Meetings

Pacific Science Congress

The largest scientific meeting ever held in Japan was the Eleventh Pacific Science Congress at the Tokyo University, 22 August–4 September 1966. There were nearly 6000 registrants from 82 countries, including 2200 non-Japanese of which 800 were American. This report attempts only to touch upon some of the geologic aspects of this complex and multipurpose congress.

Several sessions were devoted to the orogenesis of the Pacific area. Without doubt the most popular word used by these speakers was "geosyncline" with its many sesquipedalian variants. Unfortunately the term means many things to many people, thus resulting in much semantic confusion. While it was universally agreed that folded mountains develop from thick geosynclinal prisms, geologists now examining the *corpus delicti* do not agree as to the nature of the geosyncline when it was alive. Was it a deep sea trench, a continental-swale or a continental-rise sedimentary prism? Although great progress was apparent in regional geologic studies, and especially so by the Soviets in Siberia and Kamchatka, I received the impression that the fundamental principles behind mountain-making and continent-building need still to be elucidated. Most land geologists seemed quite oblivious to the recent basic discoveries of marine geology which call for drastic revision of our basic concepts. And the geologic reconstructions presented seemed to ignore the basic dichotomy between continents and ocean basins in both crustal structure and topographic position. It would seem that orogenesis remains a branch of science where the basic principles are still largely optional.

Yu. Pushcharovsky (U.S.S.R.) showed us his new tectonic map of the Pacific Basin region. His interpretation is that this hemisphere is characterized by a central immobile craton occupying the central Pacific, inside the so-called volcanic ring of fire, which is a Precambrian "thallasocraton." Forming annular rings around this oceanic craton are orogenic foldbelts of progressively older age. Pushcharovsky believes the inner oceanic belt to be a "living" geosynclinal region of Cenozoic age which is in the process of becoming continental. Older annular foldbelts of Mesozoic to Precambrian age have already been incorporated into the continents. N. Bogdanov (U.S.S.R.) offered quite similar views for the western Pacific alone and argued that the central Pacific Basin is an immobile craton of Precambrian antiquity. In contrast, I suggested that such views are inadequate to account for the circum-Pacific belt and that we must use the additional dimension of continental drift whereby the Pacific is the remnant of the once universal ocean (Panthalassa) with the other oceans being largely rift basins. The encroachment of the circum-Pacific continents upon the Pacific Basin and the underriding of these sialic plates along the trenches by oceanic crust would then account for the circum-Pacific orogenic belt.

An important new finding, and the subject of much discussion, is that (like the Kurile Basin) the Sea of Japan, between the Japanese Arc and Asia, has an oceanic crust, thus making it a "window" into the earth's upper mantle. This depression is also geologically young, certainly post-Jurassic and perhaps even Neogene, while some of the rocks of Japan go back to the Silurian. A spectrum of views was offered to account for this great hole in the Asiatic framework. One speaker suggested that the region may have been uplifted in a great dome, permitting the erosion of 35 kilometers of continental crust; then it subsided to its present oceanic depth. Others insisted that the region offers a fine example of oceanization, the conversion of sialic continental crust into oceanic simatic crust by some not clearly specified process (the most popular opinion and one especially preferred by the Soviets). Still others spoke of Japan as being displaced out from Asia in some sort of drifty manner, thus leaving the Japan Sea basin as the gap.

A new set of bathymetric maps of the Aleutian Arc by H. Nichols and R. Perry (Institute for Oceanography, ESSA) was displayed and described. This large map consists of six sheets on a scale of 1:400,000, has a contour interval of 50 fathoms, and is based upon 275 separate Coast and Geodetic Survey surveys executed between 1942–64. The basic geomorphic elements of this prime example of an island-arc system appear to be a trench paralleled by a deep terrace inside of which lies the Aleutian Ridge with its festoon of islands, and behind which are several volcano-tectonic depressions. Spoiling the arc's symmetry, is the unique hook-shaped Bowers Ridge projecting into the deep Bering Basin. The Bowers Ridge intersection seems to be a major dislocation. For example, the aftershocks of the 1965 Rat Islands earthquake affected all of the arc to this dislocation but not eastward of it. Perry suggested that the Aleutian Ridge has been formed by a conveyor belt or sea floor spreading type of movement of the Pacific floor whereby the trench marks the locus of downwelling of the oceanic crust. The ridge then marks the site of rising magmas which are the hyperfusibles ascending from this underriding plate. This new map is a major contribution to oceanic bathymetry and provides an excellent base for future geologic and geophysical studies.

E. Hamilton (U.S. Navy Electronics Laboratory, San Diego) described the marine geology of the Gulf of Alaska, using seismic profiling data. The abyssal plains have been created by turbidite deposits (150 to 600 meters in thickness) which moved out from the continental margins through well-developed, deep-sea-channel systems. These channels are leveed with the higher banks on the right side, thus suggesting deflection of the turbidity current streams by the earth's rotation. The thickness of the turbidite blanket forming the Aleutian abyssal plain is such as to suggest that it was formed before the Aleutian Trench, leaving it now as a starved or "fossil" abyssal plain with no present source of sedimentary sustenance. The moats around several hills and seamounts, originally described by Menard and Dietz as isostatic depressions, were

found to have no expression in the deeper crust. They appear to be purely sedimentary features perhaps carved by the speedup of currents around the seamount as a bottom obstacle.

