Jeffreys and the Earth

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The work of Harold Jeffreys related to geophysical processes is described and its interactions with other research of this century on the Earth and planets explored. The extent of his influence on the style of the subject by his insistence on mathematical treatment, order of magnitude arguments, and probability assessments is evaluated. For forty years, he had no peer in his wide application of wave theory to seismological problems. Jeffreys's greatest lasting success was his construction of global travel-time tables, with uncertainty estimates, for seismic waves and their inversion to a seismic velocity structure in a radially symmetric Earth model. He also contributed significantly to a wide variety of fundamental geodynamical problems, especially the Earth's rotation, its figure and gravity field, and thermal history. His development of a theory of inference applicable to geophysics was profound; but his assessment of posterior inference in relation to global tectonic evolution gave little weight to the importance of the geomagnetic observations on the ocean floor. His conclusions on the long-term mechanics of the Earth were contrary to the now widely held theory of plate tectonics. Yet his work on convection and energy dissipation remains central to the study of Earth processes.

JEFFREYS'S CAREER

It is appropriate at the outset to give a brief account of Jeffreys's life. Detailed memoirs have now been published by the Royal Society of London [*Cook*, 1990, including a microfiche with a list of publications and other material], the Seismological Society of America [*Bolt*, 1989], and elsewhere.

Harold Jeffreys was born 100 years ago on 22 April 1891 at Fatfield, England. His parents were school teachers. He obtained a B.Sc. degree in 1910 with distinction in mathematics at Armstrong College, at that time a College of the University of Durham; the site is now the University of Newcastle-upon-Tyne. He went on to study mathematics at Cambridge University and was elected a fellow at St. John's College in 1914, the first year of World War I. He remained a Fellow until his death in 1989 — some 75 years. A 1929 photograph including Jeffreys is reproduced in Figure 1.

Jeffreys published his first investigations in geophysics as early as 1915, inspired by the major geophysical endeavors — not so well known today — of G. H. Darwin [see *Darwin*, 1907-16]. From 1917 to 1921 he worked at the Meteorological Office in London, tackling some hydrodynamical problems that came from the war front in France. He returned to Cambridge in 1922, where he stayed for his life's work and where in 1946 he was elected Plumian Professor of Astronomy and Experimental Philosophy. He retired from his chair in 1958 but vigorously continued his research for many years afterwards.

With the pioneer seismologist H. H. Turner (Director of the *International Seismological Summary*) and Robert Stoneley, he promoted geophysics at the Royal Astronomical Society, recommending the publication of a *Geophysical Supplement* — later the *Geophysical Journal* and now the *Geophysical Journal International*. His papers dominated its numbers for more than thirty years. He was President of IASPEI (1957-1960) and attended the first assembly of the IUGG in 1922 and every meeting of the IUGG from 1933 to 1967.

In 1940 Jeffreys married Bertha Swirles, an atomic physicist. In 1929, she had received a Cambridge Ph.D. for a thesis on "Some Applications of the Theory of Perturbations in Quantum Mechanics." She had been a lecturer in Applied Mathematics at Manchester University when she returned in 1938 to Girton College as Fellow and Lecturer. She retired from lecturing in 1969 and has now been a Fellow for over fifty years. Papers concerning her work will be deposited with those of Sir Harold in St. John's College Library. Together, they wrote the influential and still widely used treatise *Methods of Mathematical Physics* [*Jeffreys and Jeffreys*, 1972]. For over three decades, visiting mathematicians, astronomers, statisticians, and geophysicists were welcomed by Sir Harold and Lady Jeffreys at their home at 160 Huntingdon Road, Cambridge, recently described by a Russian visitor as "an Institute for the History of Geophysics." Sir Harold died in Cambridge just short of the age of 98.

