Obituaries

Professor Harry Hammond Hess

THE death of Harry Hess from a heart attack on August 25 was a sad loss to the earth and planetary sciences. Hess was born in New York City on May 24, 1906. He took his first degree at Yale in 1927 and his PhD at Princeton. After a year as a geologist in Northern Rhodesia, a year at Rutgers and a year at the Carnegie Geophysical Laboratory in Washington he returned to Princeton in 1934 where he was to remain (apart from war service) for the rest of his life. He was promoted professor in 1948, elected to the Blair Professorship of Geology in 1964 and was department chairman from 1950 to 1966. He marked his return to Princeton in 1934 by marrying Annette Burns; they had two sons, one of whom is a physicist and the other an exploration geophysicist.

Like Holmes and Vening Meinesz, Hess was both geologist and geophysicist, and a man of ideas. Much of his early work was concerned with the petrology of the mafic and ultramafic magmas. He was especially interested in the problems of primary peridotite magmas and serpentization. He made important contributions to the understanding of the pyroxenes and their role in the crystallization of basaltic magmas. One of his major works is a 230-page Geological Society of America memoir on the Stillwater igneous complex of Montana completed in 1960. Several of his ideas in petrology were controversial and served to stimulate a great deal of research.

Hess was one of the first to recognize the rich rewards to be gained from exploring the oceans. His first paper (1932) was on the interpretation of gravity anomalies and sounding profiles in the West Indies. Although his general interest in the sea floor ranged from submerged valleys to the origin of mid-ocean ridges, his special topic was the structure of island arcs. After the Second World War he led the Princeton Caribbean Research project, which produced a series of papers on the interpretation of geophysical data from the Caribbean, on the geological mapping of the Caribbean islands and on the petrology and mineralogy of the Caribbean rocks, all with the goal of understanding the structure and evolution of this island arc. The project is still in progress.

Hess's name probably recurs most in connexion with sea floor spreading. In the 1950s he became interested in the mid-ocean ridges and presented an enlightened paper at Bullard's Royal Society discussion on the floor of the Atlantic. In England, there was a revival of interest in continental drift due to the results of palaeomagnetism studies. Hess, one of the few to be impressed, was quick to set up a palaeomagnetic laboratory at Princeton. Simultaneously, work at sea was showing that the sediments on the deep ocean floor are undisturbed. This was a powerful weapon for the anti-drifters who argued that drift would cause chaos in the sea floor. In the early 1960s, Hess proposed that new sea floor is being generated along the axes of the oceanic ridges and that the whole of the sea floor on either side is being carried along on mantle convection currents. He visualized the oceanic crust to be rigidly bolted to the uppermost mantle and the whole to be moving together (continents, ocean and sediments), the sediments thus remaining undisturbed.

This idea was strangely unpopular, especially in his own country. It gained support at the Department of Geophysics in Cambridge when Vine and Matthews argued that if the new crust were being added along the axis and spreading outwards it should record the reversal history of the Earth's magnetic field rather like a tape recorder. A careful interpretation of the total intensity magnetic field records suggested this was so. The next step was to apply the reversal time scale from the K–Ar dating of rocks on land and, knowing the distances of the anomalies from the axes of the ridges, it was possible to calculate the velocities. The ocean floors were found to be moving at speeds varying from 1 to 5 cm yr^{-1}. Happily, Harry Hess lived to see a most remarkable change in the attitude of earth scientists during the past two or three years. At the last two meetings of the American Geophysical Union dozens of papers on sea floor spreading were read. The predictions of the theory have recently been superbly verified by the results of the JOIDES deep sea drilling project.

Sea floor spreading paved the way for the highly successful plate theory. In this, the Earth's surface is considered to be covered by about ten rigid plates, which are continuously created at the oceanic rifts and devoured into the mantle near the island arcs and deep sea trenches. The idea has been developed by a few young geophysicists, most of whom have worked at Princeton or have in some way been associated with Hess and his colleagues.

During the Second World War Hess served in the US Navy in both the Atlantic and Pacific. His prowess in hunting U boats is legendary, and he was equally successful in hunting seamounts and guyots. He never missed an opportunity of running the echo sounder and his course changes served the dual purpose of confusing the enemy and providing new data for oceanic charts. His war service resulted in a classic paper in the *American Journal of Science* on drowned ancient islands of the Pacific Basin. In 1961 he was promoted to the rank of Rear Admiral in the US Naval Reserve.

After the war, he served on more than two dozen national committees, several of which he chaired. These included the American Miscellaneous Society Committee concerned with the ill fated Mohole project and several NASA committees including those concerned with manned space flight missions and lunar and planetary missions. He also served many learned societies, being President of the Geological Society of America, the Mineralogical Society of America and two sections of the American Geophysical Union, to mention but a few.

Correspondence

"Anomalous" Water

SIR,—Dr Donahoe's unduly alarmist and misleading letter concerning anomalous water (*Nature*, **224**, 198; 1969) has come to our attention. As one of the groups currently trying to sort out the chaos surrounding this phenomenon, we feel a reply is called for, especially considering the alarming newspaper reports to which the letter has given rise.

Contrary to the data which Dr Donahoe quotes as fact, remarkably little is still known about the precise properties of the substance, and it is still not certain that it even exists. Lippincott's polymeric structure, together with his binding energy figures, are still speculative and, in fact, contradictory stability figures have recently come from an independent theoretical investigation, suggesting the energy difference is not nearly so great.

