CONCLUSION

SOUNDING SOLAR AND STELLAR INTERIORS: CONCLUSIONS AND PROSPECTS

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1. Introduction

I have been charged to summarize the mood of the symposium, although not to summarize the scientific details, for they are best learned by reading the proceedings. To the best of my ability, I shall comment on those details in a manner that Philippe Delache might spontaneously have done had it not been incumbent upon him to cover everything. Therefore I shall be somewhat personal. I shall make no attempt to offer a balanced view of the observations and calculations that have been reported, but instead concentrate on how they might be viewed. In so doing I bear in mind that Philippe was not one to tow the party line. He often ignored the democratic means by which unfortunately so-called scientific truth is too often established today, and instead showed more interest in the unusual. Although such behaviour can sometimes lead one into danger, as it did with regard to Philippe's proposed explanation of the infamous 160-minute oscillation of the sun (Arvonny, 1983), it is also the most common road to true discovery.

Philippe would have been pleased to observe that one of the most prominent features of the meeting is the large number of young people present. A substantial fraction of the invited scientific papers and contributed posters is the work of a new generation of scientists. It indicates that heliophysics, and particularly the seismology used to probe the internal structure and kinematics of the sun, and of other stars and planets too, is a growing vibrant discipline.

We have been treated with some excellent descriptions of how one makes inferences from helioseismic data (e.g. Basu, 1997; Sekii, 1997). Basically, one observes some variable v on the surface of the sun, be it a velocity

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or intensity fluctuation, which one relates theoretically to the underlying structure X of the sun via an operator $\mathcal{O}$ such that $\mathcal{O}X = v$. The object of the investigation is to infer X from v. We know that the inverse $\mathcal{O}^{-1}$ of $\mathcal{O}$ does not exist, so it is necessary to replace $\mathcal{O}$ by some other operator $\overline{\mathcal{O}}$ which acts on some property $\overline{X}$ of X which can be obtained from v; inference then results from interpreting $\overline{X} = \overline{\mathcal{O}}^{-1}v$. The search for operators $\overline{\mathcal{O}}$ that lead to straightforwardly interpretable functions $\overline{X}$ is the art, rather than the science, of inversion. Typically $\overline{\mathcal{O}}$ is designed to render $\overline{X}$ a spatial average of X, which provides a somewhat blurred image of the actual structure X; however sometimes it is more expedient to attempt to answer specific questions about more global properties of X directly.

Since most of the progress reported at this symposium concerns the sun, I shall concentrate on helioseismic inference. The final goal of helioseismology is not merely to measure the solar interior. In common with other branches of science, measurement is merely one step towards our true scientific goal: to understand why the sun is as it is, and ultimately to understand the fundamental physics that controls the sun, the other stars, and also the planets. An integral part of the process of understanding the underlying physics is the construction of a theoretical model, for only then can we relate X to that physics. Necessarily, we start with the simplest model that embodies what we presume are the essential physical ingredients, and typically we add complexity only when forced to do so by the data. That model is spherically symmetrical and in hydrostatic equilibrium. It is also in thermal balance. Even though it changes with time, as a result of modifications (which one trusts are improvements) to externally calculated physical relations such as the equation of state or the dependence of nuclear reaction rates or opacity on thermodynamic state variables, the model is called the ‘standard solar model’. Aspherical perturbations, including macroscopic motion, are treated as low-amplitude deviations from the structure of the standard model; their reaction on that structure is usually ignored. (This remark does not apply to thermal convection. However, the asphericity of convection is not taken explicitly into account in the models; instead, a formalism is adopted which provides a spherically symmetrical equation for the heat flux and, occasionally, the turbulent pressure.) The influence on the global structure of genuine temporal variations, such as the solar cycle, on timescales less than the nuclear evolution time is rarely considered. The reader is referred to Janine Provost’s article in these proceedings for further information. Thus, the standard solar model provides a basis with which to compare and to appreciate reality. It is not to be believed: it is simply to be used.

