Meetings

Rubey Conference on Crustal Evolution

The writings of W. W. Rubey have been profound, original, and far-reaching; equally important have been his activities in getting other geologists to discuss their ideas openly and critically and encouraging them to reach out. In appreciation for his work and influence, a Rubey Conference was held at Santa Barbara, California, on 15 and 16 June 1973. The subject was crustal evolution. Comparative planetology, lunar science, and the origin, mechanics, and history of the earth's layered structure and larger inhomogeneities were focal points.

Human and Planetary Evolution

Lunar geophysics provided the entry. Frank Press summarized seismic results indicating a layered structure extending downward to a local zone of partial melting below 1000 km. Thermal and magnetic aspects of lunar history were then discussed, with emphasis on the problem of the moon's remanent magnetism. George Kennedy pointed out that circulation of an iron core could not produce the magnetism observed on the moon. Louis Slichter remarked that even on the earth the mechanism for producing a magnetic field is little understood in detail; in his opinion, it is not solid body oscillations of the rigid inner core, as precise observations by B. Jackson indicate that there is not enough energy in persistent core vibrations.

L. T. Silver noted that the suggested lunar profile is based on a net of only four stations in the central area of the near side of the moon. Geochemical generalizations, moreover, are based on only eight sites of direct observation, including those of the U.S.S.R., all on the near side. However, a marked chemical asymmetry has been found in the mare material. Silver then compared the moon with Venus and Mars. Limited preliminary data suggest that Venus, like the moon, has undergone extensive geochemical differentiation, while its atmospheric composition is very different from that of the earth. Mars likewise appears to be well differentiated and perhaps intermediate between the earth and its moon in stage of planetary evolution.

Paul Lowman presented results from the Apollo 15 and Apollo 16 x-ray fluorescence experiments, which imply a plagioclase-rich lunar highland crust, like anorthositic gabbro. Mariner 9 infrared spectroscopy indicates a globally differentiated martian crust, possibly basalt. Lowman suggested that the earth originally had a global sialic crust, later disrupted by sea floor spreading and subduction.

It now appears that differentiation is the rule among bodies the size of the terrestrial planets, including the moon. It seems to be caused by three kinds of processes: (i) local, large-scale regional or total surface melting in the accretional heat front prevailing during the growth of the planet; (ii) internal differentiation caused by secular buildup of radioactive heat after formation of the planet; and (iii)—more speculative but perhaps very important and in some cases even dominant—all the preplanetary differentiation mechanisms that could lead to heterogeneous preplanetary distributions, and hence heterogeneous accumulation. High temperature events, leading to local or global melting and differentiation, are implied by the results of Apollo missions, and perhaps by the existence of a crust on the earth. Our moon probably accreted with a relatively cold interior, which has subsequently been heating up to reach partial melting temperatures below about 1000 km. Surficial melting events appear to have led, at least on the moon and Mars, to differentiations that were early (ages up to 4.24 aeons are observed on the moon) and geographically extensive.

There was general agreement that similar processes were responsible for the early evolutions of the earth and its moon, Mars, and perhaps Venus. Lunar samples are petrographically similar to terrestrial igneous rocks. Major differences observed between the earth and its moon include the earth's greater volatile content, steeper pressure gradients with abundant possibilities for phase changes, and much larger thermal energy. These differences have not been satisfactorily explained.

Crustal Plates

Comparative planetology led into plate tectonics. The general idea of a global framework of relatively rigid surface plates that interact along relatively straight, seismically active boundaries was accepted as well established. The need, however, to differentiate between what we know and what we infer was stressed by John Maxwell. He challenged two assumptions in particular as oversimplifications: (i) The assumption that the oceans are youngest at the "spreading ridges" and progressively older away from these ridges. In fact, very old rocks (up to 835 million years old) that occur locally near the crests of ridges are explained so far only by highly contrived models. (ii) The assumption that magnetic anomalies can be correlated and assigned ages over wide expanses of oceans. Results of shallow ocean drilling generally favor this assumption, but few if any of the holes have as yet penetrated deep enough to assure that the oldest sedimentary beds observed do lie on true basement. In several instances the logged igneous "basement" is demonstrably intrusive.

Maxwell also observed that rates of crustal movement interpreted from magnetic correlations are embarrassingly large, about an order of magnitude larger than necessary to explain either the crustal shortening related to orogenic folding or the thickening of sediments in ancient trenches such as presumably gave rise to Franciscan rocks. Something like 90 percent of rocks at converging plate boundaries have to disappear. Finally, he pointed to the apparent general absence of deformation observed in seismic and sparker profiles across presumably active trenches containing very young, well-stratified sediments.