One of the main reasons for there still being no coherent, self-consistent picture of anomalous water is the extreme difficulty of making it in quantities other than microlitres —and there is some suspicion that larger quantities are unstable. In the laboratory—where extreme care is taken —there is no evidence of its ability to grow at the expense of the normal phase (with which it is partially miscible) and in the absence of a quartz-like surface and without passing through the vapour phase; indeed, there is evidence of its gradual breakdown, especially upon heating and upon even small amounts of mechanical shearing. Although definite figures are impossible to give because of the quantities available, we are sure that not a single worker in the field shares Dr Donahoe's science fiction worries.

There is still no adequate explanation of the phenomenon, and no coherent picture of its properties. One of the greatest difficulties in even accepting the existence of a more stable phase is its apparent absence in nature. Indeed, this is the most persuasive evidence of its inability to grow at ordinary water's expense, for it has stood the test of billions of years. The classic conditions for its formation—a quartz surface and greater than 95 per cent humidity—are very widespread in nature, yet no anomalous water has been detected. If it can grow at the expense of ordinary water, we should already be a completely dead planet.

Yet we are not, and totally unlikely to become so from this source. By all means draw the attention of scientists to the dangers of their work, but make sure it is a real danger before alarming everybody else.

Yours faithfully,

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SIR,—Donahoe's recent letter (*Nature*, **224**, 198; 1969) prompts us to draw attention to the present uncertainty concerning the nature and properties of "anomalous" water. First, it must be emphasized that, whatever the correct interpretation may be, several of Derjaguin's experimental observations have been confirmed by recent work in several laboratories including our own. Anomalous properties are readily observed when saturated water vapour is allowed to condense in silica (or 'Pyrex') capillaries with diameters less than 50 micrometres, and the bulk of the ordinary water removed by lowering the vapour pressure by about five per cent. This is true even when the experiment is done in the presence of air and atmospheric pressure.

Because of the difficulty of making precise measurements on such small samples, some of the physical properties of "anomalous" water are still subject to some uncertainty. In particular, those listed by Donahoe cannot all be accepted without question. "Anomalous" water does not have negligible vapour pressure for it can be distilled; and although a density of 1·4 g/cm^3 has been reported, this value has been challenged recently by Mansfield[1]. Nor is there yet any conclusive evidence that "anomalous" water is more stable than ordinary water. The decreased vapour pressure of mixtures of "anomalous" and ordinary water is certainly no evidence for the greater stability of the anomalous species.

The mechanism by which "anomalous" water is formed is still not understood. The available evidence suggests that it forms only at the silica surface at the onset of condensation; subsequent condensation forms ordinary water which dilutes the anomalous species. There seems to be no evidence at all that, in solution in ordinary water, further "anomalous" water is formed spontaneously.

In view of the comparative ease with which "anomalous" water can be produced in the laboratory, it seems highly probable that it is also formed under terrestrial conditions, where suitable media and appropriate humidity fluctuations occur. Indeed, some of the earliest suspicions of the existence of an anomalous form of water are to be found in work done thirty-five years ago on natural materials[2]. Ordinary and "anomalous" water must then surely have coexisted on Earth throughout geological time, without the kind of catastrophe envisaged by Donahoe. While, therefore, we respect Donahoe's concern that proper vigilance should be maintained in any research involving the preparation of new materials, we consider that none of the existing evidence warrants the pessimistic conclusions he reaches.

Robert Burns's affections were guaranteed to remain constant "till all the seas run dry". While he may not have envisaged the possibility that the oceans might instead become anomalous, we feel that his shade may derive some consolation from the fact that they have not already done so.

Yours faithfully,

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J. M. HAYNES
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Department of Physical Chemistry,
University of Bristol.

1 Mansfield, W. W., *Abst. IUPAC Conf.* (Sydney, Australia, 1969).
2 Wilson, B. H., *J. Soc. Chem. Ind.*, **53**, 397 (T) (1934).

Teaching and Scientific Research

SIR,—Professor M. C. R. Symons, in his excellent article "Teaching and Scientific Research" (*Nature*, **223**, 353; 1969), reports his experience with the scientific literature as follows: "The routine coverage of current literature is a task of very considerable magnitude, which gets progressively worse. Unfortunately, . . . one can spend hours trying to understand one paper. There may be hundreds of papers each month that need to be read carefully, and so this is clearly a dominant aspect of our work".

Professor Symons's experience accords with my own and, I believe, with that of most other scientists. Consequently, I suggest that we scientists study our method of reporting research and thereby try to find a way of designing the research report for rapid reading and quick comprehension.

From my own brief studies, I predict that on the average we can at least double the rate at which research reports can be read understandingly and that we can simultaneously decrease, by at least one-half, the accompanying reader fatigue.

Should we succeed in designing reports for extremely rapid comprehension and ease of reading, the benefits to science would be incalculable.

Yours faithfully,

F. BRUCE SANFORD

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University News

Dr L. Rotherham, Central Electricity Generating Board, has been appointed Vice-Chancellor of **Bath University of Technology** from September 1, 1969.

Professor D. K. Britton, University of Nottingham, has been appointed to the chair of agricultural economics tenable at **Wye College,** University of London.

Professor C. P. Whittingham, Imperial College of Science and Technology, **University of London,** has been elected Dean of the Royal College of Science.

Mr B. Shackel, EMI Electronics Ltd, has been appointed a professor of ergonomics in the department of ergonomics and cybernetics, **Loughborough University of Technology.**