CLOSING REMARKS

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OPEN QUESTIONS

D. O. GOUGH
Institute of Astronomy and Department of Applied Mathematics and Theoretical Physics, University of Cambridge, UK; and Joint Institute for Laboratory Astrophysics, University of Colorado and National Institute of Standards and Technology, Boulder, Colorado, USA

The principal aim of this conference has been to address the issues that pertain to our quest for understanding what is happening in the very heart of the sun, where nuclear reactions produce the energy required to replace the emission from the photosphere. As a result of this energy balance the structure of the sun has hardly varied over the last 4.6 Gy or so. The sun has belonged, and does still belong, to what astronomers call the hydrogen-burning main sequence.

There has been some slight change, however: the mean molecular weight of the material in the reacting core has been increasing with time t, as a result of the conversion of hydrogen to helium, and this has caused a gradual contraction and heating of the central regions of the star, and a consequent rise of the total luminosity L according to the formula:

$$L(t) \simeq [1 + \beta(1-t/t_\odot)]^{-1} L_\odot \quad , \tag{1}$$

where $L_\odot$ and $t_\odot$ are the present luminosity and age of the sun. The coefficient β is a numerical constant proportional to $L_\odot t_\odot/QM \simeq 0.045$, M being the mass of the sun and $Q \simeq 6.3 \times 10^{18}$ erg g^{-1} being the energy released per unit mass in the conversion of H to ^4He (thus $QM/L_\odot$ is the time that would be required to convert the entire sun to helium at the present luminosity assuming that it had started as pure hydrogen, which it did not); it is also proportional to a dimensionless parameter that depends on the functional form of the opacity $\kappa(\rho, T, X)$ and the gross nuclear energy generation rate $\varepsilon(\rho, T, X)$, where ρ and T are density and temperature, and X is the hydrogen abundance (e.g. Gough, 1990). In addition to that, it depends on what has happened to the material in the core during the main-sequence evolution, but that dependence is quite weak. Thus, if for the moment we adopt the principal explicit assumptions of the so-called standard theory of solar evolution, namely that at t = 0 the sun was chemically homogeneous and that throughout the evolution the core of the sun has been quiescent, implying that the products of the nuclear reactions have always remained in situ, then $\beta \simeq 0.40$. (If we were to have assumed an opposite, quite unrealistic extreme that the entire sun

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were to have been maintained in a chemically homogeneous state by some mixing process that was too slow to contribute directly to the energy transport, then the value of β would have been 0.3.) This value is essentially independent of the presumed initial chemical composition, provided there are heavy elements enough to dominate the opacity in the radiative interior (and provided, of course, for given total heavy-element abundance Z, or given Z/X_0, the initial value X_0 of X is chosen to ensure $L = L_\odot$ at $t = t_\odot$ as equation (1) implies).

Subject to the assumptions I have mentioned, equation (1) appears to be the most robust outcome of the theory of solar evolution that is pertinent to the history of the sun. It is because of that that I have mentioned it first, to establish some relatively secure starting position from which to admit our ignorance. It is robust because it is insensitive to the uncertain details of the internal structure of the theoretical solar model that produces it, and therefore, of course, we deduce immediately that were we able to confirm it observationally (which we shall never do directly), it would not be a useful confirmation of any of the subtle features of that model, particularly the structure of the core. However, because of that insensitivity it could in principle be used to test other aspects of the theory upon which the gross behaviour (namely the gradual rise of L with t from about 70 per cent of its value today) does depend. Thus, despite my apparent initial confidence, I must include amongst my open questions:

Is equation (1) correct? (Q1)

And then one is induced immediately to ask:

If it is not, what does that tell us? (Q2)

Of course the trite answer to question (Q2) is that one of the assumptions of the theory is incorrect. 'But which?' is then the natural response. (I do not include such natural responses to trite replies amongst my open questions.) I hope that at least the flavour of my brief introductory description of main-sequence evolution has already indicated that the answer would not lie in the details of the physics that is required for establishing such matters as the equation of state, the opacity or the thermonuclear reaction rates. The qualitative behaviour exhibited by the expression (1) for L(t) is stable to quite profound, though plausible, modifications to microscopic physics.

As I have already mentioned, the rise in luminosity comes about because the fusion of hydrogen into helium reduces the number of particles per unit mass (which increases the mean molecular weight) in the core, thereby decreasing the pressure at given density and temperature and causing the core to be compressed. In order to sustain the weight of the star the pressure must be restored, which is accomplished only by establishing a new hydrostatic equilibrium at higher density and, according to the virial theorem, higher temperature. This augments the nuclear reaction rates. The only plausible way out of this situation is to deny that the weight of the star succeeded in compressing the core. That could come about only if the weight declined with time in step with the diminution of the ability of the

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core to sustain pressure. (I am not seriously entertaining so implausible a postulate that many-body physics is so wrong that pressure does not diminish with decreasing particle number density, nor that under solar conditions it is not an increasing function of temperature. Neither am I doubting that the nuclear energy generation rate increases with density and temperature rapidly enough to overcome the opposing tendency for it to decline as a result of the decreasing abundance of hydrogen fuel; that does not occur until the end of the main-sequence phase of evolution when hydrogen is essentially exhausted from the centre.)

A decline in the weight of the sun could have occurred in either of two ways: either the total mass M of the sun or the gravitational constant G (measured in units in which Planck's constant and the speed of light are invariant) has been decreasing. Both possibilities have been entertained in the literature, and, if the manners in which they are presumed to have decreased were such as to produce the same temporal variation of L (which I assume for the moment occurs on a timescale much longer than the characteristic Kelvin-Helmhotz thermal readjustment time), would have yielded almost indistinguishable structures of the sun today. Thus we are led by question (Q2) to ask:

Has the solar mass remained constant during main-sequence evolution? (Q3)

and the bigger question:

Is gravitational physics correct? (Q4)

The latter question is intended to encompass not only the local law of gravity expressed by the governing differential equations (if, indeed, gravity can be described by differential equations), but also the cosmology that may determine the value of G that appears in the Newtonian approximation.

Of course there is always some level of precision implied by the questions. Few of them will ever by answered fully, and therefore in some evolving sense they will always remain open. As partial answers are provided, the physical implications of the questions will change as we probe into more and more subtle aspects of the structure of the inside of the sun.

By way of illustration, let us consider the specific example of the asymptotic expression for the cyclic frequencies of p modes of low degree ℓ and high order n in the form:

$$\nu \sim (n + \tfrac{1}{2}\ell + \varepsilon)\nu_o - [A\ell(\ell + 1) - B]\nu_o^2\nu^{-1} + \dots \quad , \qquad (2)$$

where ε, ν_o, A and B are functionals of the equilibrium state which do not depend on the mode of oscillation. By analogy with question (Q1) one might then ask:

Is the asymptotic relation (2) correct? (Q5)

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At some level one can immediately answer in the affirmative, particularly if one retains only the first of the two terms, and, of course, answers the question only to the precision dictated by the magnitude of the relatively small second term. One may view the question in either of two ways, depending on whether one is asking about the validity of the expression as an approximation to the eigenvalues of a certain differential boundary-value problem or whether one is asking if the sun's oscillation frequencies actually satisfy the relationship. Both aspects of the question and its answers need to be understood before measured frequencies can be used to make sound deductions about the physical state of the solar interior.

