SIR EDWARD BULLARD

1907 – 1980

BY E. C. BULLARD

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with added remarks by A. E. Maxwell, W. H. Munk, F. Gilbert, and C. S. Cox

Sir Edward Bullard, ca. 1984.

Teddy Bullard (Sir Edward), an English geophysicist, worked on a wide range of geophysical problems. Four major geophysical tools are: seismic exploration, measurements of the magnetic field, of the field of gravity, and of heat flow. In the 1930s and 1940s it was widely thought that it would be too difficult to adapt these methods for use at sea. Forty years later it is found that the most successful use of these methods has been at sea, not on land. Teddy played a significant role in adapting each of these methods for use at sea. In his mature years his efforts were mainly directed toward understanding the origin of the earth's magnetic field, and in particular the reality and mechanism of continental drift. These were in 1950 two great unsolved problems of geophysics. He, his students, and colleagues were instrumental in making the immense progress in these fields that occurred in the following quarter century. He was at the Scripps Institution of Oceanography for several summers before finally joining the institution after his retirement from Cambridge University. He had a profound impact on the science of geophysics and on the Scripps Institution itself. To understand his influence it is worth while to outline his career. A succinct version appears in the dedication pages of the

"Conference on Nonlinear Dynamics" (AIP 1978). This was ostensibly written by Walter Munk (but was actually written by Teddy himself*).

"Edward Crisp Bullard ...was born in Norwich, England, in 1907.... His scientific work started in 1929 when he became a graduate student in Ernest Rutherford's Cavendish Laboratory at Cambridge. Here, under the direction of P. M. S. Blackett, Bullard studied the scattering of slow electrons in gases. In this enterprise he was soon joined by another graduate student, H. S. W. Massey, [later] Secretary of the Royal Society. By 1931, they had found unmistakable evidence of diffraction patterns similar to the rings seen around a street lamp in fog. The results were explained very naturally by wave mechanics, and were clearly inconsistent with classical views.

"It might have been expected that this striking early success would have led to a career in the rapidly expanding field of atomic and nuclear physics. This was not to be; 1931 was the worst year of the economic depression. Bullard could see his Ph.D. approaching, but no prospect of a job. Rutherford's advice was to 'take any job you can get'.

"The job that came along was to help teach surveying to Colonial Service Probationers. The course was run by the charming, elegant, elderly reader in geodesy, Colonel Sir Gerald Lenox-Conyngham, who had taken the post after a long military career in India. He and Teddy Bullard got along very well. No more was heard of teaching surveyors. Bullard embarked on a very profitable eight years in which he learned the elements of earth science and carried out a remarkably diverse series of projects. The success of these was in large part because of the steady support of Lenox-Conyngham, who was not at all disturbed when told that the police were looking for the perpetrators of an explosion that had left a hole in a Leicestershire road.

"The success in carrying out substantial projects in instrument development and in field work, with almost no money, was made possible by Bullard's discovery that Leslie Flavill, a boy of 16 whom Lenox-Conyngham had hired as a clerk, was a most able instrument designer and a mechanic of genius. Bullard maintained Harold Jeffreys, Johnny von Neumann, and Leslie Flavill were the three most intelligent people he ever met.

"At first the work was in gravity measurement, largely because the department possessed some pendulums. This took Bullard to East Africa in 1933-1934. A long paper in the *Philosophical Transactions* that contains the observations, and what now seems an erroneous interpretation of the origin of the rift valleys, brought him widespread recognition [and membership in the Royal Society].

"Bullard's next interest was in explosion seismology. He, Tom Gaskell, W. B. Harland, and Colin Kerr Grant set out to map the Paleozoic floor beneath eastern England. In this work he acquired a good understanding of the techniques and theory of refraction shooting. This was essential to the next phase of his activities, the study of the seafloor.