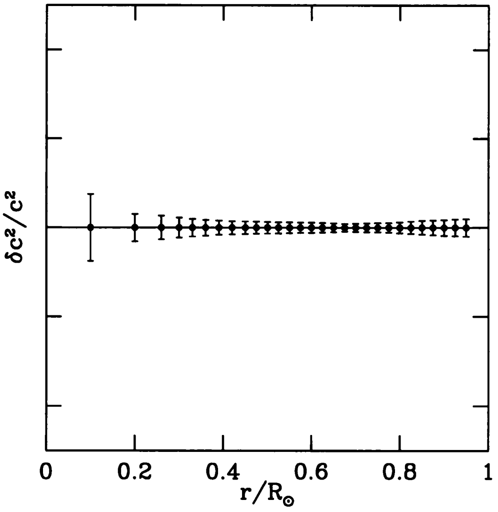

Figure 1. Generic (and somewhat inaccurate) representation of the relative difference $\delta\overline{w}/\overline{w}$ between localized averages $\overline{w}$ of any seismic variable w of the sun and the corresponding variable of the standard solar model. The filled circles represent the values of $\delta\overline{w}/\overline{w}$, and the error bars represent ± one standard deviation from them. The extent of the horizontal lines (not visible in this figure) through the filled circles measure the characteristic width of the weight function in the averages. The ordinate scale is a decreasing function of time, so I have not printed it as a protection against premature obsolescence. (Please note that no responsiblity for the accuracy of this figure should be laid on the shoulders of S. Basu or any of the others who have presented inversions at this symposium.)

2. The seismic structure of the sun

The seismic structure of the sun is that aspect of the stratification that is accessible to direct seismological investigation. Thus, it includes the variation with position of pressure p, density ρ and the first adiabatic exponent $\gamma_1 = (\partial ln p/\partial ln \rho)_{ad}$, and of any thermodynamical function of them.

It does not include the temperature, for example, because that is related to the seismic variables only through an equation of state which depends on the chemical composition. To infer temperature requires one to adopt additional, nonseismic assumptions, such as thermal balance coupled with the 'prior knowledge' of the mechanisms by which heat is transported. It is always useful to adopt the assumptions of the standard solar model in the first instance, but one must continually be mindful of the possibility that those assumptions may not apply to the actual sun.

To the best of my memory, the current status of the seismic investigations reported by Sarbani Basu in these proceedings is broadly summarized by Figure 1. Many scientists have hailed the immediate implication of that figure as a tremendous success for solar modelling and for helioseismology. But when I first perceived it, my reaction was to become profoundly depressed. For it seems to be telling us that the standard model is right. Surely we have not been labouring for so long merely to demonstrate that there is nothing more in the sun than has already been taken into account in so naive a theory as that which has been used to construct the standard models. The sun cannot be so dull. My second reaction, however, was to become cheerful. If, like Philippe Delache, one believes that indeed the sun actually is interesting, then the implication is that knowledge of what is perhaps most interesting is not to be given to us easily: there are further challenges that we must face before the true nature of the sun is revealed. Therefore there is satisfaction yet to be sought by the new generation of heliophysicists.

3. Systematic errors

It is a common feature of all the inversions that have been carried out to date that there is considerable uncertainty in the inferred structure of the energy generating core. The reason is that the results depend heavily on the frequencies of just the few low-degree p modes that penetrate into the innermost regions of the sun. Roughly speaking, the contribution of any region in the sun to the frequency of a mode is proportional to the time a propagating component of the mode spends in that region. Notwithstanding the stationarity of radial propagation in the vicinity of the lower caustic (turning point), the sound speed is so high in the core that core structure imparts only a minute signature on the frequency of a mode. Consequently there is a great danger that observational error, particularly any systematic component of it, will mask the true signature. This applies not only to multiplet frequencies, but to degeneracy splitting too.