Warren Hamilton responded that the assumption of little deformation at the landward side of trenches was based on early, low-energy reflection profiles. He discussed new data from Shell and Phillips oil exploration surveys, across the Banda arc-trench system of Indonesia. These show the landward side of the trench to consist of a wedge of severely disrupted and imbricated sediments, above a basement that is subducting at a gentle angle with little internal deformation. The top of the wedge is exposed on many islands, and there consists of melanged sediments plus subordinate tectonic shreds of oceanic crust and mantle. The Banda system has an analog in the Cretaceous of California, where the Great Valley facies represents the landward half of the outer-arc basin, and the Franciscan complex is the melange wedge formed by the long-continuing imbrication of basin and abyssal sediments. Despite this striking similarity, a problem remains as to why extensive melanges are not found in association with many ancient orogens.

Hot Spots and Mantle Plumes

J. Tuzo Wilson next discussed hot spots and mantle plumes. Hot spots were characterized as volcanic centers 100 to 200 km across that are associated with uplifts, but which are not linked with arcs and which may or may not be associated with oceanic ridges. Some 200 late Cenozoic hot spots have been identified. Beyond warping, the uplift of a flat surface generates a minimum of three radiating cracks. Hot spot theory suggests that, as extension continues, two arms of the triad open to initiate a new ocean. The third arm, nearly at right angles to the opening coastline, becomes a sediment-filled graben, perhaps the aulacogen, recognized in the Russian and Siberian platforms by N. S. Shatsky and others, and in the northern Canadian shield by Paul Hoffman. The Ethiopian rift appears to be a blind arm of the successful Red Sea–Aden spreading arms. Other possible aulacogens include the Benue Rift of west Africa, the southern Oklahoma Anadarko-Ardmore Paleozoic basin, the Amazon Valley, the Mississippi embayment, and the Rhine graben.

Linking of the aulacogen and hot spot concepts, which were derived from completely different geological evidence and subdisciplines, is one of the most provocative recent developments in geotectonics. The genetic importance of hot spots is that they may represent the surface expression of persistent rising plumes of hot mantle material. Wilson noted that the postulate of ridges of hot upwelling material has been very difficult to explain physically and suggested that the morphology of an oceanic ridge may be the surface expression of a connected line of rising mantle plumes.

It has been commonly assumed that the plumes were fixed. It is now thought, Wilson noted, that the plumes may move very slowly relative to one another within the deeper mantle and that, at intervals, plates may change their motions. From the start of the opening of the Atlantic Ocean about 200 million years ago until 25 or 30 million years ago, Africa and South America both moved away from a mid-Atlantic ridge stationary over a row of plumes which included the Azores, St. Paul's Rocks, Ascension, Tristan, and Bovet islands. For the last 25 to 30 million years the African plate seems to have remained fixed. The mid-Atlantic ridge has accordingly moved a few hundred kilometers from the row of plumes and the westward velocity of South America has presumably doubled. North America during the Paleozoic was probably on a fixed plate with other plates moving toward it from all sides.

What could the plumes actually look like? Anomalous seismic velocities in the deep mantle beneath Hawaii support Jason Morgan's suggestion that plumes may originate at the core-mantle boundary, and Press noted that hydrodynamic arguments indicate they would have to broaden upward as they approach an upper boundary. George Kennedy observed that a plume would have to be at the solidus all the way up. David Griggs noted that where there is a density inversion in the mantle so that light material rises through heavier, a plume can come up from the mantle in pipelike form.

Cloud asked what could drive the plate-and-plume system? Most respondents felt that "coffee-cup" convection is too simple; instead, gravity probably pulls the cold, dense plates down into a less dense asthenosphere. Griggs argued that mass moving gravitationally downward along a subduction zone must continue until it reaches some density boundary beneath the asthenosphere, and equivalent mass must come up somewhere to replace that lost from the surface. The rising mass would be a plume. He suggested that motion probably extends through the entire mantle, rather than the asthenosphere only. The depth limit of seismicity (600 to 700 km) is probably not a boundary to downgoing plates, but a depth at which the slabs approach their melting points closely enough to flow.

Leon Knopoff noted that a low seismic velocity zone, widely held to mark the asthenosphere and the surface on which the plates have moved, is present everywhere except under the oldest shields on each continent. As these old shield areas appear to have moved with respect to one another, the surface on which the plates have moved must lie deeper than the low velocity zone ($\sim$350 km). This calls for a significant change in the picture many of us have had of the nature and thickness of the plates that have made the global tectonics.

