EPJ Web of Conferences **160**, 00003 (2017)
Seismology of the Sun and the Distant Stars II
DOI: 10.1051/epjconf/201716000003

Salvete

Douglas Gough[1,2,★]

[1] *Institute of Astronomy, Madingley Road, Cambridge, CB3 0HA, UK*
[2] *Department of Applied Mathematics and Theoretical Physics, Wilberforce Road, Cambridge, CB3 0WA, UK*

Abstract. N/A

It is an extremely great disappointment to me that I am unable to be with you physically at what I am sure will be a wonderful meeting. Electronic is inferior by far to physical presence. I have been temporarily banned from driving and flying, which is why I cannot be with you, although now I feel to be in fine fettle. I'm sure that the ban will be lifted after my next medical consultation, which has been deliberately delayed by the doctors until after this meeting; they seem to think that I cannot be trusted to stay home!

I have often been called the Father of Helioseismology, and I see from the list of participants that many of my offspring are present: three of my twenty-five academic children, particularly Jørgen Christensen-Dalsgaard and Margarida Cunha who have played a central role in the conception and organization of this conference, and Michael Thompson; also seven or so of my academic grandchildren, one of whom, Mário Monteiro, has also played a major role here; in addition, countless great-grandchildren, several former postdocs and many many friends. It may interest those of my mathematical offspring who are here that academically we are all directly descended from Isaac Newton, who received his MA from the University of Cambridge in 1668, and before him Galileo Galilei who graduated from the Università di Pisa in 1585. Doubtless at this meeting you will not be looking back quite so far as those great scientists, nor, I suspect, quite so far forward. In fact, ostensibly you will be looking back only as far as the original conference with essentially the same title as this, held in Cambridge in 1985 with the aim of broadening the foundations of the subject by bringing together theorists and observers who had either contributed directly to the subject already or had a mind to do so. So I shall just say a few words about what happened with helioseismology in the earliest days leading up to that conference – asteroseismology was little more than a dream at that time. Four of the participants of that conference are here today: Jørgen Christensen-Dalsgaard, Yvonne Elsworth, Don Kurtz and Ian Roxburgh. Of those I thank especially Yvonne, who has kindly stepped in to deliver the introduction that I am unable to give, on basically the development of the subject over the last three decades between the two like-titled conferences, yet no doubt from a perspective somewhat different from my own.

That first conference took place exactly a decade after the date on which I view helioseismology to have began, namely 24 June 1975. It was fifteen years to the day after I had first met my wife-to-be, Rosanne, and was at a workshop on astrophysical convection that I was organizing in Cambridge. At the suggestion of Ed Spiegel, who had recently heard a fascinating seminar by Henry Hill, I invited Henry at the last minute to present us with his news. Henry reported a discrete spectrum of oscillations in the apparent diameter of the Sun, with frequencies around 3 mHz (periods around 5 minutes) corresponding to high-order acoustic modes (p modes) of low degree. It was immediately clear to me that the spectrum was rich enough for one to be able to carry out inverse calculations of the kind that geoseismologists, then known simply as seismologists, were doing to infer the internal structure of the Earth. Jørgen Christensen-Dalsgaard and I had been computing model solar g modes, so I asked Jørgen to compute modes further up the spectrum; and the next day he was able to give a talk presenting a sequence of frequencies superficially similar to Henry's. With the inclusion of some intriguing observations of an apparent 160-minute oscillation (Severny, Kotov and Tsap, 1976; Brookes, Isaak and van der Raay, 1976), the way seemed clear to try to measure the inside of the Sun (Christenesen-Dalsgaard and Gough, 1976). Soon afterwards I became aware of Franz Deubner's (1975) pioneering observations of spatially resolved 5-minute oscillations of high degree which were so detailed as to render possible a calibration of the upper boundary layer of the convection zone. The 160-minute oscillation all but went away.