Because of its extreme importance to core inference, I have always been interested in systematic errors. One of my earliest estimates of their influence was made with the encouragement of Philippe Delache during the 1983 conference on helioseismology in Catania (Belvedere and Paternò, 1984). Our concern there was with rotational splitting, and we were trying to reconcile the conclusions drawn from early disparate observations of low-degree modes. The error was presumed to result from the rotation of active regions on the sun, which biases the peaks in the power spectra of whole-disc Doppler data and of the projections of spatially resolved Doppler signals onto spherical harmonics. It was already known that solar activity induces a significant low-frequency signal in at least some Doppler data (Anderson and Maltby, 1983; Durrant and Schröter, 1983; Edmunds and Gough, 1983), but how should that bias the frequency splitting? The conclusion (which it was decided not to publish) was that the bias was small (actually miniscule) compared with contemporary differences between observers' reports. Therefore some other explanation of the observational discrepancies needed to be sought.

I returned to this problem of activity-induced bias nearly ten years later, this time in collaboration with Philip Stark, as a result of a statistical analysis which revealed that one of the sources of significant solar-cycle variation in the apparent rotational splitting measured by Libbrecht and Woodard (1990) was in the activity belt (Gough and Stark, 1993). The old calculation that had been carried out at Philippe's instigation was resurrected. Once again the bias was found to be small, and nothing other than a statement of the idea was published. The reason why the bias is small is that it is the product of two small quantities: one represents the relatively small contaminating influence that activity has on the frequencies, the other comes about because activity biases splitting by an amount proportional to the small difference between the rotation rate of the activity and the precession rate of the standing modes of oscillation.

In the meanwhile, interest in activity-induced bias in the inferred multiplet frequencies was stimulated by the emerging disparate inferences in core structure reported in 1988 at the helioseismology meeting in Tenerife (Rolfe, 1988). Philippe was there. The key point was that solar activity influences the even component of degeneracy splitting, which has a tendency to bias the centre of power, particularly in spectra of whole-disc measurements in which, for any degree l, substantial contributions from blended modes of all azimuthal orders m for which $l+m$ is even are present. Once again the outcome was found to be very small, and once again it was not published (this time, for reasons beyond the control of the author). Another source of bias is simply the global frequency shift due to solar-cycle changes in the spherically averaged structure of the outer layers of the sun. Although procedures to eliminate such effects on consistent data sets are incorporated into modern inversions, they cannot eliminate them entirely when two different data sets are combined, particularly if the data were analysed differently and, perhaps more importantly, if they were obtained in different epochs. Both of these sources of bias have been investigated recently (Gough, Kosovichev and Toutain, 1995) when combining the medium-degree data of Libbrecht, Woodard and Kaufman (1990) with low-degree data from IPHIR (Toutain and Fröhlich, 1992) and BiSON (Elsworth *et al.*, 1991, 1994). Once again they were found to be insignificant.

The issue of multipole bias has been looked at again, quite recently, by Dziembowski and Goode (1997). Yet again, the influence on inversions was found to be small, and is (probably) currently insignificant, but now only marginally so. No doubt in the next generation of inversions it will have to

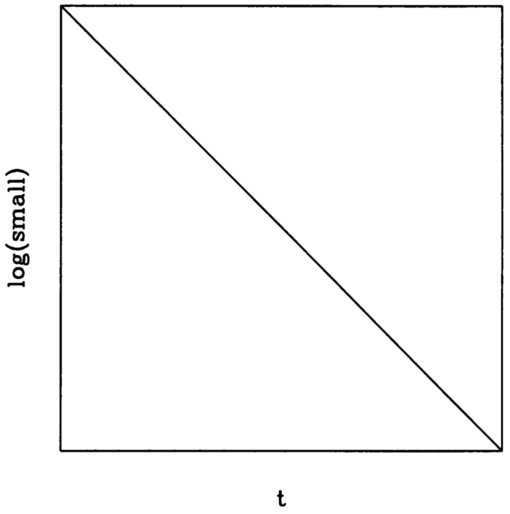

Figure 2. The logarithm of what is meant by 'small', plotted against time t.

be taken into account. Why should that be?