Vagueness as to mechanism and anomalous geometry aside, the general view was that plate tectonics provides a powerful unifying theory of crustal evolution for about the last 200 million years. Evidence relating to pre-Triassic processes and interactions is much less satisfactory and more difficult to interpret.

Pre-Triassic Record

Discussion of this ancient record began with a consideration of the oldest dated rocks by John Sutton [leaving out an unconfirmed Soviet report of 4-aeon-old rocks in Enderby Land, Antarctica; *Sci. News* **104** (No. 3), 34 (1973)]. These are granitic gneisses from southwestern Greenland, discovered by V. R. McGregor and dated by L. P. Black, Steven Moorbath, and others as about 3.75 aeons old. They occur in a region of anorthositic gabbros and metasediments older than 3 aeons, comparable to the Limpopo anorthosite terrain of South Africa and roughly equivalent in age to the Swaziland System. Like Archean rocks almost everywhere, they are strongly folded. Indeed there are no flat-lying rocks older than about 2.6 aeons. This bears on the time of initiation of cratonization and perhaps of plate tectonics.

Impact structures in early crustal evolution came up with Hamilton's assertion that morphology and shock metamorphism show the Vredefort Ring (South Africa) to be the central part of a large astrobleme and that the Bushveld Complex could have formed as a result of three other impacts at the same time, about 1950 million years ago. Melting produced by presumably more numerous large impacts of still earlier times, he suggested, could have been a major factor in differentiating the earth. Gilluly disputed the identification of Vredefort as an impact structure, pointing to buried asymmetries. Wilson, Silver, James, Waters, and others entered the discussion, which ranged from Vredefort to the moon and Mars (craters have now also been reported from radar sightings on Venus). Silver emphasized that the connection between time of impact and ages of mare basalts on the moon was not direct. Nevertheless, major impacts could have affected the earth's early crust through local partial melting, triggering a succession of basaltic episodes.

Discussion turned to island arcs. The successive, southward-younging, east-west, greenstone belts of the Superior Province were suggested as Archean analogs of modern island arcs. James drew attention to Eric Dimroth's studies of the Labrador trough, which shows all aspects of an ancient island arc except that it built up within a continent and not at a continent-ocean boundary. (Robert Dott responded that it might be due to a continent-continent collision.)

What then are the criteria for plate tectonism and how far back are they applicable? John Bird emphasized the need to utilize the full panoply of evidence. Continental crust is not easily returned to the mantle because of buoyancy constraints, and therefore must increase in volume with time. A dual asymmetry of geosynclinal belts marginal to cratons extends back to about 2 aeons ago. Ensimatic and ensialic mountain belts date back to 1.8 aeons and ophiolites to 600 million years ago. Blueschists are reported to be as old as 1.3 aeons on the west slope of the northern Urals (V. A. Glebovitsky, at the 24th International Geological Congress, Montreal, 25 August 1972). Adding it together, Bird found plate tectonic signatures in rocks as old as 2 aeons, but not older. Cloud opined that Archean tectonic patterns indicate primarily vertical movement, a thin hot crust, no marked geosynclinal duality, and no plate tectonics as we understand the mechanism in later times. He suggested that plate tectonic mechanisms may date from the aggregation of plutons into protocontinents and the onset of cratonal sedimentation beginning about 2.6 aeons ago.

Initial Atmosphere and Hydrosphere

Gustaf Arrhenius supported Hart's earlier expressed conclusion that the entire hydrosphere evolved simultaneously with planetary aggregation about 4.6 aeons ago and that the initial atmosphere was, therefore, very dense. He visualized the initial coalescence of meteoroids as accompanied by sweating out of all the volatiles at once. Meteoritic abundances of occluded noble gases, for instance, are comparable with those of the terrestrial atmosphere. Rubey observed that the geochemical consequences of this scenario are far-reaching and evidence that it really happened should be looked for in the geologic record. If the amount of CO_2 in the present ocean, atmosphere, and crust was all in the early atmosphere at one time, it would mean a pressure of CO_2 (44.7 kg/cm^2) that was 10^5 times that in the present atmosphere. This would mean a highly acid early ocean and sediments different from those we know. Had the carbon been present as CO or CH_4, the atmospheric pressure would have been 28.5 or 16.3 kg/cm^2. Rubey has not estimated what organic compounds might form under such huge atmospheres of CO or CH_4, but there could have been no oxidation of ferrous to ferric oxide or sulfur to sulfate until these gases were oxidized. Probably no forms of life that we know could have come into existence under a CO atmosphere. It seems that we are still in firm possession of the problem.

