differentiation of the syntectics, account for many of the non-basaltic species. On a particularly grand scale are the expected reactions and differentiations if large masses of the crust are thrust into, or founder in, the vitreous substratum.

The development of batholiths in orogenic belts was one of the earliest discoveries of earth science, and since Hutton's time has been accepted as proof of major, vertical displacement of melted rock. Why most batholiths are acid, granitic, is still a mooted question, but the best answer seems to be suggested by the twentieth-century proofs of extensive horizontal displacement of solid crust rocks towards each of the orogenic belts. This displacement is indicated by the arcuate ground-plans of the completed mountain-structures, by the nappe phenomena, and by the juxtaposition of geosynclinal facies originally distant from each other. Whether due to the earth's contraction or to the forceful, horizontal migration of independent blocks of continental size, each of the displacements involves an enormous addition of the solid material of the crust to the corresponding geosynclinal belt. The old view that this material or its equivalent volume of rock was locally piled up in excess on the earth's body, immediately giving each mountain-structure height to match, seems not to be in accord with the facts. For several young chains of mountains are still largely submarine, and others, somewhat older, attained their actual heights long after folding and thrusting had ceased. There appears to be only one alternative: the excess crust material sank into the earth's body at about equal pace with the orogenic paroxysm. In other words, the horizontal displacement of crust rock *towards* the geosynclinal belt was accompanied by vertical, downward displacement of crust rock *in* the belt. Such subsidence must be by downthrusting or downpulling, with the development of deep mountain roots; or by foundering of large pieces of the crust; or by both processes. If the material beneath the crust were crystalline and denser than the crust, neither downthrusting nor foundering on the required scale would be possible. Both are possible if the outer earth-shells are constituted as shown in Tables I and II.

Furthermore, it is worth noting that continental migration through the distances credited by Argand, du Toit, Heim, Holmes, Staub and Taylor, and even the horizontal displacements of the crust credited by Kober, who retains the contraction theory of mountain-building, are alike incredible unless the crust at each orogenic belt can sink deeply into a subcrustal layer. Hence, without assuming great weakness for this layer and also for it a density no greater than the mean density of the crust, both the contractionist and the migrationist are in trouble. Both are in still more trouble if they assume the horizontal displacement of crust rock to depend upon the horizontal scission, shearing, of strong crystalline rock from strong crystalline rock through the long distances and over the wide areas demanded. On the other hand, the resistance to such displacement is incomparably less if the crust moves over hot glass with the expected, little or no strength.

When there is strong horizontal displacement of the crystalline crust over the substratum, the crust yields at a geosynclinal prism. In part the roots of the mountain structure generated along that zone are due to downwarping of Sial and crystallized Sima. In part the local crust is broken into huge blocks, which founder in the substratum. This process may be called *major stoping,* to distinguish it from the ordinary *piecemeal stoping* at batholithic contacts. Major stoping means the local invasion of the crust by extraordinarily great volumes of substratum material, which at the higher levels loses much of its viscosity and becomes more typically magmatic basalt. Thus we have *abyssal injection* of the crust and that at maximum.

The foundered blocks and the deeply sunken roots of the new mountains are heated by the substratum, in which the temperature increases downwards from about 1,300° to a higher temperature, fixed by the thermal gradient (perhaps in average as much as 10° per kilometer of descent for some distance). There the immersed crystallines are selectively fused. The first of the secondary liquids melted out of the basic rocks are derived from the minerals that go into mutual solution at comparatively low temperatures. Sialic rocks will undergo this *pure-melting* at temperatures well below even the lowest temperature of the substratum. On account of viscosity true abyssal assimilation will be subordinate to pure-melting, but is also to be considered. All derived liquids are less dense than the basalt of the main abyssal injections and rise through it, to invade the mountain roots, which already inclose chilled, solidified apophyses from the basalt. These more salic bodies, due to the self-cleansing of the primary basalt, are bottomless in the sense of lacking floors of crystallized rock. For them the name *major abyssoliths* (Greek, *abyssos,* bottomless) is proposed.

Thus one important class of batholiths is speculatively explained, though in my opinion the word "batholith" should be defined without reference to its mode of emplacement.

The imagined mechanism implies that the magmas invading the mountain-structure during the prolonged petrogenetic cycle should be in the general *eruptive sequence* from basic to acid—the order actually observed. It involves also the important principle of *resurgency,* that is, the rise of both liquids and gases that had belonged to the now melted and assimilated parts of Sial and Sima.

In comparison with basalt, acid rock shows a greater volume change in melting and almost certainly has a smaller latent heat. Hence, if pure-melting of Sialic rock takes place at the substratum levels, and if the resulting liquid rises high into the mountain roots, it must be *superheated* considerably more than the primary basalt, risen to the same high levels. Have we here a partial explanation of the "caustic," replacing action of many batholiths on the intruded formations? I ask this even though we have so little field evidence of superheat in flows of rhyolitic lava.

Syntexis (pure-melting plus assimilation) is, then, supposed to be largely concentrated in the substratum itself, but should be expected also at all higher levels within the masses of primary liquid injected into the crust. Sediments as well as older igneous rocks will be so affected.

The suggested scheme implies a genetic classification of nearly all of the igneous rocks (here omitted).

