"as mad bulls in Ye Olde Morphological China Shoppe." In my opinion, the sound of breaking chinaware is loud enough in this stimulating book to irritate many a "classical morphologist"! But perhaps, as the author claims, we need a very different approach to fundamental morphological and taxonomic problems, and this may result in "a satisfactory answer in cases where the Old Morphology has failed."

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The Domestic Pigeon

This remarkable volume, **Encyclopedia of Pigeon Breeds** (T.F.H. Publications, Jersey City, N.J., 1965. 790 pp., $30), presents what is certainly the most complete coverage ever assembled of the breeds of the domestic pigeon; it was written by Wendell M. Levi, an acknowledged expert in the field. Levi describes and illustrates in color (768 color photographs from life of the best typical specimens) all of the breeds and "sub-breeds," or color varieties of breeds, of domestic pigeons. The illustrations maintain a higher level of excellence than one usually finds in reproductions made from color photographs. For each breed the text provides the country of origin and, where known, the genetics of origin as well; the common name in English, German, and French as well as in the language of the region in which the breed originated; a description—"ornaments," which apparently refers to crests, feathers on toes, and the like, and colors or color varieties; and comments on rarity or abundance. In many cases, notes on propagation and commercial value are included.

The bulk of the volume is taken up with this illustrated catalog, but there are also some 16 preliminary, short chapters which treat the needs and care of pigeons and tell how to build their "houses," flypens, and coops; these chapters also consider the ailments to which these birds are subject and provide a general account of pigeon genetics, a classification of hereditary characters, both morphological and behavioral, and even a list of "mysteries," chiefly color patterns still awaiting analysis.

The author sticks strictly to his subject, the breeds of domestic pigeons, and does not even discuss the species of wild pigeons. Within the limits of its subject matter, this volume will certainly be the leading reference work for years to come.

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Continental Drift: A Reconsideration

James Gilluly

A Symposium on Continental Drift (Royal Society, London, 1965. 333 pp., $25), a large quarto volume reprinted from the *Philosophical Transactions of the Royal Society*, is the record of a symposium held in London on 19 and 20 March 1964. Approximately 50 geologists and geophysicists contributed to the symposium in person or by correspondence. Many papers are only slightly reworked from previously published versions; others are new and present novel arguments. Despite the repetition of old ideas, the book is useful in that it brings together much widely scattered material. The symposium was organized by P. M. S. Blackett, Sir Edward Bullard, and S. K. Runcorn.

Continental drift has been the subject of lively, often vitriolic, debate for half a century. In this introduction, Blackett traces the idea back to von Humboldt, about 1800, who was much impressed by the complementary patterns of the Atlantic coasts of Africa and South America. The evidence of glacial deposits on the equator and evaporites in the Arctic forces us to choose between tremendous climatic fluctuations (if the continents have remained fixed) and continental drift (if climatic zonation has been stable). Since we know from the great transcurrent faults and the crustal shortening recorded in the folds and thrusts of the great mountain chains that parts, at least, of the continents have moved considerable distances, and from the records of the drastic climatic modifications of the Pleistocene that climatic factors have ranged widely, there can have been no absolute fixity either of continents or of climatic zones. Blackett regards the recent advances in paleomagnetism and oceanography as grounds for a review of the problem.

The symposium embraced four main topics: continental reconstructions, horizontal displacements of the crust, convection currents and the evidence for their existence, and the physics of convection in the earth's mantle.

Continental Reconstructions

S. K. Runcorn, starting from the assumption that the earth's magnetic field, when averaged over a considerable but unspecified time, is that of an axial dipole, traces the pole positions inferred from European and North American rocks back into Precambrian time. Few geologists will feel that the Precambrian correlations are firm enough to base convincing arguments upon. Although the scatter diagrams of pole positions are commonly diffuse, they are nevertheless persuasive of systematic change during the Phanerozoic, with the poles inferred from North American data falling systematically farther west than those inferred from European data, at least from Cambrian through Triassic time. The differences increase with time—a surprising result, for one would expect the difference to have remained constant prior to the start of the drifting, presumably in the Mesozoic.