Rubey added that the atmosphere of Venus is dense and consists largely of CO_2; CO and CH_4 have not been detected. Incidentally, no one seems to have worried much about how and when the CO_2 on Venus (80 kg/cm^2) became oxidized.

Arrhenius countered with two observations. (i) As no sedimentary records are known from the first aeon of Earth history, the sedimentary consequences of the initial atmosphere are unknown. (ii) We do not know the molecular distribution of carbon in the initial gases. It may have been present as CH_4, CO, CO_2, and cyanides.

George Tilton added that if the carbon was originally mainly CO and CH_4, with a slow addition of O_2 the *p*H might remain neutral as CO was slowly transformed to CO_2, which, in turn, became bound in sediments. (Carbonate rocks, however, are rare among the oldest sediments.) James pointed to the vast quantities of O_2 tied up in banded iron formations, especially the giant deposits formed around 2 aeons ago. Cloud briefly gave his views on the principal source (biologic) and time of appearance (after 2 aeons) of atmospheric O_2.

Finally, opportunities for future insights were discussed. The virtual absence of terrestrial records for the first aeon of Earth history points to the need for study of other planets that may be arrested at more primitive stages of development. Arrhenius noted the difficulties connected with the generation of the earth's concentric structure as a postaccretional feature, particularly the view that the earth's core formed so late that radioactive heating had already proceeded far enough to start the required avalanche mechanism. Other obstacles arise from the high nickel content of mantle olivine and indications from Venus of the irreversible course of a runaway greenhouse effect. These observations, in Arrhenius' view, rule out any complete, catastrophic melting and degassing of the earth at a time substantially later than accretion.

He postulates a catastrophic accumulation of the core, followed by a more gradual accumulation of the mantle during which each region of the growing planet would have been repeatedly melted and differentiated. An accretional heat front thus would have swept through the entire mantle, gradually accumulating a crust, while the average planetary surface temperature remained low. Volatiles carried by infalling projectiles were probably transferred to the atmosphere at impact. Thus, Arrhenius arrives at the view that the crust, the ocean, and the atmosphere are products of the earth's accumulation, not of processes beginning after aggregation.

If this is true, what is the origin of the Rubey inventory of volatiles now escaping from the earth's interior? Arrhenius' response was that they would be generated by continuous subduction of volatile-soaked rocks into the upper mantle. Press noted that if the earth were to be completely degassed at the present time, loss of H_2O would cause the asthenosphere to freeze, the crust to thicken, the solidus to drop to deeper internal levels, and geotectonics to terminate. The whole question of volatile outgassing and its geochemical and geophysical corollaries can do with further examination.

What Drives the Plates?

P. H. Abelson returned to the question of what drives the plate motions. Griggs expressed confidence that the mechanism was some form of thermal convection. The lithosphere slides downhill away from rises which are elevated because the upwelling material under them is hotter than average mantle. As it moves away from the rises, the lithosphere cools by conduction and thickens (as the solidus moves downward) as shown recently by Alan Leeds at the University of California, Los Angeles. The cooled lithosphere is more dense than the mantle and sinks at the trenches to at least 700 km, as shown by seismic data.

Griggs said the real nature of this vast convective system could not become clear until the "flow law" of mantle material is clarified. He reported on the argument of Robert Post [*Science* **181**, 1242 (1973)] that flow in the mantle is non-Newtonian; strain rate is proportional to the third power of stress. This type of behavior is found in dunite in the laboratory and it explains the Fennoscandian postglacial uplift better than Newtonian viscosity. Arrhenius responded that an alternative to this flow theory has been developed by Øyvind Gjevik at the Institute of Geophysics and Planetary Physics at the University of California, San Diego, who suggests that depression under the ice load and rebound subsequent to its removal is due to the olivine-spinel transition at depth, with relaxation being controlled by the advection of heat involved in this transformation.

Press commented that a flow process, rather than compression, would seem to be supported by the indication of partial melting in the asthenosphere. The crucial question remains, is the system convecting because of plate motions or the reverse? One possibility is that the lithosphere grows as the cooled boundary of a convection current. Another is that earlier expressed by Griggs—plates slide downhill away from the oceanic rises. The plates, however, do not move gradually but by jumps. They stick along their margins, and when they come unstuck and slip they produce earthquakes.

Press asked, what happens to the descending slabs? Plate motions have been directly detected and delineated. For instance, in the big Alaska earthquake in 1964, *P* waves arrived in Europe 6 seconds before the United States, presumably because they traveled along a deep-descending cold slab on their way to Europe, but followed the usual path to the United States. Although earthquake epicenters are not observed below 600 or 700 km, descending plates may not disappear at this level.