Let us therefore begin with just the leading term of the expression (2). It is immediately recognizable (at least to some) as the leading term in the asymptotic approximation as $n/\ell \to \infty$ to the zeros of a spherical Bessel function, and therefore represents the high-order frequencies of relatively low-degree oscillations of a uniform gas with sound speed c contained in a sphere (e.g. Rayleigh, 1894). The characteristic cyclic frequency ν_o is simply the reciprocal of the time τ_o taken to traverse a diameter of the sphere at speed c:

$$\nu_o = \tau_o^{-1} = (2R/c)^{-1} \quad , \tag{3}$$

where R is the radius of the sphere. The constant ε depends on the conditions imposed by the bounding surface of the sphere, and relates to the phase shift induced when a wave incident to it is reflected. If the gas in the sphere is stratified, yet retains spherical symmetry, then provided the wavelength of the oscillations is everywhere much less than the scale height of variation of the equilibrium state, JWKB analysis shows that the leading term of equation (2) is unchanged. The expression for ν_o in terms of τ_o is unchanged too, though now that the sound speed is a function of the radial coordinate r, c must be replaced in equation (3) by its harmonic mean $\bar{c}$:

$$\nu_o^{-1} = \tau_o = 2R/\bar{c} = 2\int_0^R c^{-1}dr \quad , \tag{4}$$

as was first shown in the case of adiabatic oscillations of a spherical star by Vandakurov (1967). Of course, a star is not contained within some well defined boundary, but, as Lamb (1908) first showed for the case of an isothermal atmosphere, reflection still takes place in the surface layers provided the wavelength λ of oscillation exceeds some critical value λ_c, where

$$\lambda_c = 2H(1-2H')^{-1/2} \quad , \tag{5}$$

(e.g. Deubner and Gough, 1984), where H(r) is the density scale height and a prime denotes differentiation with respect to the argument.

Since λ_c is of the same order of magnitude of H, the condition $\lambda \ll H$ necessary for the validity of the JWKB approximation is not satisfied in the reflecting layers. However, those layers are extremely thin compared with a characteristic value of λ in the deep interior of the star where the JWKB approximation is valid for high-order modes, and therefore they present themselves to the oscillations of the interior in essentially the same way as a reflecting surface; the phase constant ε can be determined by analysing the solution to the wave equation in the vicinity of reflection in terms of a simple comparison equation using Langer's technique, which has been shown to provide a valid asymptotic approximation to the exact value (Olver, 1974). Vandakurov (1967) determined how the result is related to a polytropic index μ characterizing the stratification of the outer layers of the star: he found

$$\varepsilon = \frac{1}{2}(\mu + \frac{1}{2}) \quad . \tag{6}$$

Thus we see, from a mathematical point of view, how to provide a first-order answer to question (Q5).

The physical answer was first provided by Claverie et al. (1979), from whole-disk Doppler observations of the sun, which are sensitive principally to modes with $\ell \leq 3$. The power spectrum of the oscillations revealed an array of uniformly spaced peaks, whose separation, according to the leading term of expression (2), must be $(1/2)\nu_0$, assuming that modes with both odd and even values of ℓ were present. The absolute values of the frequencies of the peaks determined the value of ε. Thus there were available an estimate of a harmonic mean of the sound speed throughout the sun, and a measure of the density stratification in the outer reflecting layers immediately beneath the photosphere. These were subsequently compared with the properties of theoretical models, but I postpone mentioning the outcome of that until after I have discussed expression (2) more fully.

The second term in expression (2) was obtained first by Tassoul (1980). Its most noteworthy feature, perhaps, is that it has the structure of the corresponding term in the asymptotic expansion of the zeros of the spherical Bessel function that determine the frequencies of high-order acoustic oscillations of a uniform sphere. [In that case $A = (2\pi^2)^{-1}$, and B, like ε, depends on the boundary conditions imposed at $r = R$.] One can show from Tassoul's analysis that

$$A = \frac{1}{4\pi^2 \nu_0} \left[\frac{c(R)}{R} - \int_0^R \frac{1}{r}\frac{dc}{dr}\,dr\right] \quad . \tag{7}$$

The expression for B is complicated, and I shall not reproduce it here; it is sufficient for my purposes to point out that it depends predominantly on conditions in the vicinity of the outer reflecting layers of the star. Once again, assuming no errors have been made, from a mathematical viewpoint one can summon Olver's analysis to affirm that expression (2) formally approximates the eigenvalues of a particular boundary-value problem in the limit $n/\ell \to \infty$. There have been further refinements to the expression, accomplished by replacing the limits of integration in equation (7) by the lower and upper turning points of the governing differential equation (Gough, 1986a), which is equivalent to retaining higher-order terms in the asymptotic sequence and which should improve the accuracy of the expression particularly when n/ℓ is not extremely large. It is important to point out at this point that for all realistic stellar models the second term in the square brackets in equation (7) is very much larger than the (geometrical) first term. Therefore a measurement of A, even with errors, provides a mean measure of the sound-speed gradient, weighted by r^{-1} and therefore dominated by conditions in the central regions of the star.

The second term in expression (2) has been confirmed observationally too, at least for the sun. This was first achieved by Grec et al. (1980, 1983) who, recognizing the dominance of the leading term of expression (2) already established observationally by Claverie et al. (1979), superposed segments of their power spectrum of solar whole-disk Doppler measurements (with frequency interval close to ν_0) in order to raise the signature of the second term above the noise. By measuring the dependence on ℓ it was thus possible both to confirm the ℓ dependence of the second term and to measure the coefficient A. Thus, Grec et al. provided the first seismic diagnostic of the state of the energy-generating core of the sun, which is the principal subject of this conference.

This brief introductory history of the early days of helioseismic diagnosis illustrates not only how questions such as (Q5) are answered progressively, with greater and greater detail and precision, and how with the answers comes more and more diagnostic information; it also shows that obtaining diagnostics of the energy-generating core from p-mode data is much more difficult than obtaining diagnostics of the rest of the sun. [The most prominent p modes in the solar spectrum have $n \simeq 25$; furthermore $(1/2)\ell \leq 1.5$ in whole-disk data and $\varepsilon \simeq 1.75$. Thus both ν_0 and $\varepsilon\nu_0$ are several per cent of the absolute frequencies $\nu_{n,\ell}$ of typical modes of order n and degree ℓ. However, the frequency separation $\nu_{n,\ell} - \nu_{n-1,\ell+2} \simeq 2(2\ell+3)(n+\ell/2+\varepsilon)^{-1}A\nu_0$ which measures the diagnostic A of the core is only about 0.3 per cent of a typical frequency; to measure A to a precision of say a few per cent, which is necessary to detect the small differences between the sun and theoretical solar models, therefore requires frequencies to be determined to a part in 10^4.] The reason is quite straightforward, though in two parts. First, stellar p modes are essentially standing acoustic waves. The contribution to the frequency from any region in that star is therefore proportional to the time taken for a sound wave to traverse that region. In the sun, the sound speed at the centre is about 2.5 times the harmonic mean $\bar{c}$, and some 60 times the sound speed in the photosphere. Therefore the wave spends comparatively little time in the central regions. The second reason is that there is a central zone of avoidance by nonradial ($\ell>0$) modes, whose radius r_t (the lower turning point of the eigenvalue equation) is given approximately by