T. S. Westoll reviews the arguments from approximate potential fit of continental margins around the Atlantic, the rias coasts of Europe, Newfound-

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land, New England, South America, and Africa. The considerable widths of the continental shelves give plenty of room for uninhibited extrapolations. During the early Paleozoic, parallel but distinct geosynclines separated by a median geanticline existed in both eastern North America and Europe. Had the Atlantic been in existence at that time, it is hard to see why the quite distinct faunas of the parallel troughs would fail to exchange some elements. They did not make such an exchange. Westoll is impressed with the agreement between Ma's climatic zoning based on corals and the pole positions for the same periods independently inferred from paleomagnetism. He accepts the argument from the midoceanic ridges and their rifts that the Atlantic is a new ocean, now spreading away from the mid-Atlantic ridge. He also thinks that the granitic Seychelles could not have developed in place but represent a continental fragment left behind during drift. He concludes with the reasonable summary that no single item of geological evidence can be considered proof of drift, but that the many suggestive items add up to a fairly strong case.

One of the most striking papers of the conference is that by Sir Edward Bullard, J. E. Everett, and A. Gilbert Smith on the fit of the continents around the Atlantic. If the continents have drifted as rigid bodies, their predrift relations can be restored by rotation about a radius through some point on the surface (Euler's theorem). Matching fairly closely spaced points on the 500-fathom depth contours of Africa and South America by successive approximations by computer, the continents can be matched with an average misfit of 88 km (maximum overlap, 140 km; maximum gap, 340 km). This fit neglects the Niger delta (a post-drift addition) and the junction of the Walvis ridge with Africa (a young tectonic feature). If we omit Iceland and the ridges joining it to Greenland and the Faroes, and rotate Spain northward to fill the Bay of Biscay, North America, Greenland, and Europe may be grouped to yield a misfit of 52 km (maximum overlap 180 km; maximum gap, 140 km). If we omit Iceland, Mexico, Central America, and the West Indies and rotate Spain, the boundaries of both North and South Atlantic can be matched with a misfit of about 130 km.

The authors concede there is no statistical criterion, but they consider these fits too close to be accidental. Certainly they are impressive on the Mercator projections that display them. If the fits are indeed significant, it is worth pointing out that they put a quietus on the recently popular but unsupported suggestion that the earth is expanding rapidly. Distortion of the continental plates demanded by expansion would vitiate a fit. This point was also made by Westoll. Despite the impressive fits of the subsea contours, it will still be a highly subjective matter to join the respective structural trends of the continents; South African land is still nearly 800 km from the nearest land in South America and the Falklands.

M. G. Rutten shows his faith in paleomagnetism, citing what he considers consistently differing pole positions derived from Permian rocks north of the Alps and from those in or south of the Alps. Fitting the Tyrolian rocks into the magnetic field recorded by the rocks of northern Europe would demand an original site in northwestern India (according to van Hilten and de Boer) or in southern Iran (according to Findhammer and Guicherit). The scatter of the data leaves this argument far from compelling.

R. M. Shackleton points out that the minute changes in shape of the continents implied by the matching on Bullard's maps would not hold true far back into the past for the shapes of all continents must surely have been considerably modified by crustal shortening in orogenic belts.

K. M. Creer, J. H. Taylor, A. L. Hales, W. B. Harland, and A. E. Nairn also spoke on this subject.

Horizontal Displacements

By matching linear magnetic anomalies on either side, V. Vacquier reviews the evidence of great strike-slip movements in the northeastern Pacific. The magnetic discontinuity of the Mendocino fault can be traced inland to 115°W, but patterns of the opposite sides cannot be matched on the continent nor do continental structures show any discontinuity along this projection. I would like to point out that the San Andreas fault cannot be followed offshore either; there must be complete decoupling of continent and ocean floor at the border.

C. R. Allen considers the large transcurrent faults in New Zealand, Chile, California, Alaska, and the Philippines and shows that some faults hitherto interpreted as thrusts are really transcurrent. None of these mentioned are comparable in inferred displacement to the oceanic faults discussed by Vacquier.

B. C. Heezen and Marie Tharp present a map showing seven transcurrent faults that offset the mid-Atlantic ridge an aggregate distance of 700 km in a ridge-crest length of 4° of latitude. Comparable faults are also common in the Indian Ocean. They attribute the midoceanic ridges to expansion of the earth, rather than to convection, for the distribution of the "micro-continents" of the Indian Ocean does not conform to what we would expect to find if the currents were diverging from the ridge. Finally, they make this interesting suggestion—inasmuch as currents greatly affect the rates of pelagic deposition, the pattern of thickness distribution of ancient pelagic sediments may furnish clues to climatic zones at the time of their deposition.