As we all know, unresolved five-minute oscillations had already been discovered by Bob Leighton, Bob Noyes and George Simon more than a decade earlier, and a degree of spatial resolution was subsequently achieved by Ed Frazier (1968) who realized that the oscillations are likely to be acoustic waves trapped in the outer layers of the convection zone. These observations had triggered theoreti-

★e-mail: douglas@ast.cam.ac.uk

cal analyses by John Leibacher and Bob Stein (1971) and by Roger Ulrich (1970), confirming Ed's interpretation. Then, soon after Franz's observations were announced, Hiroyasu Ando and Yoji Osaki (1976) carried out detailed p-mode calculations to compare with them, finding that their theoretical frequencies were systematically too high. However, until that time discussion was aimed simply at establishing the nature of the oscillations, rather than their implications concerning the structure of the Sun. It was Franz who pointed out that it was the solar models themselves that needed adjustment to fit his observations.

Fortunately Hiroyasu and Yoji's theoretical frequencies were close to the values that Franz had measured. That rendered it possible to apply perturbation theory to estimate how their equilibrium solar model needed to be adjusted to model the Sun more faithfully. It was not necessary for the adjustment to be extremely accurate for getting a first idea. I approximated the upper convective boundary layer with a polytrope, which is far from being accurate, but it gave me an analytical expression for the adjustment to the entropy jump that was required. Combining that with a relation to the depth of the convection zone, which I had recently established with Nigel Weiss (1976), I was able to estimate that the Sun's convection zone was some 50% deeper than the values preferred by solar modellers of the day (who were pushing parameters in such directions as to try to engineer agreement with the measured neutrino flux, thereby favouring a low initial helium abundance Y_0, which implied a shallow convection zone). That set the depth to be about 220 Mm, locating the transition to radiative equilibrium at $r = r_c \simeq 0.69R_\odot$. Soon afterwards, Roger Ulrich, Ed Rhodes and George Simon (1977) confirmed that result, setting r_c to lie between $0.62R_\odot$ and $0.75R_\odot$; and after some painstaking error analysis, reported only briefly in some cases, the robustness of the conclusion was established (e.g., Lubow et al., 1980; Berthomieu et al., 1980). By the time of the 1985 conference I had also made two direct estimates that were independent of any reference solar model, using an asymptotic analysis based on Duvall's Law (Duvall, 1982). The first (1984a) was made simply by inspection of the radial variation of an equatorially biased spherical average of the square of the sound speed c from data provided by Tom Duvall and Jack Harvey (1983), in which one could see the discontinuity in its second derivative. I recall getting the result one Friday morning, in the early hours – one could only compute effectively at night in those days – plotting c^2, holding the paper in such a way as to be able to squint almost in its plane, and locating the discontinuity. I grabbed a couple of hours sleep and then took the train to London where I was to present the result at a meeting of the Royal Astronomical Society later that day. The audience was incredulous, because the radiative-convective transition is difficult, almost impossible, to discern on a screen viewed at right-angles. However, I assured them that my identification had been confirmed by the only student still working at 4am in the computer room, and that he was certainly unbiased because, being a cosmologist, he had no idea where to look. The transition at the base of the convection zone appeared to be smoother than in theoretical models, indicating either a degree of material mixing or that the transition is aspherical: today Jørgen and I have seen tentative evidence that it might be both. The second estimate (1984b) relied on a separation between a (structural) function W of essentially sound speed alone and a purely thermodynamic function, Θ, the two being numerically equal in, and only in, adiabatically stratified regions. By comparing the two it was possible to identify more precisely where adiabatic stratification ceased. The method was blind-tested at the 1985 conference using theoretical eigenfrequencies kindly provided by Roger Ulrich, reproducing his depths to well within its estimated $0.005R_\odot$ accuracy. I had also carried out the analysis on new unpublished solar data obtained by Jack and Tom, the outcome of which I wanted to announce, but Jack and Tom didn't want to be included because the manner in which their data would have been presented looked, in Jack's words, "too ratty", errors in the observations having been magnified considerably in the inversion procedure, as we now know to be inevitable. So I had to hold back; all I could say was that the base of the convection zone was near to $0.7R_\odot$. The precise result was finally published six years later, after Jack and Tom had published their data (and after they had again refused an invitation to join in the publication of the consequences, this time on the grounds that they had contributed nothing beyond their own publication, an excuse which, though literally correct, was certainly not true of the overall sum of our interactions). By then I was joined by Jørgen and Michael Thompson in an extended investigation perturbing solar models. But that was after the 1985 conference, and is therefore not within the purview of this salutation.