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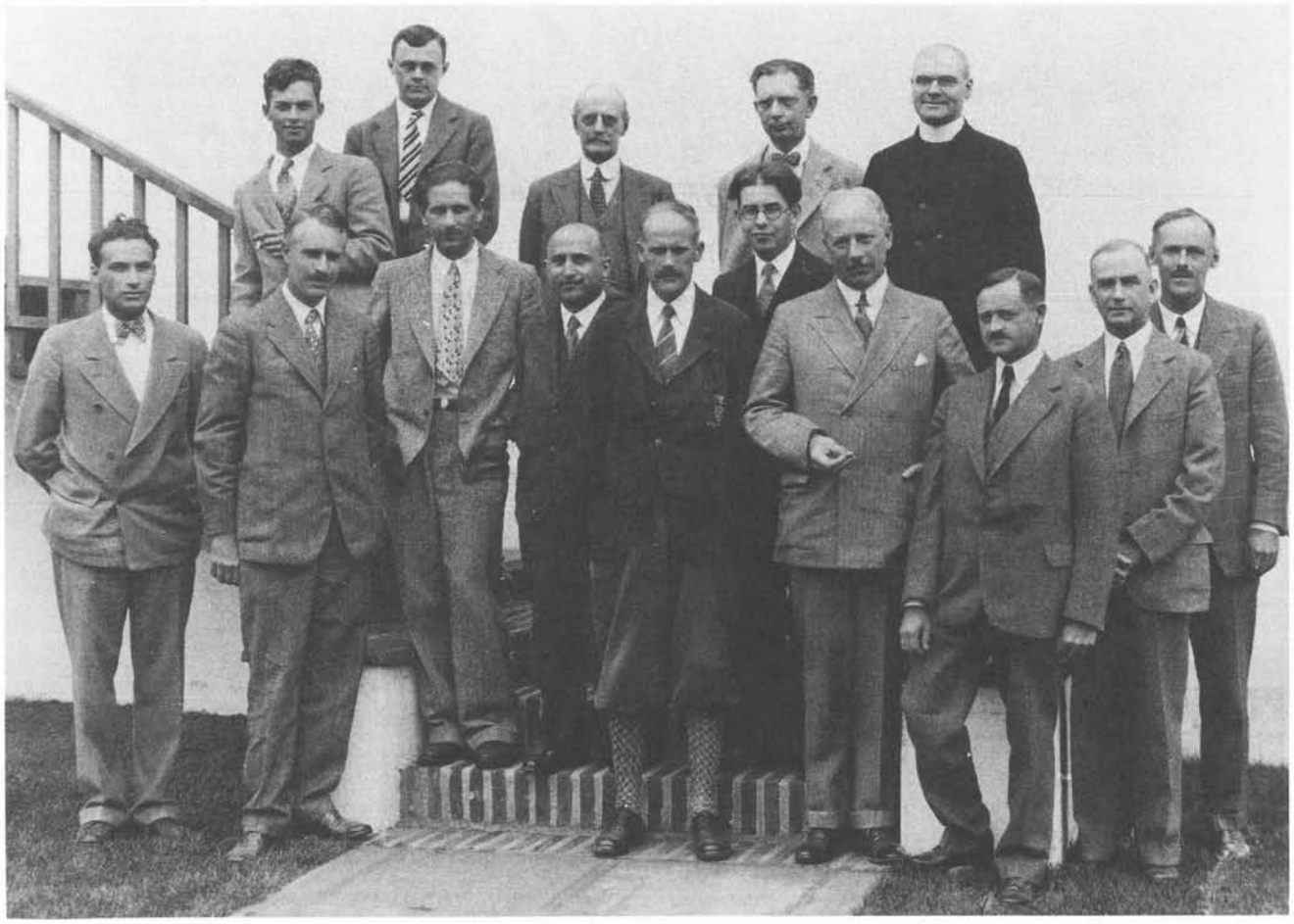

Fig. 1. Conference on the future of seismological studies at CalTech at Pasadena in 1929. Front row, left to right: assistant, Leason Adams, Hugo Benioff, Beno Gutenberg, Harold Jeffreys, Charles Richter, Arthur Day, Harry Wood, Ralph Arnold, John P. Buwalda. Back row: assistant, Perry Byerly, Harry Fielding Reid, John Anderson, Father J. B. Macelwane.

One enduring anecdote of Jeffreys is told in different forms: *Bates et al.* [1982, p. 38, footnote 27] have written "Within the fields of seismology and gravitation, it is difficult to find a research topic that Sir Harold has not already written about. However, when geophysicists from British Petroleum Ltd. approached him with the hope of learning some new survey techniques, they first explained at length the difficulties in finding new oil-bearing structures in Iran. He replied, 'I'm glad it's your problem, not mine!'"

THE TREATISE *THE EARTH*

In recent times there has been wide discussion that contemporary progress will soon bring theoretical physics to an end with a final discovery of "A Theory of Everything" (acronym A TOE). This idea of finality has been around for a while. At the end of the last century, Lord Kelvin believed that all that remained to be done in physics was to measure the physical constants more accurately. In the 1920's, after Einstein's relativity theory, Born suggested that physics would be over in six months. Afterwards, physics got busy again.

With the publication of the first edition of *The Earth* in 1924, Jeffreys brought to geophysics a large part of Kelvin's optimism concerning the power of classical dynamics. Yet Jeffreys never did claim, except perhaps in his formulation of a probability methodology, that he was describing "the geophysical TOE." Each succeeding edition of *The Earth* through the sixth (and last) in 1976, incorporated changes and iterations towards more complete and realistic models and processes. This seminal work demonstrated how comprehensive and quantitative mechanical arguments can define the physical properties of the whole Earth. Fundamental theories were laid down concerning the Earth's genesis, its internal history, its structure and composition, and its external and internal dynamical interactions. Rather than building upon the

preconceptions and assumptions from antiquity, *The Earth* altered geophysics permanently by setting up a consistent critical methodology in which physical theory and observations were explicitly interdependent. An exception in Jeffreys's multifaceted canvas which may have affected his views on plate tectonics (see the later section on geodynamics) was a relative neglect of electromagnetic properties and processes in the Earth.