The reason is quite straightforward, and is illustrated in Figure 2. To some extent modern improvements in analysis techniques might be removing some of the bias, but more significant is the reduction of random errors. That changes what we mean by 'small', and once 'small' is smaller than the bias, the bias must be taken into account. Fortunately (for the inversions), almost all of the solar-cycle frequency change is produced by structural changes in the surface layers of the sun, and it can therefore quite easily be removed from the low-degree modes using the frequencies of modes of intermediate or high degree.

In what manner do the structural changes to the sun associated with the solar cycle modify the frequencies? Of course, there is the direct influence of what is presumably an augmented mean magnetic field at solar maximum in the outer layers of the convective envelope and in the atmosphere. That would increase the oscillation frequencies, by providing an additional restoring force. But there are indirect consequences of the changing magnetic activity, such as modification of the mean stratification of the upper convective boundary layer, and modifications to the convective fluctuations. The former cannot easily be reconciled with the frequency changes, and indeed appear to change them in the opposite direction to that observed (e.g. Gough and Thompson, 1988; Goldreich *et al.* 1992; Balmforth *et al.*, 1996). The effect of the latter is less clear. Brown (1984) discussed the effect just of advection by the velocity field, which decreases the frequencies, but the associated temperature fluctuations have the reverse effect. Indeed, it was another issue which interested Philippe Delache, who stimulated a rough assessment of the combined effect of all the convective fluctuations during the study programme at Santa Barbara (Gough and Toomre, 1991) using one of Nordlund's computer simulations. The outcome of our rough trial suggested, contrary to suspicions evoked by the usual convection scaling arguments, that there was a large degree of cancellation between the opposing influences, leaving the magnitude of the frequency shift substantially smaller than the earlier estimates, and its sign uncertain. It is encouraging that more serious work is now being undertaken in this important field, some of which has been reported at this meeting (e.g. by Rosenthal).

4. Gravity modes

It has long been known that much of our difficulty in measuring the structure of the core would vanish if we knew the frequencies of a few identified g modes. Gravity modes have a great advantage over p modes, namely:

> Gravity modes are concentrated near the centre of the sun.

For that reason, unlike the case for p modes, the frequencies of g modes are very sensitive to the structure of the core. And for that reason, Philippe Delache expended a good deal of effort in their pursuit (e.g. Scherrer, these proceedings).

It is interesting that knowledge of only a few g-mode frequencies would enhance the inversions substantially. The reason is that in the delicate combinations of p-mode frequencies required to cancel out the influence of the structure of the solar envelope to infer even the crudest properties of the core, other information is lost in the noise. Therefore to relieve the p modes of this task leaves them free in other combinations to provide yet more subtle information. Indeed, the addition of only a single internal g mode to the data set would improve matters enormously. But, of course, a few would be better. We cannot hope for more.

As we have learned at this symposium from the latest reports on the current status of the networks and the helioseismic instruments on SOHO, g modes have not yet been found. The reason is that they suffer a grave disadvantage not shared by most of the p modes, namely:

> Gravity modes are concentrated near the centre of the sun.

Consequently, their amplitudes at the photosphere, where we try to observe them, are on the whole small compared with the amplitudes of p modes of comparable degree having the same energy. According to Kumar (these proceedings), if interaction with the turbulence in the convection zone is the principal source of excitation, the expected energy in the mode varies only