$$\frac{c(r_t)}{r_t} = \frac{2\pi\nu}{L} \quad , \tag{8}$$

where

$$L^2 = \ell(\ell+1) \quad , \tag{9}$$

(I hope that this L will not be confused with the luminosity, for which the same symbol has been used) which therefore hardly contributes at all to the frequency ν. This latter property can be used to advantage, however, as I shall explain later. Nevertheless, it would evidently be of very great advantage to have g-mode frequencies available, in addition to the frequencies we have at hand now, because g modes sense the central regions preferentially. Indeed, unpublished inverse calculations by A. J. Cooper and myself have demonstrated that a very substantial increase in the diagnostic power of low-degree modes is achieved even with the addition of only a very few g-mode frequencies (cf. Gough, 1984). Therefore I raise the question:

Will g modes be measured? (Q6)

We have heard some discussion at this meeting of the observational difficulties, and of the recent progress with ground-based networks of observatories and the suite of helioseismic instruments on the spacecraft SOHO that are being developed to overcome them. If all that were to fail, Roget Bonnet might have the only practical answer: GONG on the moon. However, despite my congenital optimism, I am more doubtful than he that that will come to pass in my lifetime.*

Before I terminate this discussion of p-mode frequencies, from which already I seem to have digressed somewhat, I must point out that unpublished comparisons by Jørgen Christensen-Dalsgaard and myself of numerically computed frequencies of stellar models with the asymptotic expression (2) have been rather disappointing. [I should point out that the asymptotic formula (2) was derived ignoring the perturbation Φ' to the gravitational potential produced by the density perturbation associated with the oscillations; comparisons were carried out with numerical eigenfrequencies computed not only from the full (linearized) adiabatic oscillation equations, which, as Maurice Gabriel has pointed out at this meeting, are quite poorly represented by the formula, but also from a reduced system from which Φ' had been omitted.] Although for solar models the value of A inferred from the comparisons is not very different from that given by equation (7), that is not the case for main-sequence models with significantly higher or lower masses than the sun. We found also that replacing the limits of integration by the turning points r_t and R_t, with r_t given by equation (8) and R_t by the condition $\lambda = \lambda_c$ where λ_c is given by equation (5), does not improve the situation materially. There appear to be substantial errors in the asymptotic formula (2) when applied to the finite values of n/ℓ typical of observed solar p modes, which vary along the main sequence and which perchance almost cancel for the sun. So perhaps we have been fooled by the accidental consistency of the asymptotic story that has been apparently established for the sun. Should we therefore wonder:

*I contemplated challenging Roget to a wager on this issue, but was dissuaded by the difficulties of arranging payment were I to win.

Is Nature kind? (Q7)

More specifically, one might ask:

Is asymptotic analysis useful? (Q8)

I believe that neither of these are open questions. I shall address the second first, and postpone the first until later.

Notwithstanding the disappointingly poor correspondence between the absolute values of the true eigenfrequencies of low-degree p modes and their asymptotic approximation, it is likely that the functional dependence of the frequencies on the structure of the sun is given at least qualitatively by the asymptotics. In particular, the quantity

$$d_{n,\ell} = 3(2\ell+3)^{-1}(\nu_{n,\ell} - \nu_{n,\ell+2}) \simeq 6(n + \tfrac{1}{2}\ell + \varepsilon)^{-1}\nu_o A \tag{10}$$

really is sensitive predominantly to the gradient in the sound speed in the core of the sun, even though the value of A which it measures may not be given precisely by equation (7). Therefore, for example, when comparing the frequencies of a theoretical solar model with those of the sun, we are led by the asymptotics to consider combinations such as $d_{n,\ell}$ to detect errors in the structure of the core of the model; a mere comparison simply of the absolute values of the frequencies alone is far less fruitful. Other more elaborate yet simple ways of comparing frequencies, also based on asymptotic ideas, have been discussed by Christensen-Dalsgaard and Gough (1984) and Christensen-Dalsgaard (1988). I should also mention that quite simple asymptotic expressions for the frequencies of p modes valid also at higher degree have been demonstrated to yield by inversion quite accurate estimates of the sound speed throughout most of the solar interior (Christensen-Dalsgaard et al., 1985). This has stimulated refinements of the inversion procedure (e.g. Gough, 1986b; Sekii and Shibahashi, 1988; Kosovichev, 1988; Vorontsov, 1988; Christensen-Dalsgaard et al., 1989) that have increased confidence in the original inference, more about which I shall discuss later. Finally, permit me to mention also that the asymptotic expression for the frequencies of surface gravity waves (f modes) has served as an important calibrator for both theoretical and observational investigations. For large ℓ the f-mode frequencies satisfy

$$2\pi\nu \sim (LGM/R^3)^{1/2} \quad , \tag{11}$$

irrespective of the stratification of the sun. Therefore, one can use this formula for assessing errors in the eigenfrequencies of solar models (e.g. Lubow et al., 1980). I have also used it in the past for assessing errors in observational data, but that becomes more difficult when equation (11) is used to calibrate the spatial scale of the image in the telescope, as sometimes it is. I have mentioned the f modes here simply to reinforce the conclusion that the answer to equation (Q8) is undoubtedly: Yes.

I chose p-mode oscillations to open my discussion because at present they are at the heart of heliophysical research. We have in the sun an extremely valuable physics laboratory, in which many fascinating processes are taking place under conditions that cannot be reproduced on Earth, even by our gracious hosts from CEN Saclay. However, that laboratory is of little use until we have undertaken a precise determination of what those conditions are. Seismic diagnosis is the most powerful tool that we now have at our disposal for accomplishing that task, and therefore it is quite natural that it should be honed to the best of our ability. It is a means to an end, not an end itself, but a challenging means whose promised fruit is not only a knowledge of conditions inside the sun, but also, as a very important byproduct, a more profound understanding of the physics of the dynamical processes involved in the generation, propagation and dissolution of stellar waves. It is partly this double prize that makes the subject particularly satisfying to pursue.

When I was asked to deliver this closing discussion on open questions, I naturally thought of studying the programme of the meeting to judge what issues were most likely to be of interest. Should I try to anticipate what questions would be addressed, and whether they would be answered satisfactorily? After all, most spontaneous remarks need some preparation. However, on reading the title of the very first lecture, by Evry Schatzman, I realised that that would result in duplication of effort, even though the outcome might be quite different. I therefore decided that it would really be best if only during the meeting I planned what matters to discuss, so that the outcome would reflect the flavour of at least one person's reaction to the deliberations that had actually taken place. The wisdom of this decision was confirmed within minutes of the start, for I would never have anticipated the viewpoint that Jean Andouze would take in his excellent prefatory address.