In his general publications we can trace the substantial evolution in Jeffreys's conception of both Earth structure and geophysical processes. For example, he initially argued that the solar system had been formed by tidal disruption of the primitive sun by a passing star. By 1952 he found defects in all theories of the origin of the solar system [*Jeffreys*, 1952]. At an early stage he inclined towards Darwin's resonance theory for the origin of the moon; later he wrote "the Moon was certainly not formed by fission from the Earth" [*loc. cit.*, p. 290].

An illustration [*Bolt*, 1991] of how robust some of his early inferences were, however, comes from the growth of our knowledge of the terrestrial density distribution $\rho(r)$. In the first edition of *The Earth*, Jeffreys gave a critique of the primitive available knowledge on $\rho(r)$. The Earth model he preferred had been proposed by *Wiechert* [1897]. It consisted of two constant density shells and was based on abundances of rocky and iron meteorites. Jeffreys wrote (p. 198) that "The little knowledge that we have suggests that the interior is so dense as to be probably metallic while the outside is rocky. Such materials cannot mix freely and therefore we should expect a fairly strong boundary between them with a sudden discontinuity in density."

By 1929, the second edition reflected a radical change in the available observations. Jeffreys acknowledged the discovery by R. D. Oldham of a sharp decrease in P and S seismic velocities in the deep interior and the subsequent calculation for the depth of the mantle-core boundary of 2900 km by B. Gutenberg. Reliable distributions of velocity of both P and S waves in earthquakes had by then become available, allowing the compressional increase in density to be calculated using realistic elasticity assumptions. Jeffreys perceived that *Williamson and Adams* [1923] had provided "a method for recalculating the distribution of density." About two decades later, after a provisional solution in 1936, Jeffreys's student K. E. Bullen carried through this program [see *Bullen and Bolt*, 1985] using the much more representative V_P and V_S functions derived from the *Jeffreys-Bullen* [1940] *Seismological Tables* of travel times (the J-B tables). Succeeding editions of *The Earth* tabulated the density results of Bullen based on the Adams-Williamson differential equation.

With the great Chilean earthquake of 1960, terrestrial spectroscopy was born. While at Cambridge in 1961, I had reduced a long time series of ground motions from this earthquake written in the Grotta Gigante by a long pendulum devised by the late Professor A. Marussi. The Fourier analysis schemes available at the time provided significance tests only for the amplitudes of the largest eigen modes. *Jeffreys* [1964] noticed that uncertainties of the eigen frequencies themselves were not being estimated and published a note on the subject. He later [1977] remarked "What is often done is to compute for intermediate values [of the eigen frequency] by integration. This is pointless because the resulting values are simply interpolates and their errors are highly correlated .... I do not know of a single case where an uncertainty has been properly determined." Perhaps even more important, the use of the eigen frequency measurements to make inversions to properties of the Earth was criticized by *Jeffreys* [1967] on the grounds that no allowance had been made for anelasticity in the construction of Earth models. Work by *Randall* [1976] and others, "confirmed Jeffreys's objection and has shown that discrepancies between models based on free oscillation data and models based on travel-time data can be removed, and quantitative assessment of anelasticity made" [*Lapwood and Usami*, 1981, p. 17].

Despite Jeffreys's concerns regarding the measurements, the sixth edition of *The Earth* [1976] contained a description of the new spectral method of making density estimations. Jeffreys described (p. 288) the recording of the free oscillations of the Earth following the 1960 Chilean earthquake and noted their great usefulness in principle in revising interior elastic parameters, such as density and rigidity of the inner core. Such parameters "can be taken as an unknown and adjusted to fit the periods." Now, rather than proceeding by integration of the Adams-Williamson equations, with substitution of assumed bulk composition parameters, it was more promising to invert the problem using the terrestrial eigen frequencies [*Bullen*, 1975]. The critical strength was to allow strong *independent* inferences to be made on the materials in the deep interior.

As stated by Jeffreys as early as the second edition of *The Earth* [1929], "We may look at the matter from the other end. The mean pressures and the bulk moduli [inside the Earth] can .... be estimated, and hence so can the reduction in density if these pressures are taken off." In an extension of this idea, *Jeffreys* [1937] used this scaling principle to interpolate from the Earth's density distribution to those of other terrestrial planets and hence to infer their structure and composition [*Bullen*, 1975].

Fluid Dynamics and Nutations of the Earth and Moon

Jeffreys had an early interest in fluid dynamics, no doubt fortified by the environment at the Meteorological Office. His discussion of the eddy viscosity and convective processes in the atmosphere was influential and included the idea that cyclonic storms are essential to the general circulation. When the wind blows over the surface of a body of water, surface waves are formed. For well over a century, applied mathematicians have attempted to model the mechanism or mechanisms — with only partial success. An alternative to the unsatisfactory model of Helmholtz and

Kelvin was introduced by *Jeffreys* [1925]; it was based on "sheltering" on the leeward side of crests. Little progress was made subsequently until the mid-1950's. In other seminal work, beginning in 1926, *Jeffreys* [1926*b*] used stability arguments to establish realistic conditions for the onset of convection in a viscous fluid heated from below [*Jeffreys and Jeffreys*, 1971-77, *Collected Papers*, *4*, Section XII].