"The move out to sea was a consequence of a meeting with Dick Field at the IUGG meeting in Edinburgh in 1936. Field was a maverick geologist from Princeton, who can now be seen as the most influential earth scientist of his generation. It was his enthusiasm and tremendous powers of persuasion that launched marine geology on its spectacular development. He invited Bullard to the U.S. in 1937 and exhorted him to take up the study of the seafloor. Like Harry Hess and Maurice Ewing before him, Bullard was easily converted. He went to sea with Ewing, and when he got home, started similar work on his own. He, Tom Gaskell, and Ben Browne found that the continental shelf south of Ireland was similar to the one examined by Ewing on the other side of the Atlantic. The discovery of the vast basins of sediments beneath the continental shelves has had consequences extending far beyond their scientific interest; they are now a major source of politically secure oil.

"War was approaching, but there was still time for one more investigation before it came. Bullard took up the measurement of heat flow on land, which had been almost abandoned for 50 years. He spent the winter of 1938-1939 in South Africa working in collaboration with L. J. Krige. They made measurements in boreholes, which set a standard in what, despite its apparent simplicity, is a difficult field. War interrupted further development and postponed for 10 years the initiation of similar work at sea.

"Back in England in November 1939, Bullard became an 'Experimental Officer' in the Naval Scientific Service. After a sharp struggle with the naval scientific establishment in Portsmouth, he found himself in charge of the development of methods of protecting ships from magnetic mines. A few months later he became head of the group developing methods for sweeping all kinds of mines. After 18 months spent on these matters, the losses from mines had fallen to less than 10% of those caused by submarines, and Bullard started to look for other ways of helping the Navy. He moved to London and joined Patrick Blackett, who had been his thesis advisor 10 years before in Cambridge. Blackett was starting to apply the methods of what is now known as operations analysis to naval operations. Bullard worked on naval problems for a year or more, and then became diverted to the study of the intelligence that R/V *Jones* was uncovering concerning the development in Germany of rockets and flying bombs; and later, to a study of the most economical ways of attacking their firing sites in northern France. In the middle of this work he also acquired the job of protecting the Commander-in-Chief of Fighter Command from politicians. This assignment provided him with training in the realities of politics that proved valuable in many subsequent enterprises.

"When the war with Germany ended in May 1945, Teddy Bullard returned to Cambridge. He found a shambles; the door of the laboratory had not been opened for years, most of the equipment had been removed, and what remained was covered with rust. There was no money and no help; his first task was to scrub the floor. During the next four months young men started to return as graduate students, and it became possible to take up geophysics where it had been left six years before. The first priority was to carry out seismic refraction work in the deep sea; the methods for this were developed by Maurice Hill. Other projects were observations at stations in Europe of the seismic waves produced by the explosion of a very large ammunition store in a cave under the Baltic island of Heligoland and a continuation of pre-war work on heat flow. In 1947, when he was 80 years of age, Lenox-Conyngham retired, and Bullard became head of the department at Madingley Rise.

"Continual difficulties over the financing of research and over the almost total lack of shiptime exasperated him to a point where he decided to leave Cambridge. The final straw was the refusal of the university to let him have a typist for five half-days a week instead of for two half-days. He had just been told of this decision when an emissary from the University of Toronto arrived and asked him to become head of their Physics Department. He accepted, but it proved an unfortunate decision. He and his family did not settle happily in Canada, and he became homesick for Cambridge. A bright interlude was the summer of

1949 in La Jolla, which he remembered as the most prolific period of his scientific career. He and Art Maxwell, then a graduate student, built the long-postponed equipment for measuring heat flow at sea. He shared an office [Walter Munk's], and [Walter] remembers the enthusiasm with which Bullard and Art spent long days in the workshop building this quite complicated machine with their own hands. Watertight equipment for use on the floor of the deep sea was at that time a novelty, and, so far as I know, that heat-flow equipment was the first to use the now indispensable O-ring for its watertight joints. During this summer Bullard also wrote a widely known paper on the westward drift of Earth's magnetic field.