I present in Figure 1 Jean's principal illustration, reproduced to the best of my memory. It shows that the thermonuclear reactions converting hydrogen to helium in the core of the sun were to have been at the centre of attention, as indeed they were. From them issue neutrinos, the detection of which on Earth provides a very important diagnostic, though of quite what we are not sure. Next, our attention is directed to the opacity, whose influence on the overall structure of the sun is greatest in the radiative midregions, between the reacting core and the convection zone. Had Jean anticipated the stir that was to be caused by Carlos Inglesias' announcement of the outcome of most recent refinements to opacity calculations at Livermore? And then we are led to the all-important acoustic oscillations, most of the energy of which resides in the convection zone occupying roughly the outer 64 per cent, by volume, of the sun. We know that there are discrepancies between theoretical computations of the neutrino flux from so-called standard solar models, particularly when they are calibrated to reproduce to the best of our ability the observed frequency spectrum of acoustic oscillations. So, in what respects is the common perception of the solar interior most seriously wrong? Could it be that wimps have been accreted from a universal sea, or not? These questions were indeed subsequently addressed, but what was not discussed explicitly is the most obvious of the questions raised by Figure 1:

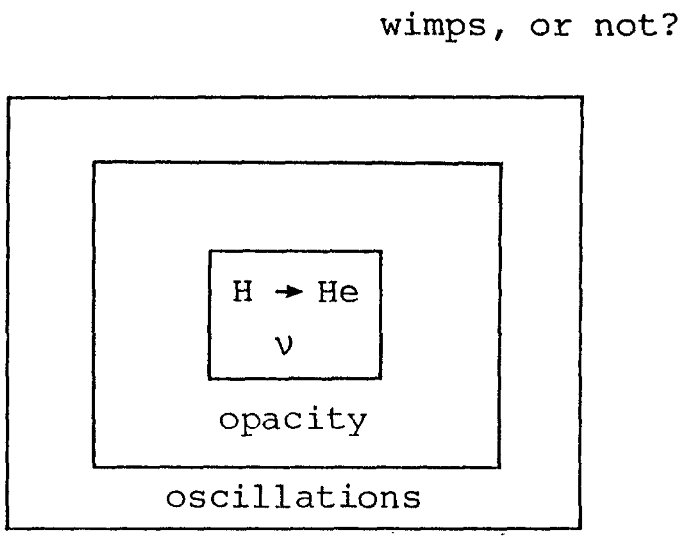

Figure 1. Jean Audouze's principal illustration.

Is the sun square? (Q9)

Of course, I intend this question to be interpreted in the general sense: namely, is the structure that of a cuboid (rather than a strict two-dimensional rectangle with equal sides)? The question must also be quite profound, for it has been known since time immemorial that superficially the sun is round: the departure from perfect sphericity, though not strictly zero as has oftentimes been believed,* has in modern times been measured to be quite small (e.g. Ambron, 1905; Dicke and Goldenberg, 1974; Hill and Stebbins, 1975). But what is the shape of the inside?

Fortunately, we now know the answer to that question. If the sun were a cube with edges of semi-length R, for example, the asymptotic expression for the cyclic frequencies ν of acoustic oscillations would have essentially the form (cf. Kurtz, 1982):

$$\nu \sim \frac{1}{2}(n^2 + L^2)^{1/2}\,\nu_o \quad , \qquad (12)$$

where, once again, ν_o is given by equation (4) (with r being any of the Cartesian co-ordinates referred to the principal axes), $L^2 = \ell^2 + m^2$, and the quantum numbers n, ℓ, and m are integers. For simplicity I have set to zero a quantity ε arising from the effective

*This belief appears to have arisen in part from Man's interpretation of God's judgement (Moses, date uncertain) that the sun was good.

phase shift suffered by the waves in the reflecting layers beneath the surface. To include this term is straightforward, as also is the generalization from cubic to cuboidal symmetry; the details are of no matter to my argument. What is important is that the observed frequencies fit the functional form of neither the relation (12) nor its generalizations, whereas they do fit the relation (2), whose functional dependence on n and ℓ (and the degeneracy with respect to m) is the signature of a sphere. Notice how much simpler this argument is than a direct comparison of solar frequencies with the eigenfrequencies of a cubical solar model. More important than simplicity is the appreciation of the signatures of the different symmetries. These can be ascertained quite generally from asymptotic analysis, at least for modes of (sufficiently) high frequency, whereas numerical eigenfrequency computations alone merely provide specific examples.

Many of the discussions throughout this week have quite naturally adopted a standard solar model as a point of reference. The question that must therefore have been raised in the minds of all those who do not already know the answer is:

Is the sun standard? (Q10)

Before even attempting to answer that question one must appreciate what it means, which requires first an answer to the question:

What is the standard solar model? (Q11)

On this issue there is some diversity of opinion, as was evident from the outset from Evry Schatzman's introduction. Evry wishes to include in the standard solar model all the generally accepted physics, including macroscopic motion, reserving for "nonstandard" models only so-called new physics. Thus the standard solar model should include such phenomena as rotation, a magnetic field, large scale circulation, microscopic diffusion, turbulent mixing and material and momentum transport by waves. In particular, it should take into account the nonlinear development of any instabilities that are found. The objective is to obtain a standardized theoretical model, within the framework of standard physics, that provides the most faithful representation of the sun possible.

That is the view of an idealist. The trouble with it in practice is that nobody understands macroscopic physics well enough to carry out the requisite calculations. Therefore each attempt to construct a standard model is likely to be different, and the resulting models would therefore not be standard. Surely it is better to ignore all these complexities, and naively construct a spherically symmetrical well-balanced model that satisfies a simple set of differential equations that can at least be solved. Provided the model did not represent conditions inside the sun too poorly, it would serve as a useful stable basis for comparison of more realistic models. This appears to be the opinion of most of those who actually compute standard solar models (or, perhaps to be more precise, the view expressed in most of the published papers presenting standard solar models), and was well presented in John Bahcall's excellent review of his view of the situ-

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ation. John and his collaborators, more than any other group, have painstakingly assessed the sensitivity of a standard model to every uncertain parameter they can think of in their description of the physics, and have addressed how observable quantities (most notably, the neutrino fluxes) are affected. Their results are summarized in two mammoth opera (Bahcall et al., 1982; Bahcall and Ulrich, 1988) which should be compulsory prior reading for anyone contemplating entering the field. What is abundantly clear from these and the many other publications on the subject is how the standard model has responded to new announcements of modifications to nuclear reaction cross sections, opacities and the equation of state. Thus theoretical neutrino fluxes, for example, have varied with time, though in recent years they seem to have hovered within a stable 6-8 snu. So at least we know the answer to the question:

Is the standard model standard? (Q12)