H. W. Menard thinks the pattern of the oceanic rises is not so much midoceanic as circular about the shield areas. The high heat flow and abundant submarine basalts associated with the ridges show them to be areas of exceptional heating. The many strike-slip displacements of the rises seem to him to favor Elsasser's suggestion of advective mantle currents, for vertical convection should yield long horizontal rollers along the rise and not much differential horizontal motion. The rises, he thinks, result from currents set up by continued differentiation of continental material from the mantle; the dense residue sinks, setting up currents toward the continental nuclei. These stretch the crust, thereby bringing about upwelling in the tension cracks to form the rises. I find this suggestion weak: (i) there is no evidence that the shield areas are being added to from below; if continental addition is going on it is more likely in the circum-Pacific orogens and (ii) all crustal rocks are very weak under tension, and the transfer of tensile stresses for thousand of kilometers demanded by this hypothesis seems most unlikely.

One of the few severe critics of drift to speak, J. L. Worzel, recalls that the geologic evidence offered so enthusiastically in support of drift has frequently been rejected on the basis of equally cogent arguments. In computer reconstructions it has been necessary to discard Mexico, Central America, the Caribbean Sea, and the West Indies, all of which have pre-Mesozoic rocks! The velocities of approximately 7 km/sec beneath the ridges have been attributed to the conversion of mantle material to new crust, but the same velocities are common beneath the ridges of the island arcs associated with deep trenches. The oceanic crusts of all the oceans are uniformly stratified, with the second layer varying in thickness only by 50 percent and the third layer by only a little more than 10 percent. How could uniform layering have occurred if the crustal matter flows around the continents from the Pacific into the Atlantic, or, for that matter, if it merely flowed laterally from the midoceanic ridges? Another puzzle is the mechanism by which oceanic crust equal areally to the whole Atlantic could have been eliminated from the Pacific when an almost identical crust was forming in the wake of the drifting continents under vastly different conditions. The heat-flow data do not point so unambiguously as was once thought (and as was stated by Menard) to the oceanic ridges as areas of high heat flow. In some places on the ridges the heat flow is normal and elsewhere even subnormal; it is high near volcanic areas and low elsewhere, just as in the rest of the world. The volume of oceanic sediments also seems inconsistent with drift; the Atlantic, in about 300 million years has collected sediment twice as thick as that collected by the Pacific in 2000 to 4000 million years.

John Sutton, R. W. Girdler, R. L. Fisher, D. Davies, T. F. Gaskell, and E. R. Oxburgh also spoke on this problem.

Convection Currents and Continental Drift

J. T. Wilson states that oceanic islands increase in age from the midocean ridges toward the continents and the andesite line, with a maximum age of Jurassic. Several pairs of aseismic ridges diverge from the mid-Atlantic ridge to join the continents at points that would match on Wegener's scheme. Holmes has shown that each such pair of ridges tends to have distinctive chemical characteristics. Possibly these lateral ridges are streamlines of the convection cells that carry the continents apart. (In a later paragraph Wilson suggests a wholly different theory: Ridges such as the Walvis and Rio Grande may diverge symmetrically from the midocean ridge because they were formed by eruptions from a central volcanic source that migrated along the ridge.) The radiometric and chemical evidence presented in support of these contentions seems to me rather tenuous. Wilson is skillful and fertile in hypotheses, but the "precise geological observations" that he offers in support seem far from convincing to a geologist, however appealing they may be to a geophysicist. Several of Wilson's "facts" were disputed by Menard during the later discussions; more recently, the discovery of Miocene rocks on the mid-Atlantic ridge weakens his first argument.

Jean Goguel declared that whether or not Alpine nappes record crustal shortening, the great transcurrent faults present unequivocal evidence of large horizontal displacements of the crust. At the time of Wegener and Argand it was generally believed that orogenic cycles were short and distinct; their matching across oceans might thus give evidence for former continuity. We now know, however, that tectonic deformation is long lasting, at variable speeds, with paroxysms that occur at different times in different places along a range. For distant continents the periods of deformation, even in the broad sense (Alpine, Variscan) may be very different. Temporal matching of orogenic belts is thus no longer a strong argument for former continuity, nor does a temporal mismatch disprove it.