"Just before he came to La Jolla in 1949, Bullard was offered, and had accepted, the Directorship of the National Physical Laboratory (NPL) in England. He took up the job at the beginning of 1950. He was, I think, a well-liked and effective director, but his heart was not really in it. Like many of his contemporaries, he suffered from an indecision of purpose. If he was in an academic job where he could get on with his own work, he felt that he should be directing some great and prestigious enterprise; if he let himself accept such a position, he at once started to lament the loss of freedom and opportunity to do his own work.

"In fact, Teddy Bullard did succeed in doing a substantial amount of his own work while at the NPL. A new heat-flow apparatus was built, this time with the help of an outstanding design office and workshop, and it was used in the Atlantic. He also wrote a long and elaborate paper on dynamo theory. It now seems likely that the particular dynamo proposed in the paper will not work; nevertheless, the paper had an important influence on the general acceptance of a dynamo in Earth's core as the cause of its magnetic field. ...

"In 1956, Bullard got what he wanted, and returned to Cambridge, to almost the bottom position on the academic totem pole. This time he managed to resolve his indecision about what kind of life he wanted. His primary commitment was to his own work and to helping a large number of very able graduate students. This was the decade when the ideas of seafloor spreading and continental movement were developing rapidly. The Madingley Rise assemblage played a large part in this development. Bullard had long been interested in these matters, but it was only after his return to Cambridge that he was fully convinced of the reality of the movements. The critical year was 1964 when Harry Hess, Tuzo Wilson, Drummond Matthews, and Fred Vine were all at Cambridge. After this, none of the group had many doubts, and soon embarked on a campaign to convert unbelievers, particularly in the United States. An odd consequence of their success is that Bullard, almost for the first time, found himself a defender of orthodoxy.

"While at the NPL and in Cambridge, he spent much time on matters far from his usual scientific interests. Among other things, he became a director of the family brewery Bullard's Ale (now, alas, taken over by a faceless conglomerate), a director of the British subsidiary of IBM, a British representative at the 1958 talks with the Russians on the possibility of a test-ban, and joint chairman of the Anglo-American Ballistic Missile Committee.

"In 1964, the University of Cambridge made him Professor of Geophysics. While at Cambridge, he paid regular visits for one or two months each year to Scripps.

"In 1974, Teddy retired and came to live in La Jolla and to work at Scripps. He [gave] widely appreciated lectures, directed nominally to first-year nonscientists, but into which many scientists infiltrated. He wrote on his favorite topics of plate tectonics and the origin of Earth's magnetic field, though his main interest had shifted to the murky politico-scientific-industrial questions of nuclear waste disposal.

"While he had received many honors and awards, I think what he appreciated most was the friendship of his former students and fellow workers in earth sciences. The Director of Scripps recently described him as 'a happy geophysicist'; it is, I think an apt comment."

Teddy Bullard described his two months working with Art Maxwell on the seafloor heat flow apparatus in the summer of 1949 as the 'most prolific of his career'. Art described this experience as follows (personal communication, 2002):

> "Teddy and I essentially arrived at Scripps on the same day and Walter Munk assigned me to him as a graduate student. Neither of us knew much about Scripps or its *modus operandi*. This led to many interesting and sometimes challenging situations.
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> "We spent much time together over the summer before Bullard returned to Great Britain. During this time we put together an instrument in an attempt to measure the heat flow through the ocean floor."

"Although Hans Petterson had attempted earlier heat flow measurements at sea aboard R/V *Albatross*, no previous measurements had been successful. The instrument Teddy (with my help) designed was based on simple straight-forward principles. Teddy used O-rings to seal the end plates to the steel cylinder, and I believe this was the first use of them at Scripps and maybe at any United States oceanographic laboratory. The sensing elements were two thermocouples placed about two meters apart in a probe about three meters long. The recording apparatus was a galvanometer with a light source and a mirror to give a folded path for the light beam, which was recorded on film. As you might guess we had all sorts of difficulties with this arrangement. The mirror fogged and vibrated. The probe bent, the film fogged by pre-exposure, etc. Eventually, it worked well enough for trial at the ZLAC Rowing Club in

Sir Edward Crisp Bullard in Samoa, Nova Expedition, 1967.