One of the sources of variation amongst standard solar models appears to be numerical imprecision. There is no good excuse for that. The idealized governing differential equations in the regime pertinent to the main-sequence history of the sun have no peculiar properties, and even though they are singular at the centre, that singularity (which is merely a coordinate singularity) is regular and is quite straightforward to handle. The same is true of at least the simple linearized adiabatic pulsation equations that are generally used for computing oscillation eigenfrequencies. Because of the assumption of spherical symmetry, the mathematical problem to determine the basic structure of the model is posed in only two dimensions (spanned by the independent variables r and t), so with modern computers it is in principle quite easy to attain a resolution sufficient to render truncation error, which I presume, aside from programming errors, is the major source of imprecision, negligibly small. The oscillation problem is reduced to only one dimension, and should be adequately resolved more easily. Modern seismic data, in particular, have caused us to revise our computational standards, because some physically important properties of the frequency spectrum depend quite sensitively on aspects of the model that hitherto have not been considered worthy of accurate modelling. That numerical imprecision is at least partially responsible for some of the theoretical error is exhibited, for example, by Ulrich and Rhodes, who in two separate publications (in 1983 and 1984) presented oscillation frequency spectra (presumably computed separately) of the same solar model, one of which can be fitted by the asymptotic formula (2) with standard deviation E less than one tenth of that of the other (Gough, 1986c). Please note that I use E here merely as a means of indicating the large variation of a quite subtle feature of the pattern of eigenfrequencies (the relative differences between the corresponding eigenfrequencies in the two publications is very small). I do not intend it to be used as a factor for deciding which of the frequency sets is the more accurate, particularly because at least one of the authors quite understandably regards the asymptotic formula (2) as being materially inadequate to describe the numerical results (Bahcall and Ulrich, 1988). Nor do I intend it to be inferred that the computations by Rhodes and Ulrich are less accurate than others. (Indeed, there is considerable circumstantial evidence to suggest that in some instances that is very far from the case.) I use this illustration simply because it is the best example I know of an essentially duplicated published theoretical data set that is relevant to my discussion. It shows, however, that at least some modern calculations are too inaccurate. Even though we may not understand the physics of the solar interior, it is the responsibility of every model-builder in the subject to find representations of the solutions of the governing equations that permit the determination of eigenfrequencies to a precision at least as great as that attained by the observers. Otherwise it will not be possible to know whether discrepancies are indicative of errors in the physics or mere carelessness. It is therefore of extremely great importance that Jørgen Christensen-Dalsgaard, as part of the research effort of the Global Oscillations Network Group (GONG), has undertaken to lead a thorough purge of uncontrolled numerical error in a group of solar models. The intention is that, together with his collaborators of like mind in the research group, he will thus be able to provide a solar model built with clearly defined physics to a known precision. That standard model will surely become a standard.

Acquiring a standard standard model does not imply that we would have a faithful representation of the sun. We already know that models such as those discussed at this meeting by John Bahcall and by Sylvaine Turck-Chièze and her collaborators, which I am quite sure have been computed precisely enough for their purposes, do not agree with observation (or each other), and are therefore not correct. Indeed, to my knowledge no standard model that has ever been produced is correct; in the light of Evry Schatzmann's and André Maeder's discussions, nobody should expect them to be. That surely answers question (Q10). But that does not mean that they are not useful. As André Maeder has reminded us, an essential step towards understanding Nature is understanding wrong theories.

Without doubt the most extensively discussed discrepancy is the neutrino luminosity L_ν, which is usually expressed as a flux at Earth, and often in units of neutrino capture rates in a terrestrial detector (Bahcall, 1969). One of the most outstanding questions in our subject is therefore:

What is the value of L_ν? (Q13)

To answer that question we need to know not only the neutrino flux on Earth, but also what has happened to the neutrinos (ν) during their passage from the sun. The latter issue has been comprehensively discussed by Haim Harari and Alexei Smirnov in their two excellent reviews. Between them they seem to have raised more open questions about ν creation, ν types and ν transitions than have been posed on any other issue discussed at this meeting. What was abundantly clear

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from these talks was that understanding of ν physics will come from combining information from nuclear and particle physics, cosmology and astrophysics; it cannot be achieved by any one of those branches of science alone. It is therefore of paramount importance that workers in these fields be brought together, as has occurred at this conference. Of couse it is necessary for communcation that a common scientific language be spoken, which makes me wonder whether that is in the minds of those solar physicists who insist on quoting the sun's rotation rate as a cyclic frequency. At present, however, with regard to the neutrino problem we cannot answer the fundamental questions:

Is nuclear physics correct? (Q14)

Is particle physics correct? (Q15)

The answers to some of the questions raised by the neutrino physicists will come from the various new ν detectors described early in the meeting. In some cases the role of the sun will be solely that of providing a source, whose properties need be known only approximately. The most obvious example is a potential measurement of the low-energy ν produced by the $p + p \rightarrow D$ reactions at the beginning of the proton-proton chain. Although we cannot yet answer the question:

Is the sun in thermal balance? (Q16)

precisely, we are certainly confident that the total rate of generation L_n of thermonuclear energy, almost all of which is a product of reactions in the proton-proton chain, is presently in approximate balance with the photospheric luminosity $L_\odot$. Therefore, if a discrepancy between theoretical and measured low-energy ν_e fluxes were found that is as great as that already encountered for the higher-energy ν_e, we would surely conclude that ν transitions must have occurred.

Interpreting ν data to answer more detailed questions will require more detailed and more precise knowledge of conditions inside the sun, which is partly why the programme to infer the solar structure from seismic observations is so important. The structure of the reacting core is the most valuable goal, and at this meeting we have been presented with preliminary and conflicting results of two independent attempts to determine it, one by Wojtek Dziembowski and his collaborators and the other by Alexander Kosovichev and his collaborator. We do not yet know why the results disagree, but considering the extreme delicacy of inversions of only high-order p modes to determine core structure, and bearing in mind that different unproven procedures on different data sets were employed, that there is disagreement should perhaps be hardly surprising. Nevertheless, it is important to understand the results, and to add to the excitement of that challenge I wager Wojtek Dziembowski that conditions in the solar core will be found to be closer to those estimated by Alexander and his collaborator than to those by his own group, the measure of closeness being the factor by which the neutrino flux (in snu) differs from that of the appropriate standard model, computed using the same reaction

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physics as that adopted by Bahcall and Ulrich (1988).* The underlying implictions of the wager are complicated, because they relate not only to the influence of possible errors in the data, or of errors in the inversion procedures, but also to fundamental inconsistencies that may be present as a result of incorrect assumptions that are embodied in the frequency constraints that are inverted. I have in mind, for example, deviations from thermal and nuclear balance that may have arisen from the nonlinear development of instabilities in the core. It is interesting to note, for example, that, so far as I am aware, all dynamical stability studies of the sun published in the last 17 years have found the core to have been unstable to g modes at some epoch since arrival on the main-sequence, yet modellers, as Ian Roxburgh complains, almost invariably ignore the consequences. Perhaps their growth has been suppressed at an inconsequential amplitude by resonant coupling to stable modes (Dziembowski, 1983). But if not, material and thermal redistribution in the core would have had a profound influence on L_ν. Therefore I include as an important open question:

Is the core disturbed? (Q17)

I should point out that theoretical solar models are usually calibrated to reproduce the observed solar radius and luminosity at an appropriate age $t_\odot$ after arriving on the main sequence. That calibration is an essential feature of any complete description of the sun, as Jørgen Christensen-Dalsgaard forcefully argued in his model talk. It is normally accomplished by adjusting the initial helium abundance Y_0, at given fixed Z_0 or Z_0/X_0, and the mixing-length parameter α appearing in the formalism determining the entropy gradient in the convection zone. The calibration is unique, and so yields a one-parameter sequence of solar models, each of which can be labelled with the unique value of Y_0. To choose the most appropriate model one needs answer the question:

What is the value of Y_0? (Q18)

The answer to that question has an obvious important bearing on theories of helium production during the first fifteen minutes or so after the Big Bang.