Of the major unresolved issues concerning the Sun in those early days, there were two, perhaps one might say three, that stood head-and-shoulders above the others: the so-called solar neutrino problem, associated with which was the matter of the helium abundance, and the test of the theory of General Relativity from orbital measurements of Mercury and spacecraft, the latter by radar ranging. I shall discuss them in order.

Establishing that the convection zone was deep by the standards of the day implied, according to theoretical models of the Sun, that the zero-age-main-sequence helium abundance Y_0 was relatively high – of the order of 25 per cent by mass – which was comforting because an abundance low enough to reproduce Ray Davis's neutrino counts in a standard solar model would have required facing the problem of explaining how it could be substantially lower than what was believed to be the cosmic value immediately after the Big Bang. However, because there was evidently a serious problem with the production of neutrinos in the Sun, the theory of stellar evolution, at least as applied to the Sun, was evidently in some doubt. A deep convective envelope was only an indirect and questionable indicator of the conditions under which neutrinos are being produced in the energy-generating core. Consequently Jørgen and I (1981) undertook a calibration of solar models with the low-degree p-mode frequencies which pene-

DOI: 10.1051/epjconf/201716000003

trate into the core. At that time we made the most naive attempt imaginable, merely minimizing the difference between theoretical eigenfrequencies and full-disc observations by Claverie et al. (1980, 1982), Grec et al. (1980) and Deubner (1981), paying no regard to what aspects of the frequency spectrum pertained principally to the core. We found two promising fits, each having different mode orders n assigned to the peaks in the power spectrum, one fit with low Y_0, the other with high Y_0, the latter being marginally better, although not significantly so, given the coarseness of the procedure. We had earlier found evidence for Y_0 not being low from what are now called the large and small frequency separations observed by Gérard Grec, Eric Fossat and Martin Pomeranz (1980), the former being a measure of the acoustic radius of the Sun, the latter a signature of the core. To a first approximation, neither requires knowing n. We compared the observations with new theoretical values obtained in 1980 taking the influence of the Sun's atmosphere more carefully into account. Nevertheless, some people, such as George Isaak (1980), adhered to a former opinion to the contrary based on an earlier analysis of the Birmingham group's observations (Claverie et al., 1979) and earlier theory (Christensen-Dalsgaard et al., 1979). The reason that mode orders n could not be identified observationally is that the full-disc spectra did not extend to sufficiently low frequency for it to be possible to extrapolate reliably to $n = 0$; fortunately, at least the degrees l were known, for they could be inferred from the pattern of the large and small frequency separations.

It was Tom Duvall and Jack Harvey (1983) who came to the rescue: I had persuaded them to make crucial observations of modes of intermediate degree which connected the full-disc data to the high-degree modes whose lowest-order branch (f modes) is essentially independent of the internal structure of the Sun, and can therefore be identified unambiguously. The outcome completed the low-degree seismic calibration, selecting the solar model with the higher Y_0. That preference was reinforced by observations having low spatial resolution that were carried out by John Wilcox and Phil Scherrer (1983), which extended the range of the whole-disc observations of deeply penetrating modes up to $l = 5$. My interaction with Tom and Jack, and with John (until his untimely death) and Phil, continued. Those early days were the most exhilarating period in my scientific life, Jack and Tom, particularly, directing observations towards answering questions that I posed rather than pursuing what they could measure the most accurately, and I carrying out calculations stimulated by their new observations in the hope of shedding more light on the matters in hand, all of which, of course, raised new questions: it was textbook scientific method in action. It has been fascinating to investigate the ramifications of the new preliminary data, under the understanding, of course, that the outcome could not be relied upon until the analysis of the data had been confirmed. Working at the border of credibility, with appropriate caution, is essential for advancement. Indeed, I recall the exuberance that I experienced from one of Jack's tentative findings: it led to us entering into a serious wager about its veracity. The scientific details are of no concern here, except to say that I wagered that Jack was correct, and he that he was wrong. The matter would be decided by further observations, expected in a year or two. Jack and I regard the wager as being very serious, not just because of the scientific issue, but also because of the magnitude of the stake: it is defined operationally, the outcome of which depends on the circumstances in which the winner is determined. Unfortunately for one of us, the magnitude of the outcome is no doubt an increasing function of time, and the crucial observation is yet to be made. Perhaps a more serious matter is that Jack is now more reticent about revealing to me his preliminary observational results, on the ground that I might believe them.