The existence of isostasy demands an asthenosphere and creep extending to considerable depths. Seismologists have estimated the power involved in seismic activity at 10^{25} erg/yr. This is only a small fraction of the heat flow (10^{28} erg/yr), and thus the mechanical energy involved in orogeny is easily available from heat flow alone. Only two processes have been suggested for this derivation: contraction by cooling and convection. Cooling is wholly inadequate; even if it is going on, which is doubtful, the accumulation of energy of the order of 10^{25} erg/yr would demand persistence of high stresses at depths of hundreds of kilometers for 10^8 years. Convection currents seem possible, whether motivated by differentiation of the mantle or by simple thermal convection in an isochemical system, but it is difficult to supply firm parameters that can be tested. Rather surprisingly, Goguel's very thoughtful paper seems to have elicited no discussion.

G. P. L. Walker points out that Iceland lies astride a midoceanic ridge and thus offers a good opportunity to study dilation in process. Gaping tension cracks with total width of about 30 m have formed during the last 3 to 5×10^3 years. Dikes in eastern Iceland with aggregate thickness of 3 to 6 km have fed a cross-section of 80 km^2 of flood basalts. The extrapolation of this ratio to the whole island suggests dilation of 200 to 400 km. This simple picture is disputed by Rutten, who says that western Iceland shows no relation between flow thickness and dike width such as Walker found in the east, and that the structure of western Iceland cannot be explained by east-west tension.

G. D. Nicholls, J. A. Miller, F. J. Fitch, L. U. De Sitter, M. J. Graindor, and W. B. Harland also spoke on this subject.

Physics of Convection Currents in the Earth's Mantle

According to G. J. F. MacDonald, gravity and heat-flow measurements show that the structural and chemical differences between oceanic and continental regions extend to depths of several hundred kilometers. It is therefore difficult to think of relative movement between the two regions. A comparison of the figure of the earth derived from satellite observations with that calculated on the assumption of hydrostatic equilibrium demonstrates that stress differences of the order of 100 bars exist in the mantle. Were these stress differences maintained by convection, an average viscosity of 10^{26} poise is required—far too high to permit convection at all.

In discussion, Worzel points out a disagreement between sea surface gravity data and the geoid as determined from the satellite data. These show a deep depression of the geoid in the eastern North Pacific. Were such a depression present, the pendulum should show this to be a region of large negative anomalies, which it is not. As the pendulums are checked at both ends of every voyage, the surface measurements are probably better than the satellite data.

MacDonald reports a spherical harmonic analysis of the heat flow data available for the earth. His map shows that the highest heat flow on earth should occur in the Tibetan plateau. This seems to be merely an exercise in computership. With only 757 values for the entire earth and these very poorly spaced—none from Africa north of the Rand, none from three-fourths of South America, none from North America west of the Colorado Front Range, none from two-thirds of Australia nor from within several thousand kilometers of the supposed hot spot in Tibet—the analysis is far from convincing. A map of the active volcanoes of the earth would seem considerably more pertinent to the actual distribution of heat flow.

Runcorn assumes drift as proven, and on a scale such that the greatest transcurrent faults and orogenic crustal shortening are mere second-order effects, unlikely to be understood until an adequate theory for convection is found. Rebounds of Fennoscandia and Lake Bonneville from Pleistocene loading agree in giving an upper mantle viscosity of 10^{21} poise, from which the Reynolds number is 10^{-18}. Flow in the mantle is therefore surely laminar. If we assume a rigid crust and a Newtonian mantle, the present radius of the core—0.55 of the earth's radius—is close to that at which the fifth harmonic in convection cells is favored over the fourth; the spurt of Mesozoic drifting is probably due to the geologically young change from fourth to fifth harmonic cells. He assumes convection velocities that would give temperature differences of about $\frac{1}{3}$°C between rising and falling currents. With a volume coefficient of expansion of 10^{-5} per degree centigrade, he derives gravity anomalies of about 10 milligals or about 30 m in geoid height—about the size found from satellite observations. Thus the geoidal irregularities, far from proving a bar to convection, are entirely consonant with it.

This thesis is disputed by D. C. Tozer who says that Runcorn's earth model of incompressible fluids in spherical shells, in which the convection pattern is controlled by the ratio of core to surface radius is surely much too simple—" . . . such theories are almost as unconnected with our knowledge of mantle properties as the continental drift they are designed to explain. They rest on a precise but very limited mathematical theory, great simplification of mantle properties, and the ad hoc hypothesis that the core has been growing continuously throughout geologic time." Goguel also doubts the control of harmonic cells on mantle deformation.