Of course one could ask whether there is a model in the sequence that reproduces the observed neutrino flux, and if there is, select that model. There is such a model, but that has a value of Y_0 (about

*At the time of the meeting Kosovichev and his collaborator had made only a very crude estimate of that factor (0.6) based on a simple extrapolation from conditions at r = 0 and assuming the entire flux to scale as the dominant ^{8}B flux. A more careful estimate (0.7) is published in these proceedings. The wager, which was accepted by Dziembowski for the stake of a bottle of cognac, is of course for the original factor, against Dziembowski's factor of 1.7, which is the reciprocal of 0.6.

0.15) which is substantially lower than the values observed in the atmospheres of hot stars, and in any case is in serious conflict with almost all cosmologies. Moreover, it has also been ruled out by oscillation data. The neutrino problem therefore remains.

Seismic calibrations of solar models to determine Y_0 have revealed since the earliest attempts (Christensen-Dalsgaard and Gough, 1980, 1981) that, even if L_ν is ignored, a model cannot be found that reproduces the observed data. The situation is basically that if one approximates the asymptotic expression (2) by its first term only, one cannot adjust the single parameter Y_0 to fit simultaneously both the global parameter ν_0 and the surface phase parameter ε which characterize the low-degree data. (If one includes the second term as a guide, and compares more subtle features of the frequencies, additional discrepancies are revealed.) Therefore there must be errors in the physics of the standard model which, as Christensen-Dalsgaard and Gough (1984) and Christensen-Dalsgaard (1988) have argued using also the frequencies of intermediate degree, must be present both in the radiative interior and in the surface layers. Attempts by others to fit the seismic data have failed similarly. However, since we do have a picture from the nature of the discrepancies of where and in what respect the solar models are in error, and, moreover, since those errors are of the kind that depend on physics in which I am sure we should not be confident, I cannot agree with the opinion expressed by Ulrich and Rhodes (1983), nor with those who have subsequently quoted them, that the significance of these discrepancies is comparable to the failure to predict the neutrino flux.

As a topical example I consider the mid-regions of the sun. An asymptotic inversion of frequencies by Christensen-Dalsgaard et al. (1985) revealed that the sound speed between $r \simeq 0.3R$ and $r \simeq 0.6R$ is about 1 per cent greater than in a typical standard model, a result that has subsequently been confirmed by several other inversions, both asymptotic and otherwise. Christensen-Dalsgaard et al. pointed out that that discrepancy could be reduced to an insignificant level in a standard model if the opacity in the radiative envelope immediately beneath the convection zone, between temperatures of about 10^6 K and 4×10^6 K, were increased by about 20 per cent. (The temperature at the base of the convection zone is actually about 2×10^6 K, and since the opacity in the adiabatically stratified lower regions of the convection zone has essentially no influence on the structure of the star, one can make no seismic deduction about the value of the opacity below abut 2×10^6 K.) More recently, Korzennik and Ulrich (1989) and Cox, Guzik and Kidman (1989; reported by Art Cox at this meeting) have reached similar conclusions. The report we heard by Carlos Inglesias that the outcome of the most recent opacity calculations at the Lawrence Livermore National Laboratory are consistent with these ideas is therefore of very great interest, for it suggests (and I suspect it will be shown that it even demonstrates) that the mid-regions of the sun are in thermal balance. This goes some way towards answering question (Q16).

The resolution of some of the discrepancies in the outer layers of the sun appears also to have been found. Beneath the upper superadiabatically stratified convective boundary layer, the structure within the convection zone is quite close to being adiabatic, and we believe that fluctuations are relatively small. Moreover, it is also likely that in this region of the sun the magnetic field is dynamically unimportant. Therefore the structure of the convection zone depends on only a few parameters: principally the value of the entropy and the helium abundance Y (which is probably the same as Y_o), provided the equation of state is known. Yet it is required to fit a whole spectrum of frequencies. Evidently, previous failures to do so precisely must reflect errors in the equation of state, and it is once again encouraging that the recent computations discussed by Jørgen Christensen-Dalsgaard using the new equation of state developed by Mihalas, Hummer and Dappen have reduced the disrepancies considerably. This is a good illustration of how the sun can reliably be used as a physics laboratory. In future it is hoped that by studying the detailed stratification in the ionization zones it will be possible at least to determine Y. My hope is even that it will eventually be possible also to determine the abundances of carbon and oxygen, and to test some of the physical assumptions upon which the calculations of the equation of state depend.

There remain contributions to the frequency discrepancies that arise from errors in the representation of the superadiabatic convective boundary layer. There, the physics of both the basic state and the oscillations is very uncertain: convective fluctuations in the macroscopic state of the gas are substantial, radiative transfer becomes important, at least near the photosphere, the complicated exchange of energy and momentum between the convection and the oscillations is significant, both directly contributing to the dynamics of the oscillations locally, and indirectly through the nonlocal influence on the eigenfunctions which modifies the dynamics elsewhere, and magnetic fields, possibly in the form of concentrated fibrils, may scatter the acoustic waves, to mention but a few of the problems. All these issues can be raised as open questions.

Finally, let us be reminded that there are errors in the core of the sun. Continuing to set aside the neutrino problem (for one of our goals is still to determine the structure of the core independently of L_ν, in order to ascertain whether the resolution of the neutrino problem is to be found as a revision of our view of solar structure, or in nuclear or particle physics), these were first detected as a discrepancy in the value of A in equation (2) [the mean value of $d_{n,o}$ is predicted to be about 10 μHz by standard solar models, whereas the latest observations yield 9.2-9.3 μHz (G. R. Isaak, personal communication; C. Fröhlich, these proceedings)], which implied, according to equation (7), that a mean value of the sound-speed gradient dc/dr is greater in the sun than in the theoretical models, the discrepancy responsible for that being most probably in the core. I have already discussed the conficting reports on attempting to elaborate on this result. I have raised the matter again here partly to complete my list of errors in current standard solar models (and thus to show that they exist almost everywhere), though mainly as a means of emphasizing how remarkable it is that we can even realistically hope to attempt to estimate the structure of the most inaccessible region of the sun from only acoustic waves which must propagate from the core through all the

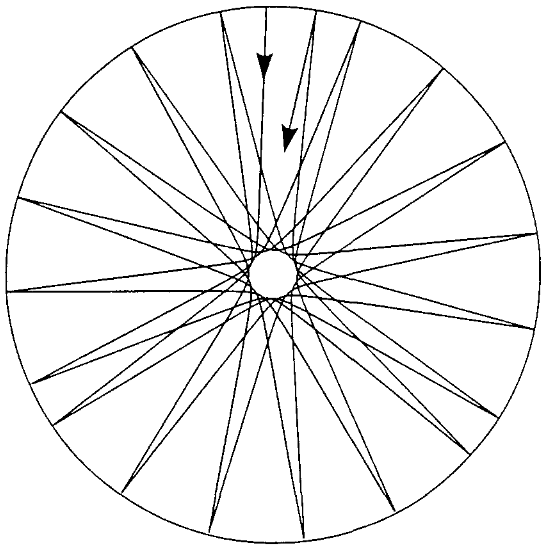

Figure 2. Ray path of an acoustic wave with $n/\ell = 5$. The radial coordinate has been distorted so as to be uniform in acoustical radius τ. Note that the ray path is not closed, which is generally the case for all nonradial modes.

other erroneously modelled and in some places ill-understood regions of the sun before reaching the surface where they are observed.