Some Objections: Finally, a word about certain arguments against the crust-substratum hypothesis. That supposed to derive from the high velocities of earthquake waves below the 60-kilometer level has lost practically all its force since Bridgman measured the compressibility of tachylite. The objection that the hypothesis implies danger of general catastrophic foundering of the crust fails to recognize the strength of the crust as ample security against that danger. Jeffreys, Joly, Richardson and Kirsch are others who agree with this judgment. Foundering occurs only where the crust rocks are wholly immersed in the substratum, but immersion is difficult and needs the coarse brecciation of the crust at orogenic belts.

The repeated objection founded upon the high rigidity of the outer shells is likewise fallacious. The actual rigidity is relative to the smallness and periodic character of the stresses set up by the passage of earthquake waves, the wave of the body tide and by the elastic reaction of the globe to the Eulerian nutation. The stresses persist for the limited times because of the earth's viscosity, a quantity all the greater because the stresses are small, as Adams and Williamson found when experimenting on hot glass. In any case, the proof of rigidity gives no information about strength, so that there is no evident ground here for doubting the vitreous state of the basaltic substratum and of the shells lying between it and the iron core.

CONCLUSION

We have briefly surveyed an old problem, weighted, as few others are, with fundamental meaning for geology. A problem it will long remain. Cosmogonic theory, seismological results, study of thermal gradients and of isostatic adjustment, like the multitude of facts of tectonics and petrology, all seem to support a thesis: Our planet is still too hot to crystallize at any depth greater than about 80 kilometers or 50 miles. But the support is not proof, nor is any theory of the earth to be absolutely demonstrated. As usual in the leading questions of science, we are pragmatists and search for the theory that works best. The thin-crust theory appears to work best. Yet the chief reason for putting it in the foreground is the fact that it can guide to fruitful research in the future. As never before, the geologist realizes the meaning of the ancient maxim, "deep calleth unto deep," the need of seeking in the shells and core of the earth explanation for the dramatic changes registered in its relief and visible rocks.

A PROGRAM OF MEDICAL CARE FOR THE UNITED STATES

By Dr. C.-E. A. WINSLOW

PROFESSOR OF PUBLIC HEALTH, YALE SCHOOL OF MEDICINE; CHAIRMAN, EXECUTIVE COMMITTEE OF THE COMMITTEE ON THE COSTS OF MEDICAL CARE

IN every field of human activity we are to-day facing one common problem—the problem of adjusting our social order to the altered conditions produced by a revolution in technology. "Neither do men put new wine into old bottles; else the bottles break, and the wine runneth out, and the bottles perish: but they put new wine into new bottles, and both are preserved."

The traditional relationship between a physician and his patient, for example, was admirably adapted to the conditions of a century ago. It was indeed one of the finest and most fruitful examples of human relationship which one could well imagine. The physician practiced an art over which he had complete control and mastery. In his head he could carry all the knowledge then in existence and in his black bag all the paraphernalia available for the healing of the sick. His profession was a priestly mission, not a business. He cared for all who were in need of his ministrations. Those who could pay nothing paid nothing and those who could contributed in proportion to their means for the support of an honored servant of society. The physician and his patients were neighbors and friends. They knew each other intimately and for a lifetime, and the adjustment between service and recompense in a given case was made almost automatically.

This is, of course, an extreme statement of the actual situation; but it represents an essential ideal which underlay the relation between the doctor and his patient at the dawn of the machine age. It is a relationship which still exists to-day. Most of us know physicians and patients who maintain contact on the same elevated plane. Yet it would be safe to say that such a relation is rare and is becoming more rare with the passing of the years. This is due first of all to changes in medicine itself. The old art is now also a new science. No longer can one man understand it or practise it by himself. There must be specialists and consultants of many and diverse kinds (some twenty-five such specialties are now recognized). There must be well-equipped and costly hospitals. There must be nurses. There must be laboratories and laboratory technicians. There must be physiotherapy, devices of numerous kinds and special experts to use them. There must be x-ray machines and radiologists.

Equally fundamental in their influence upon the older ideals of medical practise are the changes produced by technology in the general social order itself. The old neighborhood life has gone and with it the intimate and prolonged personal contacts which made the old relationship between physician and patient simple and easy of attainment. Still more deeply is this relationship affected by the subtle forces of a society dominated by the profit-motive in which as John Dewey has pointed out in "Individualism Old and New," old motives of social responsibility have disappeared and new ones have not yet been developed to take their place The physician finds himself half priest and half business man, a servant of society in a world which has ceased to recognize service except as measured by financial return, a business man in a field where the fundamental requirements of basic human need preclude the application of ordinary principles of economic individualism.

It is characteristic of the forces which dominate our civilization that the impulse which actually precipitated a broadly conceived study of this vital social problem was largely an economic one. It was primarily wide-spread complaint of the financial burden of illness on the one hand and legitimate dissatisfaction on the part of the professions and agencies furnishing medical service on the other which led to the formation on May 17, 1927, of the Committee on the Costs of Medical Care. From the first, however, the committee realized that it was impossible to consider costs without considering quality as well and it has set as its ultimate goal the "development of preventive and therapeutic services in such kinds and amounts as will meet the needs of substantially all the people" and the provision of such services "on financial terms which the people can and will meet, without undue hardship, either through individual or collective resources."

The committee, which made its final report on December 29 last, was composed of 48 members under