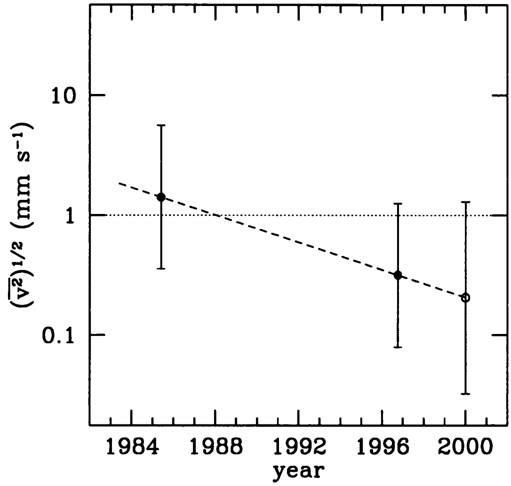

Figure 3. Estimates of the rms surface velocity amplitude of the gravest solar g modes, plotted against the time at which the results were announced. The filled circles represent theoretical estimates of the amplitudes of the modes (Gough, 1985; Kumar, 1997). The error bars were estimated from the uncertainties in the convective fluctuations estimated by Gough (1977) assuming that numerical parameters of the *calibrated* mixing-length formalism used to calculate the solar model are each uncertain by a factor 2 and that the uncertainties are independent. No contribution to the uncertainty from the approximations in the theory of excitation has been included. The open circle was obtained by linear regression, and its associated error bar was obtained assuming that the errors in the theoretical estimates (on the log plot) are independent and normally distributed. The horizontal dotted line is the ultimate threshold of detectability estimated by the observers. If that threshold is achieved by 1 January 2000, and if the assumptions upon which the extrapolation depends are valid (which is unlikely), then the probability of g-mode detection by the beginning of the year 2000 is 0.19.

weakly with frequency at low frequency. However, there is a rapid variation with frequency of the modal inertia, rendering the predicted power in the observations to be a rapidly increasing function of frequency.

It is, of course, of great interest to judge how likely it is that g modes will be detected, identified and have their frequencies measured. Therefore I compare the maximum expected amplitude with the anticipated threshold of measurement. For years George Isaak has assured us that an rms velocity in the solar atmosphere of 1 mm s^{-1} (and probably not much lower) is attainable, and Eric Fossat (e.g. 1985) concurs. Moreover, we have seen at this meeting that the latest observations are within a stone's throw of that value. How does that compare with theoretical expectation?

Until this meeting, to the best of my knowledge, there was only one estimate of the amplitudes of the gravest (i.e. lowest degree and order) modes (Gough, 1985). At this meeting Pawan Kumar has offered us a second, which provides us for the first time with a basis for extrapolation. The two estimates are plotted in Figure 3. It is difficult to know how the value will change as the theory is refined – I hope that the matter will be settled before there is time for much further refinement. However, linear extrapolation, although it is unreliable, suggests that the real amplitudes (to be calculable and, it is hoped, measurable in the future) are even lower. Fortunately, the observational upper bounds are decreasing somewhat faster. I do not represent them in Figure 3, however, but instead include the estimate of the ultimate threshold promulgated by the observers. Unfortunately, the intersection of the two lines does not tell us unambiguously whether a useful detection will be attained by the end of 1999, which for some of us is a criterion of considerable interest (Gough, 1995).

I should not end this discussion without mentioning a recent claim by Thomson *et al.* (1995) to have detected solar gravity waves propagating through the solar wind, not least because it is the result of an unusual investigation of a character that delighted Philippe Delache. There is a sequence of sharp peaks in the power spectrum of particle fluxes measured on the spacecraft Ulysses with frequencies corresponding to those of the solar oscillations – both p modes and g modes. A sophisticated statistical comparison of the frequencies of the fluctuations in the wind, after modifying them with a Doppler shift which it was hoped would account for the motion of the spacecraft relative to the spiralling wind, with the known solar p-mode frequencies led Thomson *et al.* to conclude that the two are causally connected. The argument is not wholly convincing. However, if one accepts it, then it should not be too unreasonable to accept that the peaks in the power spectrum in the g-mode frequency range are signatures of solar oscillations too. Not surprisingly, there has been considerable debate amongst scientists about this claim, much of it concerned with interpreting the frequencies as (extremely) high harmonics of the solar rotation, but there are yet few publishable conclusions. To convince most of us would require a substantially more extensive analysis of the data, particularly in the p-mode range where the presumed source of the waves in the wind has been measured directly.