Why that is so is best understood in terms of (asymptotic) ray theory. Figure 2 illustrates the path of a typical ray of acoustic waves in a spherically symmetric star whose frequency is presumed to be such that the waves interfere constructively to constitute a resonant mode of low degree. The value of the frequency is determined by a quantization condition essentially on the sound travel time along a ray. Therefore the picture has been stretched radially, in such a way that radial distance is proportional to sound travel time $\tau = \int c^{-1}dr$ in the radial direction, in order to provide a more appropriate acoustical weighting. The most pertinent feature of the pattern is the central zone of avoidance, which is bounded by the envelope of the rays, where they form a caustic surface. This property is important for two reasons. First, the radius of the caustic surface, which depends principally on the ratio ν/L [cf. equation (8)], varies from mode to mode, whereas far from the caustic the rays of all low-degree modes are almost radial and consequently very similar. Therefore, by suitably combining mode frequencies [as a first approximation, merely by subtracting the two almost equal frequencies of modes of like $n + (1/2)\ell$ (cf. equation (2)) with ℓ differing by 2 (the smallest difference possible), which determines the quantity $d_{n,\ell}$ defined by equation (10)] the large yet nearly identical contributions from the almost radial rays which pervade most of the star can be made to cancel, and the result depends mainly on conditions within the vicinity of the caustic surfaces. Secondly, because the rays are almost horizontal near the caustic surface, they spend much more time in a given radial interval $d\tau$ of τ in the neighbourhood of the caustic than they do elsewhere, and therefore the relative contribution to the frequency from the core, though very small, is greater than one might have expected by naively comparing sound speeds. It is worth noting now that in a body with cubic symmetry, having acoustic frequencies satisfying equation (12) or a generalization of it, there is generally no caustic surface. Therefore, had the sun been square, determination of the structure of the core from acoustic modes would have been much more difficult: probably impossible with observational data at the present level of accuracy. That surely answers question (Q7) affirmatively.

Before I leave the subject of the core, I must at least mention the currently fashionable question:

Does the sun harbour wimps? (Q19)

The question has been asked by several of the contributors to this meeting. It was addressed most extensively in David Spergel's well-balanced presentation, and also by John Faulkner standing uncharacteristically (and metaphorically) at the periphery; John's planned contribution to this meeting was his entertaining discussion of the advantages of infesting other stars with wimps. The attractive feature of imagining the sun to have collected an appropriately adjusted cloud of wimps is that a theoretical model that has been tuned to reproduce the obseved neutrino flux has been found also to more-or-less reproduce the value of the seismic parameter A appearing in equation (2). However, according to Gough and Kosovichev (1988), details of the sound-speed distribution in the solar core inferred from seismic analysis are not in accord with the theoretical model C of Faulkner and Gilliland (1985) which harbours wimps. John Faulkner has chided us at this meeting for misrepresenting the case, because a rise of the relative sound-speed difference (between the sun and a standard solar model) as one approaches $r = 0$, which is exhibited in the solar inversions depicted in Figure 11 of Gough and Kosovichev (1988) and which I reproduce here in Figure 3, is not present in the representation of the wimp model in Figure 9 of Gough and Kosovichev, whereas it should be. I should perhaps first explain the reason. It was difficult to estimate the sound speed in the wimp model because, as Faulker et al. (1986) encountered, insufficient information was provided in the original paper. It was necesary to supplement the information with data from a different model, scaling appropriately, which was not wholly consistent. Moreover, those data were measured from figures, to which it was evident that some draughtsman's licence had been granted, which added to the uncertainty. Finally the hydrostatic equations of stellar structure were integrated using a pocket calculator (which explains why only a simple, though adequate, approximation to the equation of state was employed). In order to produce a solar model which accurately satisfied the hydrostatic equations, Gough and Kosovichev reintegrated those equations using the value of $\Gamma = d\ln p/d\ln \rho$ obtained by drawing a smooth curve through the data of

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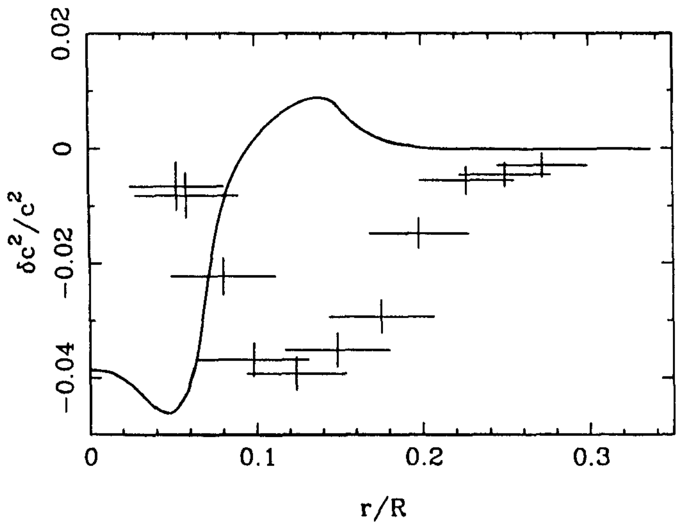

Figure 3. The crosses represent relative differences $\delta c^2/c^2$ in sound speed c between the sun and Christensen-Dalsgaard's (1982) standard solar model 1, inferred by Gough and Kosovichev (1988). The continuous line is the estimate by Faulkner, Gough and Vahia (1986) of the corresponding difference between the wimp infested and the standard model.

Faulkner et al. to define the stratification; they could have chosen c^2 instead, but did not. Consequently the value of c^2 was not identical to that inferred by Faulkner et al. To set the record straight I include in Figure 3 a curve drawn through the values of $\delta c^2/c^2$ taken from the working notes of Faulkner et al. (referred, perhaps not surprisingly, to a different standard model); the curve does show a slight upturn, but it is not as pronounced as that inferred for the sun. Therefore the conclusion that there is a substantial discrepancy is maintained. But that does not imply that the sun does not contain energy-transporting wimps. We have analysed but a single wimpish model. Perhaps some careful tuning of the several free parameters in the wimp theory could result in a theoretical model that is not yet ruled out by observation.