Jeffreys was deeply interested in the nutations of the Earth — both free and forced by the Moon. Oldham in 1906 and Gutenberg in 1914 had given seismological evidence that the major part of the core of the Earth was much less rigid than that of the mantle, in that no S waves were detected at relevant distances [see *Bolt*, 1982]. Jeffreys, using rough estimates of density and elastic parameters based on Wiechert's Earth model, calculated in 1915 that the resulting theoretical free period of the monthly nutation was too short when compared with observations. By 1926, after including the effective compression of density, he demonstrated that the seismological radius of the core obtained by Gutenberg was compatible with the observed free period if the core had negligible rigidity [*Jeffreys*, 1926*a*]. In celebration of this accomplishment, Jeffreys sent a postcard to Stoneley, his closest colleague and friend: "Die Wiechertsche und Oldham-Gutenberg Kerne Identisch sind" [see *Jeffreys*, 1976].

Jeffreys then went further. He concluded [1926*a*, p. 371] that "There seems to be no reason to deny that the Earth's metallic core is truly fluid." I take this demonstration of the core's fluidity as Jeffreys's most significant geophysical discovery, although other judgements can be strongly argued. The sequence of events which sustain this view is well described with detailed references by *Brush* [1980]. Brush writes: "In this context, 'discovery' simply means presenting a claim with arguments sufficient to convince the scientific community at the time; originality lies in the successful persuasion not in the conception of the claim." The majority view among geophysicists and geologists in the mid-20's was that the entire Earth was solid, a view apparently accepted by Jeffreys as late as 1925. About that time, Jeffreys had reworked some of the basic material. Brush argues that the credit for the discovery of the fluid core goes to Jeffreys because he was able to convince Gutenberg and others and refute certain arguments against the hypothesis. While Gutenberg appeared to accept Jeffreys's arguments of the non-rigidity of the core, he was reluctant to describe the core as liquid until as late as 1957 when one finds in print, "the core is liquid" [*Gutenberg*, 1957]. Much earlier, Gutenberg himself had considered a liquid core but rejected it. Although he had not observed S waves passing through it, his explanation was that they would be rapidly absorbed within the core by frictional attenuation.

Later, with Vicente, Jeffreys further investigated nutation of the Earth's axis [*Jeffreys and Vicente*, 1957]. This heavy computational work allowed for elastic properties of the mantle and fluid core and yielded fair agreement with the observed amplitude of the 19 yearly nutation. *Munk* and *McDonald* [1960] judged that "the subject [of rotation of the Earth] was reopened in the light of modern geophysical knowledge by Jeffreys. His contributions dominate the subject." He worked on the variation of latitude from at least 1916 almost to the end of his life.

Jeffreys's great interest in dissipative processes in the Earth is nowhere better illustrated than in his study of tidal friction in shallow seas [*Jeffreys*, 1920]. The work grew out of a demonstration by *Taylor* [1919] of the astronomical importance of energy loss by tidal action in the Irish Sea. Jeffreys considered two possible mechanisms for dissipation of the Earth's rotational energy: internal friction and torques due to tides raised by the Moon. He rejected the first and supported correctly the energy loss by frictional forces produced by currents in shallow seas around the world and along the coasts. *Knopoff* [1991] remarks "All of this work demanded extraordinary attention to the measurement of the Earth's gravity field, of the astronomical observations, of the rates of change of the length-of-day, the currents in the oceans, etc. Some of the results were done on mechanical calculating machines. The analysis could not have been done without Jeffreys's brilliance as an applied mathematician."

In a related field, Jeffreys did key work on the determination of the Earth's gravitational field [*e.g.*, *Jeffreys*, 1941, 1959]. He produced a plot of the deviation of the Earth's figure from hydrostatic shape, which showed gravity highs over the North Atlantic and the Pacific and gravity lows over the Caribbean and India [see *e.g.*, *Jeffreys*, *The Earth*, fourth edition, 1962, p. 187]. Although, as in other studies of his, these results were at first unfashionable and met with skepticism, they were eventually vindicated by observations of perturbations of artificial satellite orbits.

SEISMOLOGICAL TRAVEL-TIMES

One of Jeffreys's most lasting contributions to geophysics was his work in improving tables of travel times of seismic waves through the Earth. Such tables permit not only the precise location of remote earthquake sources, but allow mathematical inversions to estimate the seismic velocity structure. In the mid-1930's, Jeffreys set about correcting the generally used tables and was soon joined by Bullen as a Ph.D. student. By 1939 this research program, which produced the Jeffreys-Bullen tables and a corresponding seismic velocity model for the Earth, was complete. Contemporaneously, Gutenberg, with help from Richter, derived at the California Institute of Technology similar tables using rather different methods. Jeffreys excelled in statistical reduction of published times measured by others; Gutenberg, in personally correlating wave recordings. Jeffreys remarked that the closeness of the final results was most satisfactory given the independence of the analyses [see *Jeffreys*, 1939*b*, and *The Earth* , sixth edition, p.121].