5. Linewidths, excitation and damping

In his review of mode excitation, Pawan Kumar discussed the issue of the linewidths in the power spectra of acoustic oscillations. These constitute one of the important ingredients in the theoretical computation of the oscillation amplitudes. There have been several observational reports of linewidth measurements. Libbrecht and Woodard (1991) showed that, when scaled with the inertiae of the modes, they depend essentially on frequency alone.

This is as one would expect, since the strongest damping and excitation processes take place in the outer regions of the sun, mainly, we believe, in the upper superadiabatic boundary layer of the convection zone.

On the whole the linewidths increase with frequency ν, but there is an interesting dip in Libbrecht's data centred at about 3 mHz (Figure 4). Why should that be? It appears that it might be due to the influence of convection, since all calculations that ignore convection find the linewidths to be monatonically and smoothly increasing with ν. Christensen-Dalsgaard *et al.* (1989) compared the theoretical predications with Libbrecht's (1988) observations, and found tolerable agreement with a computation in which the modulation of the turbulent heat flux and Reynolds stress (turbulent pressure) by the oscillations had explicitly been taken into account, admittedly using only a very crude time-dependent local mixing-length theory. Subsequently, Balmforth (1992) obtained considerably better agreement using a nonlocal generalization.

Some solar physicists have believed – and perhaps still do believe – that the dip represents a transition between two different damping processes, one which predominates at low frequency and the other at high frequency. But the theory that predicted the dip had the same mechanism operating throughout. According to the theory, the properties of the modulation of the convective heat and momentum fluxes, which determine the energy exchange between the oscillation and the background state of the star, depend locally on the ratio σ of the characteristic turnover time of the convective eddies to the period of oscillation. Roughly speaking, the frequency range in which the damping rate is low compared with a smooth curve is that in which the spatial phase of maximal coupling of the eigenfunction with the convection occurs at a level in the star at which the value of σ is such as to cause minimal damping.

In his presentation, Pawan Kumar discussed none of the theoretical predictions, but instead offered an adjustment computed *post hoc* by Murray (1993), which is reproduced here in Figure 4. Perhaps that is because Murray's adjustment has attracted more attention in scientific conversations than the truly theoretical calculations, such as those carried out previously by Balmforth (1992). The reason, apparently, is that it is well known that the theory upon which Balmforth's calculations are based is highly uncertain. So what is the basis of Murray's computation? It is summarized in the caption to the figure (which could perhaps have been read only by those sitting in the front few rows). Murray considered damping from wave scattering by turbulence, and simply reduced the damping rate artifically in such a manner as to make the outcome appear to agree with the observations. I say this not to belittle the computation, but to make explicit what was actually done, in order that the result can properly be appreciated.

One can then recognize that Murray's result is important, for it confirms our view of how the line widths are controlled.

The manner in which Murray's adjustment was accomplished can be understood by translating frequency into eigenfunction phase. Any localized perturbation to the solar model, or its interaction with the oscillations, has an oscillatory signature with respect to frequency: as frequency varies, so does the phase of the eigenfunction at any specified location, and a perturbation at that location produces an oscillatory contribution to the (complex) frequency when that location lies in a propagating region of the mode (cf. Gough and Thompson, 1988; Vorontsov, 1988). For perturbations in the outer layers of the sun, the apparent 'frequency' of the oscillatory contribution is twice the acoustical depth of the location of the perturbation, namely $2(T-\tau)$, where $\tau(r)$ is the acoustical radius of the perturbation and T is the acoustical radius of the sun. It is evident from a comparison with observation of Murray's unadjusted theory that an oscillatory contribution with a 'period' of about 2.5 mHz is required. Therefore an appropriate reduction of the scattering at an acoustical depth of $(5\ \text{mHz})^{-1} = 200$ s does the trick. That location is at $\tau/T \simeq 0.94$, corresponding to $r/R \simeq 0.9965$ (cf. Gough, 1990), which is in the middle of the hydrogen ionization zone, about half a wavelength of a 3 mHz eigenfunction beneath its upper turning point. Murray accomplished the adjustment by multiplying the scattering by a positive power of $\gamma_1 - 1$, which is smallest in the hydrogen ionization zone.