The equivalence of W and Θ in the adiabatically stratified regions of the convection zone provided the means to measure the helium abundance Y directly, for in its ionization zones Θ is augmented significantly above its perfect-gas value. Werner Däppen and I (1984) made a preliminary analysis of that augmentation, finding that $Y \simeq 0.23$, which, after accounting for gravitational settling (which we did not do at first; it was nearly a decade before it was taken into account in this context), implies $Y_0 \simeq 0.25$, a result which has not changed significantly since, although there has been much more work on assessing its reliability. We also foresaw the possibility of measuring the individual abundances of the most abundant heavy elements.

I experienced an interesting consequence of the f-mode character. In view of the fact that the frequencies ω of the high-degree modes – which hardly sense the sphericity of the Sun – do not depend on stratification, but just on the surface gravity g and horizontal wavenumber k according to the relation $\omega^2 = gk$, I could assess the accuracy of published $k - \omega$ diagrams, both theoretical and observed. I recall one day finding in a new publication by Tom and Jack a plot that fitted the theoretical relation precisely. I was very impressed by the seemingly implied accuracy of the observations. I mentioned it to Jack when next we met, to which he responded with a smile, adding that on finding my paper he had realized that use of the formula was a more accurate way of gauging the image scale on his telescope than what was in use before. That formula had long been known to describe surface gravity waves in deep pure water. But it was not appreciated that the formula is more generally applicable. In fact, a different expression for the frequencies of surface gravity waves in terrestrial oceans, derived in the Boussinesq approximation, was presumed by oceanographers of the time to provide information about salinity stratification; little was it realized that the deviation from the simpler formula was actually a product of the inaccuracy of the Boussinesq approximation. The ubiquity of the formula came to the attention James Lighthill, the Lucasian Professor in the applied mathematics department in which I taught. One day at coffee time, he approached me for an explanation. It seemed to me that no sooner had I began to establish what I considered to be a fruitful way of thinking about the matter when James interrupted, saying: "Thank you; of course you're right". It's amazing how impressively rapidly some people think with such enormous perspicacity.

Jack and Tom refined their observations – or perhaps just the analysis of their data. With a view to measuring the acoustic stratification of the entire Sun, they had imaged the Sun through a cylindrical lens whose axis was aligned with solar latitude, thereby extracting just the zonal and near-zonal modes which provide a weighted latitudinal average concentrated somewhat in the polar regions. It was now possible to invert the data to obtain a direct estimate of the sound speed throughout all but the innermost regions of the Sun. With the help of Jørgen, who provided theoretical solar models with which to compare the seismological analysis together with considerable additional insight, a quite detailed picture of the solar interior was emerging (Christensen-Dalsgaard et al. 1985). In particular, it convincingly ruled out solar models with low Y_0, although, interestingly enough, that inference was not immediately appreciated by the solar-neutrino community who appeared to be unaware of the robustness of conclusions derived from such simple physics as acoustics. An interesting suggestion arising from the work – only a suggestion at the time because it concerned much more complicated physics – was that under conditions near the base of the convection zone the opacity incorporated in the theoretical models was about 20% too low. Five years later that suggestion was verified.

The body of helioseismological evidence now provided the most compelling evidence towards locating the resolution of the solar neutrino problem: it lay in nuclear or particle physics, and not in the theory of stellar structure and evolution.