Outstanding unsolved problems include (i) the mechanism of plate motions, (ii) the location of the olivine-spinel transition in subducted slabs, (iii) the delineation of plate tectonic motions before 200 million years ago, (iv) the mode of aggregation of the earth and the origin of its concentric structure, (v) the time and manner of outgassing of atmosphere and hydrosphere, and (vi) the nature and geochemical consequences of atmospheric, hydrospheric, and biospheric evolution.

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Note

1. Persons attending the conference included P. H. Abelson (Carnegie Institution of Washington, Washington, D.C.), G. Arrhenius (Scripps Institution of Oceanography, La Jolla, California), J. M. Bird (Cornell University, Ithaca, New York), P. Cloud and J. C. Crowell (University of California, Santa Barbara), R. H. Dott, Jr. (University of Wisconsin, Madison), G. Ernst (University of California, Los Angeles), J. Gilluly (U.S. Geological Survey, Denver, Colorado), D. Griggs (UCLA), W. Hamilton (USGS, Denver), S. R. Hart (Carnegie Institution of Washington), H. L. James (USGS, Menlo Park, California), G. Kennedy and L. Knopoff (UCLA), P. D. Lowman, Jr. (Goddard Space Flight Center, Greenbelt, Maryland), J. C. Maxwell (University of Texas, Austin), F. Press (Massachusetts Institute of Technology, Cambridge), W. W. Rubey and J. W. Schopf (UCLA), L. T. Silver (California Institute of Technology, Pasadena), L. B. Slichter (UCLA), J. Sutton (Imperial College of Science and Technology, London), G. Tilton (UCSB), A. C. Waters (University of California, Santa Cruz), and J. T. Wilson (University of Toronto, Toronto, Canada). Bird, Dott, Ernst, and Lowman served as recorders for the conference. Their notes, plus reactions to an early draft from many others, contributed importantly to the preparation of this summary. This conference was sponsored by the Carnegie Institution of Washington and organized by a steering committee consisting of Abelson, Cloud, Crowell, and Ernst.

Radiation Chemistry of Condensed Phases: Report of a Joint Japan–United States Seminar

When high-energy radiation impinges on any system a spectrum of excited states is formed; these immediately decompose and set in train a complicated series of processes, which eventually result in the observed chemical and physical changes. It is the business of radiation chemistry to categorize and describe these processes as they occur in various types of material, so that radiation effects in actual biological and industrial systems can be understood. The processes occurring in gaseous systems are now well understood in principle and have been studied in some detail, but those in liquid and solid systems are more complicated and many aspects are still uncertain. The purpose of this seminar, which was sponsored by the National Science Foundation, was to review and discuss current American and Japanese research bearing on this subject.

The participants included 17 Japanese and 15 American scientists. The meeting was held at the Marine Laboratory of the University of Southern California on Catalina Island on 5 to 9 February 1973. The material systems discussed included water and organic compounds in their crystalline, glassy, and liquid states, and organic polymers.

The spectrum of excited states produced by electron bombardment of simple hydrocarbons has been determined by W. H. Hamill (University of Notre Dame, Notre Dame, Indiana) by looking at the energy losses of electrons scattered out of a beam on passage through a thin film of hydrocarbon deposited at 77°K. The method is a powerful one, but so far only a beginning has been made in characterizing the states and transitions which they undergo. A more detailed characterization of the behavior of the lower excited states is obtained by studying fluorescence of hydrocarbons induced by light in the far-ultraviolet. S. Lipsky (University of Minnesota, Minneapolis) reported on luminescence obtained from saturated as well as aromatic hydrocarbons. The factors that determine whether an excited state should ionize or not were discussed on a theoretical basis by T. Watanabe (University of Tokyo). So far, theoretical treatments are available only for the simplest molecules, such as methane.

When ionization does occur, the fate of the electrons produced depends strongly on the nature of the medium. In polar liquids the electron usually becomes solvated—it forms a shell of molecules that move with it so that its properties resemble those of an ordinary ion. The optical and chemical properties of the solvated electron in various ethers were reported by L. M. Dorfman (Ohio State University, Columbus). Solvated electron states may even exist in saturated hydrocarbons, but their stability may be so low that an equilibrium occurs between the solvated and "quasi-free" state (A. O. Allen, Brookhaven National Laboratory, Upton, New York). The result is that the electron energy and mobility are extremely sensitive to the detailed molecular structure of the hydrocarbons. Y. Hatano (Tokyo Institute of