It is important to realize that none of the calculations incorporates the whole story. Murray's calculation ignores nonadiabatic effects and the interaction of the oscillations with the perturbed Reynolds stress; Balmforth's calculation ignores scattering off spatial inhomogeneities. Since all three processes are weak compared with the dominant dynamics of the oscillations, to a first approximation one might simply add the contributions to the damping rate. Unless scattering is relatively unimportant at frequencies near 3 mHz, it seems likely that agreement with observation will not be achieved without at least some adjustment of the scattering process discussed by Murray, because Murray's *ab initio* theoretical estimate of the contribution to the linewidth by scattering alone exceeds the observed values.

It is important to realize also that there is considerable uncertainty in the magnitude and form of the turbulent convective fluctuations, even after the formalism for calculating the heat and momentum transport has been calibrated to yield a complete theoretical model of the sun having the observed luminosity and radius (Gough, 1977). Therefore there is no doubt adequate leeway to reconcile theory with observation. It is my opinion that in the fullness of time the observations will be used in the reverse sense: to calibrate turbulence theories. Rosenthal's contribution in these proceedings is a step towards that goal.

Another indication of the properties of the turbulent fluctuations, and how they interact with the oscillations, is provided by the statistical distribution of mean power. This was discussed by Yvonne Elsworth in her review. The long BiSON data set has provided a valuable confirmation that on the whole the energy in the modes satisfies the Boltzmann distribution (Elsworth *et al.*, 1995; Chaplin *et al.*, 1995), which is as one would expect if the modes are excited stochastically by the turbulence in the convection zone. But also, it reveals that the frequency of occurrence of highly energetic modes is greater than Boltzmann. There are two posters at this conference addressing the issue. Relative fluctuations about the expected frequency of the common low-energy occurrences are small, and the data adhere well to the most likely distribution, irrespective of the details of the excitation process. But in the high-energy tail there remains some legacy of the excitation, and because that process is superexponential, the distribution that is realized lies above the Boltzmann line. Once again in the fullness of time, further study of this distribution might lead us to a better understanding of the convective motion.

Another way to calibrate convection theories is by studying the peaks in the power spectrum at frequencies above the critical cutoff frequency of the atmosphere. This is perhaps a more direct method, for, unlike the trapped modes, these waves suffer almost no reflection in the surface layers of the sun, and instead propagate directly away. Because the refracting properties of the sun are such that waves emanating from any local region are divergent almost everywhere, there is no substantial coherent interference to mask the excitation process. The peaks in the power spectrum arise simply as a result of observational filtering. As discussed by Pawan Kumar at this symposium, the positions of the peaks provide an estimate of the depth at which the excitation occurs. His most recent calibrations yield a value of about 140 km beneath the photosphere, which now agree with earlier local studies by Goode *et al.* (1992).

Another very interesting calibration can be carried out by matching the variation with frequency of the amplitudes of the supercritical peaks. It determines the spectral index of the turbulent energy, which Kumar (1994) has shown agrees with the Kolmogorov law. This is one of the few cases in nature in which evidence, albeit indirect, for the existence of inertial-range turbulent cascade has been found.