The Jeffreys-Bullen travel times remain in 1991 in

standard use for most teleseismic locations today even though the corresponding derived velocity structure has been modified in important ways. Attempts have been made to refine them drastically, so far without much change, differences in mean times generally being within the standard errors estimated by Jeffreys. A recent revision, the 1991 IASPEI Tables [*Kennett*, 1991], that combine ISC data from various regions, gives a mean offset from J-B of about 1.85 seconds for P times. The variations are mostly within the J-B standard errors.

This result is in accord with *Jeffreys's* [1977] claim that "no other global tables could give better agreement with observation for all earthquakes." In fact, he was critical of efforts to revise global tables from mixed data that ignore regional differences. After World War II, Jeffreys spent much effort developing travel times for various regions of the Earth, but as yet this type of modification remains unused in routine global hypocenter programs.

Jeffreys's interest in geological processes [*Jeffreys*, 1935] made him well aware that, in the Earth, seismic waves do not propagate through a homogeneous, perfectly elastic medium. (His last research publication [*Jeffreys and Shimshoni*, 1987] dealt with regional differences in seismology.) He certainly foresaw the need to examine the three-dimensional structure of the Earth [see *Masters*, 1989]. Because he knew that rock discontinuities and strong velocity gradients would lead to serious inadequacies in the application of ray theory, his attention turned to numerical approximations of waveforms near cusps, caustics, and shadow zones. He also incorporated viscosity and scattering. In his wave analysis, he made incisive use of the Airy integral and independently rediscovered the Green-Liouville solution of the harmonic equation with a variable coefficient. This solution is now known as the JWKB approximation (named after Jeffreys, G. Wentzel, H. A. Kramers, and L. Brillouin).

Jeffreys stressed that one of the major questions remaining in seismology was the persistence of the coda in seismograms, a problem of current interest in understanding crustal processes [*The Earth*, fourth edition, 1962, p. 399]. Much modern seismological research incorporates the application of Rayleigh's Principle by Jeffreys for computing approximate wave eigen functions, as well as his results on viscous effects on waves and wave dispersion. Perhaps the only major parts of mathematical seismology that he did not visit in depth [*Lapwood*, 1982] were the inverse fault-plane method developed by P. Byerly and the related, but more general, theory and modelling of earthquake sources. Even so, Jeffreys addressed the process of rock fracture and melting in fault rupture [e.g., *The Earth*, 1962, p. 358].

Jeffreys had the insight to see that a fresh, strict analysis of the newly accumulated measurements from many widely distributed earthquakes could provide robust estimates of the unknown velocity structure of the Earth. In the course of the development of the J-B tables, he considered thoroughly the convergent nature of the process undertaken. He initiated methods of dealing with outlying observations by developing an early form of biweights ("the method of uniform reduction"). Because for travel-time curves there is no theoretical function available that can be fitted for estimating the adjustable parameters by least squares, he developed a "method of summary values" and connected the various branches of the curves using the chi-squared test in a masterly way.

My own introduction to the stature of Jeffreys came through work on travel-times suggested to me by Bullen, who from 1945 was Professor of Applied Mathematics at the University of Sydney, Australia. *Bullen* [1957] believed that Jeffreys's research approach which incorporated Bayes's theorem and probabilistic scientific inference was a quintessential example of "the spirit of applied mathematics."

Seismic Velocity Structure in the Earth

By a nonlinear inversion, the seismic travel times lead to P and S velocity distributions of the Earth. This process is the fundamental seismological contribution to knowledge of deep Earth structure. In Figure 2, a comparison is given between prominent Earth models of Jeffreys and Bullen and the recent, widely-used PREM [*Dziewonski and Anderson*, 1981]. The comparison indicates that the velocity curves closely agree generally. The main exceptions are that the low-velocity zone outside the inner core (Region F) has now disappeared and, in the upper mantle, more discontinuities have been introduced. There are also small but geophysically important differences in detail in the narrow boundary layer D″ around the Earth's outer core.

Many of Jeffreys's other seismological contributions also use ingenious mathematical arguments. For example, a major problem in observational seismology in the early days was whether earthquakes were generated by deep sources. In 1922, *Turner* [1922] pointed out some discrepancies in travel times in earthquakes around the world, in particular from Japanese earthquakes. For some earthquakes, arrival times were later at antipodal stations than normal and earlier for others. Turner put "normal" focal depths at 200 km.

This surprising hypothesis sparked Jeffreys's interest, and he proposed as a crucial test a theorem from eigen theory which states that a mode of a vibrating system cannot be generated by a source at a node of that mode. It follows that because seismic surface waves have their motions restricted to surficial depths, they cannot be generated by deep sources. Later, *Jeffreys* [1976, p. 559] recalled that "I pointed out to Turner that ... a deep focus earthquake should excite very small surface waves or none. This could be tested by a simple inspection of the seismograms. But Turner was so satisfied by his own arguments that he would not look." The matter was settled decisively by *Wadati* [1928] in Japan, who published strong direct arguments