M. H. P. Bott summarizes his recent hypothesis: because the continental crust contains more radioactive materials than the oceanic, it is a less efficient sink for heat from the mantle. Other factors being equal, convection should occur preferentially beneath the oceans. If convection is normally lacking beneath the continents, temperatures in the underlying mantle should there rise at a rate of about $100°C / 10^9$ yr because of radioactive heating. Eventually subcontinental convection would begin and move continents or parts of them onto portions of the mantle previously covered by oceanic crust.

E. Orowan presents an important analysis of mantle properties, which he bases on the assumption that the mantle is crystalline and therefore possesses Andradean rather than Newtonian viscosity. The critical value of the yield stress at which convection could occur, with plausible values of a hot column 1000 km deep and temperature difference 100°, practically agrees with the stress drop estimated for deep focus earthquakes. It therefore seems probable that convection does occur in the mantle, probably as narrow upward currents in hot dikes. Non-Newtonian convection drives the dikes toward a central position between continents.

Orowan assumes that flow diverging westward from the mid-Atlantic ridge never dips downward but carries America westward while the Pacific crust and upper mantle dip under the Gutenberg-Richter shear band on the west coast, in complete discontinuity with the Atlantic convection cell. The velocity of drift can be estimated in two ways: from the mean annual circum-Pacific seismic energy release and from the height of the mid-Atlantic ridge, whose pressure is driving the current. Both estimates give a drift velocity of about 1 cm/yr.

P. N. Kropotkin writes that paleomagnetic data have shown that the Siberian and Russian platforms approached each other by some 3000 km during the time of folding of the Urals, western Siberia, and Kazakstan. Seismic data support the convection ideas of Runcorn, Wilson, and Girdler. Measurements of latitude at Pulkova, near Leningrad, and at Ukiah, north of San Francisco, show that these points are approaching each other at the rate about 10 cm/yr.

D. C. Tozer, J. A. O'Keefe, J. Verhoogen, Sir Harold Jeffreys, F. A. Vening-Meinesz, and J. D. Bernal also contributed to the discussion of this matter.

Rutten summarizes the entire conference by saying that interpretation and analysis of the continental drift problem has not advanced much since the arguments of the 1920's. Wilson and Bullard, in order to get good fits, omit the Caribbean (with evaporites under part of its floor) and Iceland "because it is young." But Iceland is in part as old as Rockall Bank, which is retained "because it fits." Moreover, eastern Iceland has continental volcanics. The only new argument is that introduced by paleomagnetism, which seems to be better established owing to improved methods of investigation.

Bullard closes the conference, pointing out that there is no agreement about whether drift is possible, and that much depends on arbitrary assumptions about the interior of the earth. There are other phenomena such as ice ages and thunderstorms whose existence is incontrovertible but for which no theory advanced is free from serious objections. Difficulties encountered in accounting for a phenomenon do not disprove its existence, although they do indicate that some evidence is being misinterpreted. A serious difficulty with the convection theory is the difference between the observed ellipticity and that appropriate to hydrostatic equilibrium, which suggests a strong mantle. Another difficulty is in the roughly equal heat flows in continents and oceans. The heat from the continents must be largely from the crust, that from the oceans from the mantle. Had the continents moved, none of them should have stayed over its own particular piece of mantle, and the heat flows should differ. There are thus real difficulties in the drift hypothesis, but it must be remembered that they depend on properties of material about which we have only indirect knowledge. The true processes are doubtless far more complex than our theories.

It thus seems that Blackett's opening remarks are well justified. The only new evidence bearing on the problem is from paleomagnetism and oceanography. Paleomagnetism seems to carry more conviction to physicists than to geologists, but despite the special pleading obvious in many paleomagnetic papers, there remains a rather persuasive residuum. The oceanographers are divided about whether the midocean ridges arise from localized convection or from expansion of the earth. The geophysicists differ by five orders of magnitude in their estimates of mantle viscosity. The thick Jurassic evaporites of the northern Caribbean demand a seaward barrier (Africa?), but the very area involved has to be ignored in order to bring about a good "pre-drifting" fit. Mantle movements in both continental and oceanic segments having magnitude and velocity enough to justify the drift theory seem well established. So, too, is the decoupling of continental and oceanic crust in the circum-Pacific region. The probability of drift seems fairly high, but many of the arguments, both geological and geophysical, involve too much special pleading for comfort. Both geologist and geophysicist must go considerably farther before the theory can be considered either firmly established or disproved.