In a stroke of genius Jack Harvey thought of rotating his cylindrical lens by $\pi/2$. Although that did not collapse the solar image precisely on lines of longitude, it almost did so principally in the equatorial regions where the sectoral modes were concentrated. In that way Tom and Jack were able to measure rotational splitting. They were joined by theorists to infer the Sun's angular velocity almost to the core, and, because the inversions, primitive as they were, were relatively straightforward, publication (Duvall et al., 1984) came before that of the sound speed. By far the dominant feature of the inference is that the interior of the Sun is rotating almost uniformly at roughly the photospheric equatorial rate, and not a great deal faster as most theorists of the time suspected. Indeed it immediately settled a dispute with Bob Dicke (1964, 1967; Bretherton and Spiegel, 1968), who had argued from his inference of the high oblateness of a measure of the visible solar disc that the Sun's core rotates with a period of no more than a day or so, and that the associated oblateness of the gravitational potential supported his scalar-tensor theory of gravity against General Relativity. A value of the quadrupole moment J_2 of the Sun's exterior gravitational potential of 2×10^{-7} was calculated from our seismological inference; that is consistent with General Relativity. Because the Sun's angular velocity is inferred from frequency splitting by advection of seismic waves, which depends linearly on the (small) angular velocity, and not on the much smaller centrifugal force, which is quadratic, the inference of the value of J_2 is much more robust than Bob's estimate, not to mention the difficulties that had confronted Bob in interpreting the apparent shape of the photosphere.

The 1985 conference also addressed, briefly, the seismology of other stars. At that time asteroseismology was only just emerging as a reality. There was just one definitive observation of a sequence of well defined p-mode oscillation frequencies, by Don Kurtz and Jan Seeman (1983) of the rapidly oscillating (ro) Ap star HR 1217, which heralded the beginning of the subject proper. In addition, there were strong hints of solar-like oscillations in α Centauri A (Fossat et al., 1984) and ϵ Eridani (Noyes et al., 1984), but the individual frequencies were not yet properly resolved. And certainly one should not forget that Don, the discoverer of roAp stars (1982), already had important observations of single modes of the significantly aspherical stars, which were to provide important asteroseismic diagnostics of not only the influence of asphericity on the dynamics of acoustic oscillations (Dziembowski and Goode, 1984, 1985; Gough and Taylor, 1984) but also, in due course, the means of addressing aspects of the nature of the large antipodal spots that are responsible for the asphericity. The state of the subject at that time is authoritatively reviewed by Don in the proceedings of the 1985 conference.

In addition to the issues that I have mentioned here, there was much work being undertaken to develop new instrumentation and advance the theory, the fruits from which I am sure Yvonne will address. The state of developments prior to the 1985 conference can be judged from reviews (Christensen-Dalsgaard & Frandsen, 1983; Christensen-Dalsgaard, 1984; Deubner & Gough, 1984; Christensen-Dalsgaard, Gough & Toomre, 1985) and the proceedings of conferences (Hill & Dziembowski, 1980; Belvedere & Paternò, 1984; Ulrich et al., 1984)

I have had a wonderful journey through the early days of the development of the subject, and I have enjoyed sharing it with so many of you. Many astronomers were stunned by what we had achieved, and there were some amongst them who even believed that we had accomplished all that there was to do. How wrong they were. The subject since has blossomed enormously, especially in the arena of asteroseismology. We are poised for further exciting advances, for which I'm sure this meeting will provide a solid foundation.

References

[1] Ando, H. & Osaki, Y., Publ. Astron. Soc. Japan, **27**, 581 (1976)

[2] Belvedere, G. & Paternò, L., Oscillations as a Probe of the Sun's Interior, Mem. Soc. Astron. Italiana, **55** (1984)

[3] Berthomieu et al., Nonradial and Nonlinear Stellar Pulsation (ed. H.A. Hill and W.A. Dziembowski), p.307 (1980)

[4] Bretherton, F.P. & Spiegel, E.A., Astrophys. J., **153**, L77 (1968)

DOI: 10.1051/epjconf/201716000003

[5] Brookes, J.R., Isaak, G.R. & van der Raay, H.B., Nature, **259**, 92 (1976)

[6] Christensen-Dalsgaard, J., Space Research Prospects in Stellar Activity and Variability (ed. A. Mangeney and F. Praderie, Springer-Verlag), p.11 (1984)

[7] Christensen-Dalsgaard, J. & Frandsen, S., Sol. Phys., **82,**, 165 (1983)

[8] Christensen-Dalsgaard, J. & Gough, D.O., Nature, **259**, 89 (1976)

[9] Christensen-Dalsgaard, J. & Gough, D.O., Nature, **288**, 544 (1980)

[10] Christensen-Dalsgaard, J. & Gough, D.O., Astron. Astrophys., **104**, 173 (1981)