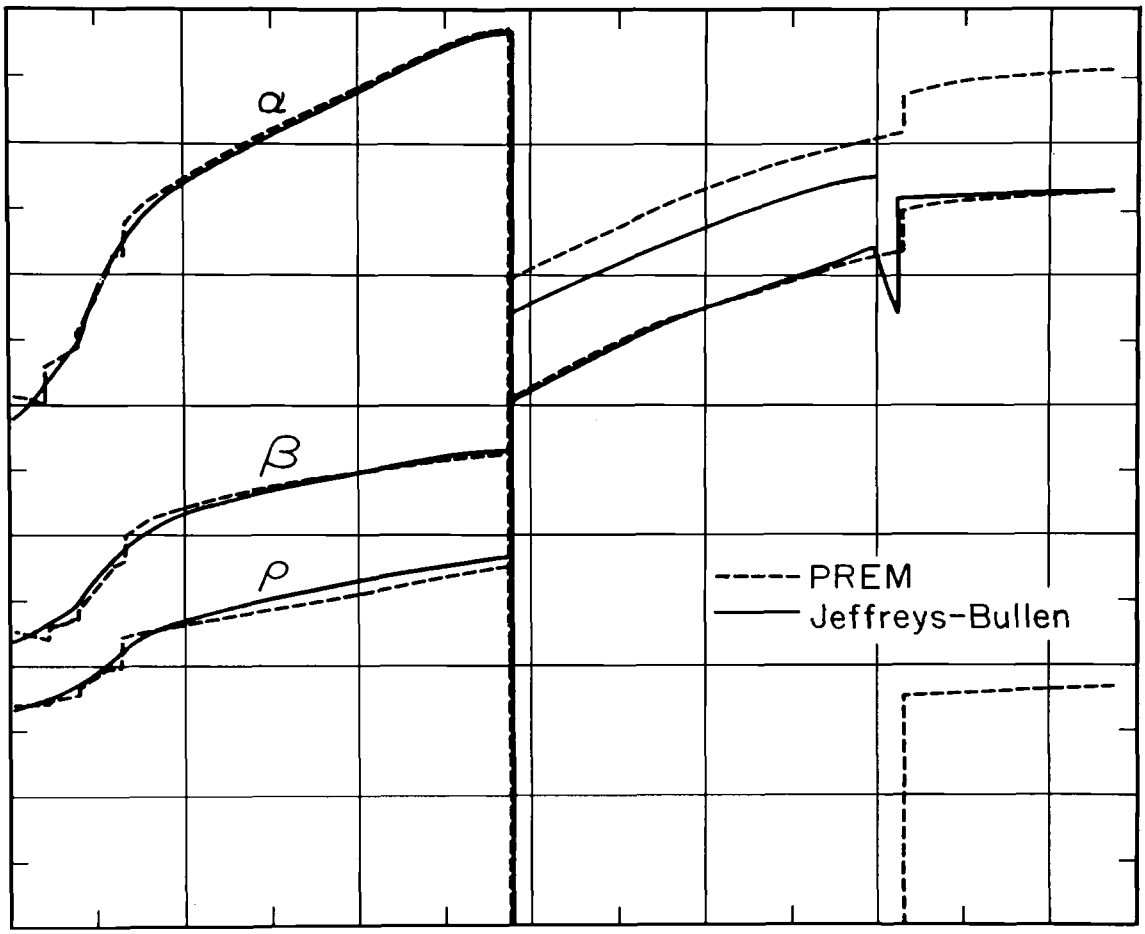

Fig. 2. Comparison of seismic P velocity (a) and S velocity (b) and density (r) distributions Model A and PREM in the Earth estimated by Jeffreys and Bullen (see *Bullen* and *Bolt*, 1985) and *Dziewonski* and *Anderson* (1981).

using local observations that showed that local earthquake sources ranged between tens and hundreds of kilometers in depth. The correct assessment of focal depth led ultimately to the mapping of subduction structure.

GEODYNAMICS

There is, of course, much more that could be said about Jeffreys's voracious curiosity about geophysics. Given the spectacular accumulation of geophysical observations of diverse types from 1924 when the first edition of *The Earth* appeared, through the era of space satellites and massive oceanographic observations, it was inevitable that many hypotheses on structure and processes of the Earth waxed and waned during his working life. Through his major contributions in geodynamics and Earth structure he became involved in highly consequential controversies concerning global tectonic processes.

The best known was Jeffreys's rejection of continental drift and doubts on the geomagnetic evidence for plate tectonics. He had worked out models which he argued demonstrated that the cooling of the interior of the Earth would strain the crust sufficiently to account for mountain structures. Jeffreys considered convection of the Earth and he saw a place for it, but his studies of yielding led him to reject mantle convection as a prime mechanism.

The reasons for the rejection are not simple and can be only summarized here. It is generally agreed that there is a basic indeterminancy problem to overcome in selecting one Earth model or process from an infinite number. Jeffreys invoked the basic principle of simplicity that he first discussed as early as 1921 [*Wrinch and Jeffreys*, 1921; see also 1931, p. 38]. A modern view of the simplicity principle and Jeffreys's contribution to its evolution in statistical inference is discussed by *Jefferys and Burger* [1992]. Given this principle, Jeffreys went on to examine the consequences of a single law to describe imperfections of elasticity — one that was valid over all wave lengths: from S waves to free oscillations, to tidal yielding, to mountain building [*Jeffreys*, 1965]. In 1977, he stated that this problem was "his chief interest at present" [p. 11].