Enrico Fermi's Papers, 1939 to 1954

Volume 2 of **The Collected Papers of Enrico Fermi** (University of Chicago Press, Chicago, 1965. 701 pp., $22.50), edited by Edoardo Amaldi, Herbert L. Anderson, Enrico Persico, Emilio Segrè, and Albert Wattenberg, is divided into two distinct parts.

Beginning with item Number 129 (the first paper in volume 2), we have almost 100 papers that deal in one way or another with interactions of neutrons with matter, and with other problems that are relevant to the chain reaction in uranium.

The first paper, item Number 129, was published in 1939, shortly after Fermi arrived in the United States from Stockholm with his family and his newly acquired Nobel prize. (The Fermis used the trip to Stockholm as a way of escaping from Fascist Italy.) We can trace the course of events of those historic days in the transition from the publication of Fermi's papers in the *Physical Review* to the secret reports that he made to various agencies of the United States Government as the grave and fateful meaning of the subject became apparent to the Roosevelt administration. Almost all of the latter material is now declassified but not easily available. They present a story of the development of the ideas of the nuclear chain reaction—the discoveries, measurements, and calculations that it was necessary to make, or to invent, before the first chain reactor could be realized.

Paper 228, again in the *Physical Review*, marks the end of the war period and the resumption of more normal research and publication. In the intervening years, Fermi had moved from Columbia to the Metallurgy Laboratory in Chicago to Los Alamos and then back to Chicago as a professor. His subsequent papers were chiefly concerned with researches in meson physics and with cosmic radiation. For Fermi, this was another period of great contributions, both in research and as a teacher and a source of inspiration to graduate students. His enormous talents as a teacher are demonstrated by the quality of the physicists who did their doctoral dissertations under his supervision, for the roll of Fermi's students reads like a "Who's Who" of the leaders of contemporary physics.

The postwar papers, about 40 in number, were all done in the space of less than 9 years, and they represent an important section of the most brilliant and fruitful contributions to experimental and theoretical physics of the period. The chief subjects to which Fermi turned his attention were as usual those fields that were most promising for future knowledge. First of all, there were papers on meson physics (now called particle physics), cosmic rays, and the interaction of slow neutrons with matter. In meson physics he was chiefly concerned with problems of the production and scattering of pions from nuclei, notably hydrogen. His papers show not only his theoretical and experimental ingenuity but also his instinct for the important questions of physics. In cosmic rays, he presented his very fruitful theory of the origin of cosmic rays through the collision of charged particles with cosmic magnetic fields. In neutron physics, he pressed the development of the beautiful methods of neutron interference, reflection, and diffraction which have been so useful in studying both the solid state and nuclear properties, including the interaction of electrons and neutrons.

Fermi was active up to the time of his untimely death on 28 November 1954, and his last papers are dated 1954. He left his distinctive mark on the physics, and on the physicist, of two worlds.

None of these papers speak to the larger public, outside of physics and technology, on the great issues that were raised by Fermi's contributions to the release of nuclear energy. Fermi was not unaware of these issues. He knew them, he understood them, and he felt very keenly about the political and cultural implications of his great contributions.

He served on the General Advisory Committee to the United States Atomic Energy Commission where not only his scientific gifts but also his wisdom and insight had a large influence on fateful decisions. These contributions are an important part of his complete works, but unfortunately they are not included in his published works.

Fermi died the noble Roman that he was.

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Earth Science

Peter C. Badgley's **Structural and Tectonic Principles** (Harper and Row, New York, 1965. 538 pp., $13.95) covers rock deformation from single fractures to the evolution of entire mountain ranges. The book should be interesting reading for professional geologists and probably also for scientists interested in summaries of structural features and syntheses of new data on worldwide mountain building and tectonic patterns. Extensive bibliographies are appended to each chapter and suggest that the writing of the text was completed early in 1963.

This book results from the Herculean and well-done task of summarizing modern structural and tectonic concepts (no attempt has been made to discuss the subject from a historical point of view). Beginning with a discussion of modes of rock failure, mechanics of deformation, and laboratory data pertinent to the behavior of rock materials, the author launches into fold structures by discussing them from the point of view of the various classifications that are currently in use. Succinct definitions lead the reader rapidly past a swarm of terms into brief discussions of the mechanics of growth of folds and competency of materials involved, kinematic classifications (vertical uplift, gravity gliding, and the like), and on to a discussion of fold types based on their position in the tectonic framework.

In successive chapters Badgley bypasses the trees of terminology and observes the forest as a whole: jointing and fracture analysis; description, classification, and recognition of faults;