R. Verma and H. Narain (India) reported on detailed paleomagnetic studies of Indian rocks. The results are consistent with India being positioned in the southern hemisphere from Permian to Eocene time. Continental drift of the Indian subcontinent seems to have occurred mostly from Jurassic to Miocene times covering a distance of nearly 4000 miles. The impingement of India against Asia apparently played an important role in the uplift of the Himalayas. The geomagnetic field probably was reversed during the Lower Cretaceous while it was normal during the Upper Permian.

A rather remarkable development over the past decade has been the growing realization, paced by radiocarbon dating, that even around the diastrophically active Pacific margin eustatic sea-level oscillations (and especially a Recent rise of perhaps 120 meters) generally have overwhelmed diastrophic uplift and subsidence. But in the symposium on Pacific margin sea-level changes, George Plafker and Meyer Rubin (U.S. Geological Survey) reported that, in the Gulf of Alaska, tectonic displacements locally have been orders of magnitude greater than the postulated sea-level changes. Radiocarbon-dated samples in coastal areas affected by tectonic movements during the 1964 Alaska earthquake indicate: (i) uplift relative to sea level of as much as 55 meters in the past 7650 years and subsidence of at least 4.5 meters in the past 2800 years; (ii) 40 meters of relative uplift of Middleton Island in five major upward pulses during the last 4500 years, the most recent pulse probably having been less than 460 years ago; and (iii) a gradual general submergence of the coast for several centuries before the earthquake, at rates as high as 4.3 meters in 400 years. Striking evidence of continued active diastrophism along the Gulf of Alaska was provided by the regional coastal uplift of as much as 11.5 meters and the subsidence of 2.3 meters that accompanied the 1964 earthquake.

The next Pacific Science Congress will be held in 1971 at Canberra, Australia.

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Gastrointestinal Circulation

More than 15 years have passed since there has been an international meeting devoted to the gastrointestinal circulation. In the interim, advances have occurred in our understanding of the physiology of the circulation in salivary glands, stomach, and the intestine. Simultaneously, knowledge has been accumulating about other areas of the circulation and in methodological fields upon which research into the circulation must depend.

A meeting of investigators representing four continents met at the Lake Arrowhead (California) Conference Center on 5–9 September 1966. There was a total of 13 topics covered, and these fell into two general categories: first, subjects in the area of rheology and advanced methods of measuring blood flow and, second, topics relating to specific regional circulatory problems. The format of this conference was such that there were two formal speakers for each subject to present opposing points of view where possible, yet more than half of the formal meeting time was devoted to open discussion and questioning by all participants. The meeting opened with a paper entitled "Bioenergetics, why?" by Wilfried Mommaerts (UCLA School of Medicine), which was a philosophical essay on the nature of life from the point of view of a biochemist. Mommaerts takes issue with that school which would define life solely in terms of a double-helical molecule. He also managed to prick the balloon of those whose apparent aim is to replace the irritations of existence with a vegetable state held together by artificial organs. His neoclassical redefinition of biology will be reassuring, at least to a medical reader.

The first technical paper was a broad review of the rheology of blood flowing in the microcirculation by Harold Wayland (Caltech). Among the interesting concepts Wayland developed and presented evidence for was the hypothesis that there is a dynamic structuring of blood as it flows through the microcirculation. This structuring appears to consist of an interaction between red cells as they aggregate and dissociate reversibly. Both Wayland and his formal discussant, Donald McDonald, agreed that the magnitude and importance of the plasmatic zone at the intimal lining of microvessels has been greatly exaggerated in both size and importance.

Michael Taylor (University of Sydney) approached the relationship between the physical structure of arterial walls and the role played by the arteries in the circulation from a novel point of view. He considered what the characteristics of the arterial walls should be if the workload upon the heart were to be minimized. He ended his discourse confirming the wisdom of the body at least with regard to engineering specifications of arterial walls. The discussant, Allan C. Burton, noted that the entry of the engineer into biology may not solve the problems of the life sciences, but it should provide engineering with many new ideas that could be applied to industrial needs.

The Fick principle has been applied in many ways to the measurement of blood flow in regional areas. One of its most recent applications was the isotope fractionation method developed by Leo Sapirstein (Stanford). Using organ uptake of radioisotopic potassium or rubidium, simultaneous estimation of blood flow in many regions can be carried out. The errors and limitations of this variation of clearance methodology were pointed out by Robert Pitts.

Alvin F. Sellers and Alan Dobson of Cornell presented a paper intended for those who employ the chronically implanted electromagnetic blood flowmeter. The authors categorized the sources of error with the instrument and described a preparation which allows evaluation of the artefacts. A major question raised by Alexander Kolin related to whether these artefacts are due to poor commercial products or whether they are an inherent flaw in the principle of this instrument.

A respiratory physiologist, Robert Forster (University of Pennsylvania), investigated the possibility of using gas exchange methods for the measurement of blood flow in the intestine. He developed a model of the enteric hollow organ and the criteria of a suitable gas, carbon monoxide. In the companion article, Selkurt and Wathen (Indiana University) gave their results with xenon-133 uptake in the intestine. Blood flow values, using these two methods, differed by a factor of 10.

John Grayson (University of Ibadan) discussed the theoretical basis underlying the use of the heated thermocouple to estimate tissue blood flow in solid organs. While taking care to define the limits of this instrument, he cited likely applications of the technique. Loren Carlson amplified the list