Mission Bay, San Diego. We were able to get some records there, but no usable data. It only proved the concept was workable.

"Taking the instrument to sea exposed new problems. It was difficult, if not impossible, to determine when the instrument hit bottom. This was essential because the instrument needed to remain in the bottom undisturbed for about fifteen minutes to obtain a sufficient record to extrapolate recorded temperatures to their equilibrium values. To solve this problem, John Isaacs and I developed a ball breaker, which imploded a hollow glass sphere on impact with the bottom. The noise of the implosion could be received on a hydrophone aboard ship. With the ball breaker placed in line with the heat flow equipment, we got a good indication when the instrument hit bottom.

"Although most of the bugs were worked out during Teddy's stay at Scripps, no successful deep sea measurements were made. Teddy returned to the National Physical Laboratory in England and began work on an electronic amplifier and recorder, which was scoffed at by oceanographers at the time. You may recall Athelstan Spilhaus made the comment about that time 'that any successful oceanographic instrument should contain less than one electron tube.' A similar effort using electronics was undertaken at Scripps with myself, Jim Snodgrass, and others. Roger Revelle was the principal scientist behind these efforts.

"Work on heat flow at Scripps and NPL proceeded in parallel and both groups made successful heat flow measurements at about the same time: Bullard's in the Atlantic and Scripps's in the Pacific.

"Teddy was a most inspiring person to work with. He would always explain simple things using physics principles. For example, he would explain the making of a milk shake by the transfer of kinetic energy into thermal energy. Once, while driving between La Jolla and Point Loma he breezed through a stop sign with the comment 'I wonder what those signs are for, as there are no cars.' This was followed by the statement in which he said none of his students at Cambridge would ride with him and praising me for my bravery.

"That summer was as busy and as much fun as I can remember. Teddy was such a delight to be with. Even though he had a short stay at Scripps, Teddy mentioned that he was asked if he would be interested in being Director of Scripps. He said that as much as he liked La Jolla and Scripps he would rather return to England. As you know he became Director of NPL and was eventually knighted by the Queen."

The results of the heat flow work were an immense surprise. Why was the heat flow through the seafloor as great as that through the continents? It was well known that radioactivity in continental crust was the primary source of heat flow on land. The evolution of Bullard's thinking can be followed through his published notes. The first is a commentary following Revelle and Maxwell's letter describing their first measurements in the eastern North Pacific. He wrote (*Nature*, 1952, 170, p.200): "The preceding communication by Revelle and Maxwell gives a result that is completely unexpected, and demonstrates again how little we know about submarine geology. ... A possible interpretation of the results therefore appears to be that when Earth solidified most of the radioactivity was concentrated in the upper 150 km of the mantle; under the oceans this distribution still exists. ... Other explanations can be suggested. It might, for example be supposed that at some not too remote time a convection current rose under the Pacific and brought hot material near the surface, ..." Bullard considered the second suggestion entirely speculative even though the first was not in agreement with the little data then available on radioactivity of ocean crustal rocks.

After many more measurements in both Pacific and Atlantic, these three pioneers of oceanic heat flow measurements wrote an extended interpretation that must have been mainly Bullard's ideas (Bullard, Maxwell, and Revelle, 1956, *Adv. Geophys.* 3, 153-181). It discarded the idea of radioactivity concentrated in the upper mantle under the oceans and provided a prescient discussion of heat transfer by sluggish convection of mantle rocks. Bullard not only described the quasi-adiabatic temperature gradient to be expected in the mantle, but provided a quantitative estimate surprisingly close to present estimates for the upper mantle.