[11] Christensen-Dalsgaard, J., Gough, D.O. & Morgan, J.G., Astron. Astrophys., **73**, 121 (1979)

[12] Christensen-Dalsgaard, J., Gough, D.O. & Toomre, J., Science, **229**, 923 (1985)

[13] Christensen-Dalsgaard, J. et al., Nature, **315**, 378 (1985)

[14] Claverie, A. et al., Nature, bf 282, 591 (1979)

[15] Claverie, A. et al., Astron. Astrophys. Lett., **91**, L9 (1980)

[16] Claverie, A. et al., Sol. Phys., **74**, 51 (1982)

[17] Däppen, W. & Gough, D.O., Theoretical problems in stellar stability and oscillations (ed. A. Noels & M. Gabriel, Univ. Li‘ege), 264 (1984)

[18] Deubner, F-L., Astron. Astrophys., **44**, 371 (1975)

[19] Deubner, F-L., Nature, **290**, 682 (1981)

[20] Deubner, F-L. & Gough, D.O., Ann. Rev. Astron. Astrophys., **22**, 593 (1984)

[21] Dicke, R.H., Nature, **2902**, 432 (1964)

[22] Dicke, R.H., Astrophys. J., **49**, L121, (1967)

[23] Duvall Jr, T.L., Nature, **300**, 242 (1982)

[24] Duvall Jr, T.L. & Harvey, J.W., Nature, **302**, 24 (1983)

[25] Duvall Jr, T.L. et al., Nature, **310**, 22 (1984)

[26] Dziembowski, W.A. & Goode, P.R., Mem. Soc. Astron. Italiana, **55**, 185 (1984)

[27] Dziembowski, W.A. & Goode, P.R., Astrophys. J., **296**, L27 (1985)

[28] Fossat, E. et al., Comptes Rendus Acad, Sci Paris, Ser. II, **299**, 17, (1984)

[29] Frazier, E.N., Zeitschr. für Astrophys., **68**, 345 (1968)

[30] Gough, D.O., The Observatory, **104**, 118 (1984a)

[31] Gough, D.O., Mem. Soc. Astron. Italiana, **55**, 13 (1984b)

[32] Gough, D.O. & Taylor, P.P., Mem. Soc. Astron. Italiana, **55**, 215 (1984)

[33] Gough, D.O. & Weiss, N.O., MNRAS, **176**, 589 (1976)

[34] Grec, G., Fossat, E. & Pomerantz, M., Nature, **288**, 541 (1980)

[35] Hill, H.A. & Dziembowski, W.A. (ed.), Nonradial and Nonlinear Stellar Pulsation, Springer-Verlag, Berlin (1980)

[36] Iben Jr, I. & Mahaffy, J., Astrophys. J., **209**, L39 (1976)

[37] Isaak, G.R., Nature, **283**, 644 (1980)

[38] Kurtz, D.W., MNRAS, **200**, 807 (1982)

[39] Kurtz, D.W., Seismology of the Sun and the Distant Stars (ed. D.O. Gough), p417 (1985)

[40] Kurtz, D.W. & Seeman, J., MNRAS, **205**, 11 (1983)

[41] Leibacher, J.W. & Stein, R.F., Astrophys. Lett., **7**, 191 (1971)

[42] Leighton, R.B., Noyes, R.W. & Simon, G.W., Astrophys. J., **135**, 474 (1962)

[43] Lubow, S.H., Rhodes Jr, E.J. & Ulrich, R.K., Nonradial and Nonlinear Stellar Pulsation (ed. H.A. Hill and W.A. Dziembowski), p.300 (1980)

[44] Noyes, R.W. et al. Astrophys. J., **285**, L23 (1984)

[45] Scherrer, P.H., Wilcox, J.M. et al., Sol. Phys., **82**, 75 (1983)

[46] Severny, A.B., Kotov, V.A. & Tsap, T.T., Nature, **259**, 87 (1976)

[47] Ulrich, R.K., Astrophys. J., **162**, 993 (1970)

[48] Ulrich, R.K., Rhodes Jr, E.J., Harvey, J.W. & Toomre, J, Solar Seismology from Space, JPL Publication 84-84 (1984)