The model process that he studied most was the "modified Lomnitz law of damping" [*Jeffreys and Crampin*, 1970]; this entails that thermal movement once started will die down. The problem with the application of the Lomnitz law throughout the Earth is that the rheological coefficients are strongly temperature dependent. Dominant terms near the surface become small at the high temperatures of the mobile mantle. Most recent study on rheological properties of the Earth also makes it probable that there is no single simple law that applies universally whatever the time scale or whatever the strain.

For many of the present generation of geophysicists, Jeffreys's name is associated with his skepticism of the hypothesis of plate tectonics. Although notions of continents moving relative to each other can be found well before, a systematic account of continental displacement was presented in lectures by Alfred Wegener in Germany in 1912. But not for about ten years, probably because of the first World War, was the hypothesis taken seriously in the geological profession. In England on 16 February 1922 a short unsigned review of a book describing Wegener's theory appeared in *Nature* [p. 202-203], stating "This book makes an immediate appeal to physicists but is meeting with strong opposition from a good many geologists." Wegener postulated the breakup of a Carboniferous supercontinent, the lateral displacement of the fragments and the matching of geological structures and of biological and climatic zones. Both support for and objections to Wegener's theory became common in the ensuing decade, with major attacks and debates in England by such geologists as *Lake* [1922] of the Sedgwick Museum in Cambridge [see *Marvin*, 1991]. In a letter to *Nature*, *Jeffreys* [1923] said that Wegener's theory depended on two assumptions: first, that any force, however, small, can deform the Earth to any sizable extent if only it acts long enough. This he did not take to be incorrect, although there was some evidence against it. The second assumption, that a small force can overcome a larger force acting for the same time in the opposite direction was, he held, inconsistent with current physical knowledge. The objection to a mechanism for the rafting of continents through an upper layer of basaltic material with definite strength was, of course, correct. Indeed, as time went on, the seismological evidence grew that the structure of continents is deeper than earlier believed. On the other hand, paleomagnetic evidence during the 1950's and 60's became strong that drift has been occurring between North America and Europe since the Paleozoic era. Further history of the construction in the late 1960's of the plate tectonic model can be found in *Cox* [1973] and *Glen* [1982].

As has been noted, the first edition of *The Earth* appeared in 1924. In this treatise Jeffreys argued for a cooling, contracting Earth which had outer layers of finite strength down to hundreds of kilometers. This hypothesis had competition from several others, notable among which was the upper mantle convection current theory of *Holmes* [1931]. Later, *Holmes* [1965, p. 1202] wrote, "Following the lead of many influential geophysicists, most geologists were reluctant to admit the possibility of continental drift because no recognized natural process seemed to have the remotest chance of bringing it about." According to *Marvin* [1991], "Not until the 1960's was the existence established of a low-velocity zone in the upper mantle, nor did it become definite that earthquakes are only generated at depths of hundreds of kilometers at very special sites. Before that there was no reason to believe that the uppermost 700 kilometers of the interior were not equally stiff everywhere."

Oliver [1991] argues that Jeffreys was clearly on the track of plate tectonics before 1952 but because the globe-encircling rift systems were only partly mapped out at that time, he believed volcanism to be always intermittent. In analyzing melting at depth in the third edition of *The Earth* [1952, pp. 289-290], Jeffreys writes, ".... we should ordinarily have in this state a solid crust resting on a less dense liquid .... such a state would be stable if the crust was thick enough, and there is no reason why it should not become permanent. If it was too thin, or if it broke anywhere under some local disturbance, instability would arise and would lead to wholesale fractures of the outer crust. Solid blocks would be continually foundering and melting on the way down, while the fluid would actually come to the surface in places." As Oliver points out, this physical reasoning led to an Earth much like the one visualized in the theory of plate tectonics today. As always, Jeffreys looked for a crucial test. His argument continues: "This would be a tempting explanation of surface igneous activity, but unfortunately it requires a continuous connection among the liquid parts at the surface, with the solid blocks separated. Actual igneous activity is always local and the crust has remained connected throughout geological time." If this Bayesian assessment could have been conditioned on the observations of the midoceanic volcanic system, Jeffreys's views may well have changed. Even so, his argument [*Jeffreys*, 1974] was clearly set out for others to modify when the critical observations on magmatic flow rates were made. His final paper on the subject was in 1978 [*Jeffreys*, 1978].

Geophysical Inverse Problems

The modern literature of geophysical inverse theory [see *Backus and Gilbert*, 1970] was created towards the end of the most productive part of Jeffreys's career and was stated in terms of an algebraic formalism not found in Jeffreys's papers. Nevertheless, from the beginning of his research he was deeply involved in inverse problems which are nowadays called estimation, resolution, uniqueness, and so on. In the first edition of *The Earth* [p. 1] we find the statement: "Thus, the problem of the physics of the Earth's interior is to make physical inferences over a range of depth of over 6,000 km from data determined only for a range of 2 km at the outside." As the earlier discussion makes evident,

although Jeffreys may not have used the term, mathematical modelling of physical processes in order to compare observations was his special mark.