I shall conclude, quite briefly, with the dynamical questions that were discussed in connection with the solar cycle:

How does the sun rotate? (Q20)

What controls the solar cycle? (Q21)

We have a partial answer to question (Q20). The angular velocity, averaged in a rather complicated way about the equatorial plane, has been determined as a function of r by Duvall and his collaborators (1984). Aside from a shallow maximum at a radius of about 0.9R, this equatorial average of the angular velocity appeared to be roughly constant in the convection zone, and then declined slowly with depth until the edge of the energy-generating core.

Does the radiative interior of the sun really rotate more slowly than the convection zone? (Q22)

And if so;

Why does the radiative interior of the sun rotate more slowly than the convection zone? (Q23)

The answer to the first of these two questions can be obtained only observationally, presumably by more precise measurements.

More detailed measurements of the nonuniformity of the rotational splitting of acoustic modes have permitted us to infer the latitudinal distribution of the sun's angular velocity down to a depth of about 0.5R. The results are summarized in Figure 4. It appears that, roughly speaking, the latitudinal variation observed in the photosphere is maintained throughout most of the convection zone, and then there is a transition towards latitudinally independent rotation beneath. How abrupt is the transition cannot yet be determined, since it appears to occur on a scale no greater than the resolution length of the data. Nor do we yet know whether the radiative interior rotates approximately uniformly on spheres throughout, or whether at depths greater than that to which the current measurements penetrate the angular velocity is greater at the poles than it is at the equator. Roughly speaking, however, the value of the angular velocity in the radiative interior immediately beneath the convection zone, though lower than in the convection zone in the equatorial regions, is greater at high latitudes, and appears to be such as to lead to approximately no torque being applied between the two regions of the sun, assuming that the stress between the zones is proportional to the shear. That condition is just what is required for a steady state, and therefore goes some way towards both interpreting and answering question (Q23).

Understanding what happens at greater depths is perhaps a greater challenge. There observations are much less secure, and much less detailed.

A particularly interesting and challenging question is thus:

How does the core rotate? (Q24)

The inversion carried out by Duvall and his collaborators indicates that the core might be rotating rather more rapidly than the rest of the sun. But that conclusion depends on the rotational splitting of the lowest-degree modes, which are the least accurately determined. There is corroborative evidence for the high rotational splitting of low-degree p modes from the whole-disk observations of Isaak and his collaborators (Isaak, 1986), and some evidence from apparent g-mode

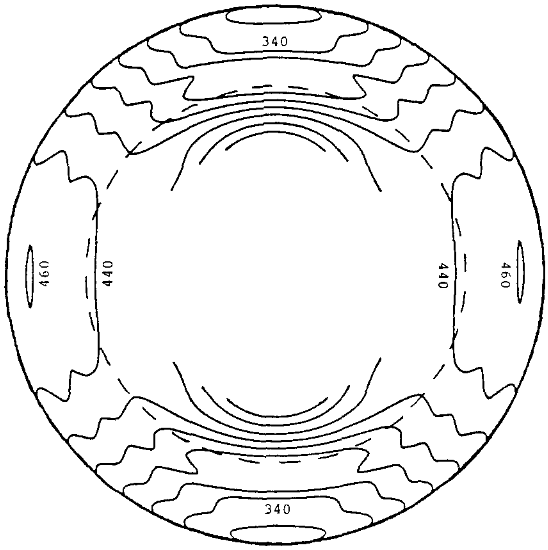

Figure 4. Contour diagram of the sun's angular velocity, estimated from the inversions by Christensen-Dalsgaard and Schou (1988), Dziembowski, Goode and Libbrecht (1988) and Brown et al. (1989).

data, but there is considerable doubt over the interpretation of those observations. The question still is quite open, so much so that Jack Harvey and I have a wager on the issue.* Indeed Claus Frohlich's comment at this meeting that his line-width measurements of dipole and quadrupole p modes are consistent with the core rotating at the same rate as the surface throws some doubt on the previous contradictory claims.

An interesting consequence of a rapidly rotating core is that it is unlikely to be pure steady rotation. Associated with the rotational shear would be a meridional component to the flow, and the whole flow is likely to vary with time. Could it be with a characteristic period of 22 years? The interesting discussions by Jean-Paul Zahn and Henk Spruit at this meeting show that the situation is not yet understood, though the recent observations have certainly triggered a redoubling of theoretical interest.

> Is the motion in the core essentially laminar, and on only a large scale? (Q25)

> On what timescale does it vary? (Q26)

*I have wagered that his observations are basically right, and he that they are wrong.

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I was interested that Jean-Paul Zahn appears to be taking the idea of ^{3}He fingers seriously. Surely fingers would act as relatively efficient absorbers of acoustic waves, and so enable p modes to transfer angular momentum between the core and the convection zone on an interesting time scale. Another possibility, which has attracted the attention of several people during the last two decades, is that there are g modes of quite high amplitude trapped in the core. As Ian Roxburgh has reminded us at this meeting, these will modify the nuclear reaction rates in the core, both directly through the nonlinearities in the fluctuations, and indirectly due to the modification brought about by nonlinear fluctuation interactions to the thermal stratification of the core. It now seems not unlikely that the outcome would be a reduction in the neutrino flux. It would also lead to a rapidly rotating core, surrounded in the equatorial regions by a region of relatively slow rotation. That is consistent with the equatorially averaged angular velocity inferred by Duvall et al. (1984) from seismic rotational splitting data. Nevertheless, one cannot allow one's thoughts to wander this far without forgetting Dziembowski's (1983) conclusion that the modes cannot attain an amplitude great enough for this phenomenon to be significant.

Associated with all these issues is the question:

> What is the geometry and the strength of the internal magnetic field? (Q27)

Henk Spruit had concluded, by reasoning I do not fully understand, that there is a sheet of field at the base of the convection zone of intensity of about 10^4 G. This is contradicted by the analysis of the even component of Ken Libbrecht's p-mode splitting data by Wojtek Dziembowski and Phil Goode, who argue that they have detected a field a thousand times more intense. This does not prove Henk wrong, however, because it is not yet known whether the p-mode splitting is produced by a magnetic field. One possibility is that it is a latitudinal variation of the thermal stratification at the base of the convection zone. Wojtek Dziembowski has argued against this on the ground that one cannot induce asphericity by purely thermal means without destroying hydrostatic balance. The ground cannot be denied, but its relevance can certainly be questioned. Kuhn (1988) implicity did just this, by disregarding hydrostatic balance when trying to explain splitting data in terms of sound-speed variations, in the face of previous calculations that had taken some account of the hydrostatic constraint and had thereby failed to reproduce the earlier splitting data of Duvall et al. (1986) without producing a photospheric temperature variation that was implausibly large (Gough and Thompson, 1988). It seems likely, therefore, that the mean stratification in the convection zone is not in strict hydrostatic equilibrium.

A rough estimate of the large-scale flow velocity that would thus be driven by the hydrostatic imbalance resulting from a thermal distortion of magnitude sufficient to produce the p-mode splittings is quite moderate, and does not seem to be contradicted by observation. Such a flow, varying through the solar cycle, is likely to modify the latitudinal variation of the angular velocity of the sun, on the rectified 11-year period. Whether such a modulation will be detected in rotational splitting data in the foreseeable future is certainly an open question.

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