The idea of continental drift as proposed by Alfred Wegener had had a troubled history, partly because little was known about seafloor geology, and Wegener's suggestion of the continents plowing through the ocean floor seemed absurd. A. L. du Toit, a South African geologist supported the idea of continental fission strongly in the 1920s and 1930s by demonstrating the similarity of geological structures in corresponding parts of South America and Africa. As late as 1955 Teddy described himself as a 'fence sitter' on the controversy but in the early 1960s his group reexamined the fit of the edge of the continental shelves of the Americas plus Greenland to the continental shelf edges of Africa to Europe. This fit, plus the increased understanding of paleomagnetic results on all continents, convinced him of the reality of great horizontal divergence of blocks of the earth's crust. An important contribution was the demonstration of enormous lateral displacements on the seafloor demonstrated from the displacement of magnetic lineations shown on the seafloor magnetic maps made by Ronald G. Mason and Arthur D. Raff in the mid-1950s. Just how the high heat flow through the ocean floor fitted in had to await the discovery of seafloor spreading. This was proposed elsewhere by Harry Hess and Robert Dietz, but field evidence from the Indian Ocean for its reality was definitively developed at Madingley Rise in 1962-1964 after Bullard returned to Cambridge, (1956, professor; 1960 head of Department of Geodesy and Geophysics). Here he built a department whose members found the evidence for the new theory which could convince geologists everywhere.

Edward L. Winterer and Sir Edward Crisp Bullard on Vanua Mblavu, Nova Expedition, 1967.

A very strong interchange between Scripps and his department developed. Walter Munk was the first of many visitors from Scripps to Cambridge. Teddy had pioneered the use of digital computers to explore the nonlinear equations appropriate for the magneto-hydrodynamics of Earth's core. When Teddy was Director of the National Physical Laboratory he had an early electronic digital computer (ACE) built. He used it in some of his dynamo calculations in 1950-1952. A scientist at NPL, J. H. Wilkinson, used ACE to develop some of the best algorithms in numerical linear algebra; they were published in his book, *The Algebraic Eigenvalue Problem* (Oxford University Press, 1965). Later Walter and Teddy became interested in using electronic digital computers to analyze geophysical data. Their work led to the then famous and much used set of computer programs called BOMM (Bullard, Oglebay, Munk, and Miller).

Among Scripps scientists with sustained visits to Teddy's group at Cambridge (Department of Geodesy and Geophysics at Madingley Rise) were George Backus, Bob Fisher, Freeman Gilbert, Bill Menard, and Vic Vacquier.

Herbert Eric Huppert did his Ph.D. with John Miles of Scripps/Institute of Geophysics and Planetary Physics and became a professor at Cambridge. Many other Scripps scientists have visited Cambridge on briefer trips for seminars, working visits, etc. From Madingley Rise Scripps hired John Mudie, Bob Parker, and John Sclater. Dan McKenzie and Bob Parker wrote their famous 1967 *Nature* paper about plate tectonics while they were postdocs at Scripps.

In recent years several Cambridge scientists have visited Scripps either as Green Scholars or as visitors to Scripps's departments. The exchange continues with every sign of benefit to both institutions.

Footnotes

*"Conference on Nonlinear Dynamics" held in honor of Sir Edward Bullard, American Institute of Physics, 1978. About the dedicatory introduction to the proceedings Walter Munk writes:

> "'A tribute to Sir Edward Bullard' was in fact written word-for-word by Sir Edward Bullard. I had just completed the presentation of the Maurice Ewing Medal to Sir Edward (Transaction AGU 59:794-795. 1978) when I was asked to write the dedication to Sir Edward Bullard of the AIP Conference Proceedings. I was in no mood to do so, and asked Teddy whether he would enjoy writing about himself what HE thought ought to be said; it was to remain forever a secret. Teddy wrote a delightful account of his career, full of glimpses such as returning from the war and finding his Cambridge laboratory in shambles: 'his first task was to scrub the floor'. The manuscript was returned to me by the editors with the above quote and many others deleted. Upon protesting, I was told that 'Sir Edward would be gravely offended'. I persisted. Teddy was so delighted with the dedication that he spilled the beans."