As is illustrated in earlier sections, Jeffreys stressed the Bayesian approach to inference that uses previous knowledge and observations to help a scientist decide which hypothesis is most probable [*Zellner*, 1980]. He was brilliant in his treatment of uncertainty — particularly the differentiation between random errors and systematic errors. His methods are well illustrated in his analyses of gravity data [see *Jeffreys and Jeffreys, Collected Papers*, 1971-77, *3*, Section IV] and, as already mentioned, regional differences in seismological travel times.

It can be strongly argued that a clear understanding of Jeffreys's mode of thought on the structure and evolution of the Earth is impossible without consideration of his views on inference and probability. Many geophysicists do not realize that, like *The Earth*, Jeffreys's other opus, *Theory of Probability* [1939*a*] has also had great influence. Typically, Jeffreys not only developed therein a personal theory but included a wealth of applications to geophysics.

As described by *Lindley* [1991], Jeffreys was a contemporary at Cambridge of another giant in probability and statistics, the geneticist, Sir Ronald Fisher. Not only on the nature of scientific inference, but also on continental movement, were they in opposition. *Lady Jeffreys* [1991] has written, however, that "the apparent differences between Harold and R. A. Fisher have been much exaggerated." Sir Edward Bullard related the story of the day that the feud between them evaporated. After both hearing a lecture by Sir Arthur Eddington on scientific inference, they were both so horrified that they shook hands and promised not to write any more rude things about each other [*Box*, 1978, p. 422]. As statistics has evolved, the prevailing fashions have followed Fisher with his formulation of maximum likelihood, and J. Neyman with his use of confidence intervals. The essential difference is that *Jeffreys* [1977] considered probability to be the appropriate description for all uncertainty, whereas the majority of practicing statisticians restrict its use to the uncertainty associated with data.

It is of interest that *Runcorn* [1956] in his work on paleomagnetism in rock samples, turned to Fisher for a statistical way to test homogeneity in a cluster of directions of magnetization. *Fisher* [1953] subsequently produced his famous paper on statistical dispersion on a sphere, and the method was widely applied in geomagnetic investigations that became decisive evidence for tectonic plate movements.

An eminent geophysical theorist on the subject of inference of geophysical processes, *G. E. Backus* [1991] has lately reanalyzed the inverse problem of uniqueness very much along the lines explored by Jeffreys decades earlier. When prediction functionals are not linear combinations of the data functionals, prior information must be found. In cases where quadratic bounds exist, such as in modelling of the geomagnetic field at the core-mantle boundary [see *Bloxham, et al.*, 1989], Backus favors a confidence set inference (CSI) scheme derived from the developments of J. Neyman, rather than Bayesian prior probability distributions (BI). We cannot know what Jeffreys's answer would be to Backus's suggestions, but he generally took exception to such Fisher and Neyman constructs. *Jeffreys* [1977] did insist that the "actual scientific method consists of successive approximations to probability distributions." Indeed, he derived a useful form for the prior distribution of a parameter based on the log likelihood function. He says that he "once remarked to Fisher, that in nearly all practical applications we should agree, and that when we differed, we should both be doubtful" [*Jeffreys*, 1977]. Backus is, in fact, not far away from Jeffreys's position even today when he writes [1991]: "I advocate replacing BI by CSI only in certain circumstances. If prior information really is a probability distribution and the observer has confidence in it, then BI is preferable to CSI."

A quotation from Jeffreys's many stimulating philosophical writings helps us understand his insistence in geophysics (and indeed in all scientific argument) on the need for a universally consistent system of inference. "The theory of probability thus enables us to solve many of the major problems of scientific method ... it makes it possible to modify a law that has stood criticism for centuries without the need to suppose that its originator and his followers were useless blunderers." [*Jeffreys*, 1938]

Evidence for Jeffreys's continuing influence in modern times is easily found by consulting advanced texts [see *Lapwood*, 1979, 1982; *Jacobs*, 1987]. For example, in his discussion of the properties of terrestrial planets, *Kaula* [1968] writes, "The outstanding treatise of the mathematical treatment of the physics of the Earth's interior has long been *The Earth* by Jeffreys. Much of the material discussed in this chapter [On the Mechanical and Thermal Aspects of a Planetary Interior] is based on work of Jeffreys." A famous and seminal paper on the elasticity and constitution of the Earth's interior was published by Francis Birch in 1952. In his references, Birch cites the work of Jeffreys and Bridgman most frequently after his own. He states that Jeffreys in 1937 gave what he still regarded as the most reasonable values for the radii of the cores of the inner planets. I had occasion myself in rewriting Bullen's classical textbook, *An Introduction to the Theory of Seismology*, to realize how much of that book [*Bullen and Bolt*, 1985] was based upon the researches of Jeffreys in seismology. The most widely used advanced text in that subject today [*Aki and Richards*, 1980] gives a substantial list of references to Jeffreys's pioneering publications.

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