Returning to the linewidths of the trapped p modes, it is interesting to observe that there is not always obviously a local minimum near 3 mHz. Figure 4 shows three examples, one from Libbrecht and Woodard (1991), one from BiSON and the other from VIRGO. Libbrecht and Woodard's data

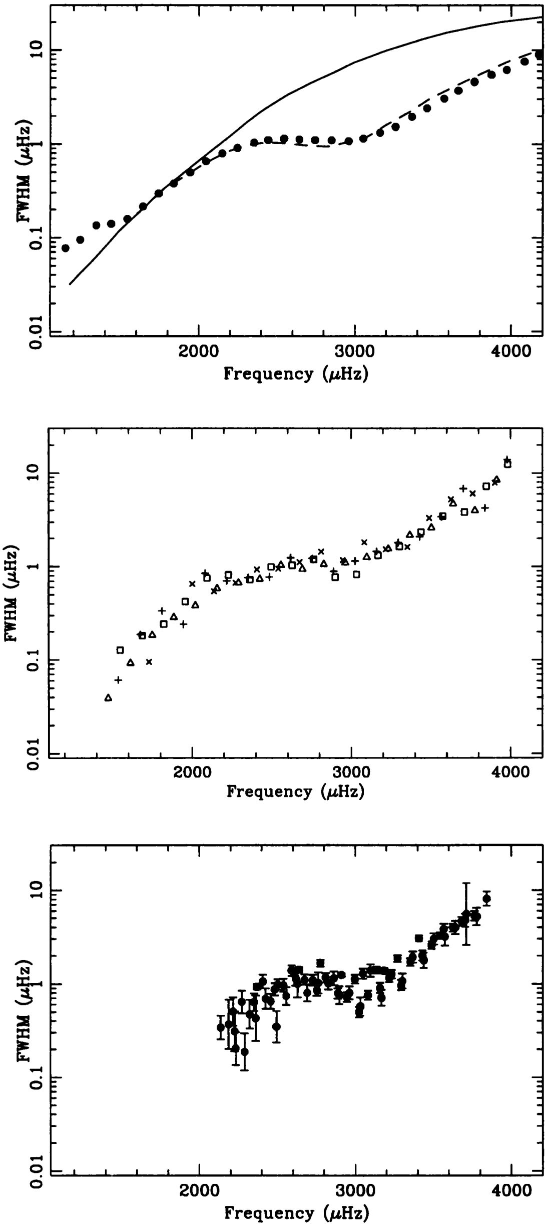

Figure 4. Full width at half maximum of p-mode power, plotted against cyclic frequency ν: top panel from Libbrecht and Woodard (1991), middle panel from BiSON (Elsworth, these proceedings), bottom panel from VIRGO (LOI and SPM) (Fröhlich, these proceedings). The filled circles in the top panel represent data from low-l modes (l probably between 5 and 60) in 1989 scaled by the modal inertiae to $l = 0$. The continuous curve is Murray's (1993) estimate of the contribution to the damping rate from scattering. The dashed curve was obtained from the continuous curve by Murray by artificially decreasing the assumed turbulent eddy correlation length in the hydrogen ionization zone by about 30 per cent.

exhibit a minimum near 3 mHz, whereas in the BiSON data there may be only a point of inflexion. No discussion of the apparent disparity has yet been put forward. Is it a result of the different procedures for analysing data, or is it an indication of a temporal variation related, perhaps, to magnetic activity? In Libbrecht and Woodard's (1991) data one can see a slight hint of a change with time. Such change might be the answer to why in the BiSON and VIRGO data the scatter appears to be locally maximal in the vicinity of 3 mHz.

It is interesting to note that the recent LOI data from VIRGO show slight evidence of enhanced excitation at active latitudes (Fröhlich *et al.*, these proceedings). This suggests that in the convection zone there is perhaps an extra component of motion to excite the waves which is associated with the presence of intensive active-region-scale concentrations of magnetic field, some evidence for which has already been provided by Haber *et al.* (1988). That result is apparently contrary to the findings of the BiSON group (Elsworth *et al.*, 1993), however, who find a 35 per cent augmentation of the power in low-degree modes near solar minimum. One is reminded of the similar disparity in the total solar radiance, which is decreased locally by the presence of active regions yet is a global maximum at the epoch in the solar cycle corresponding to maximum activity (e.g. Fröhlich, 1994). That too is yet to be understood (cf. Balmforth *et al.*, 1996).

A final thought on linewidths is provoked by the interesting autocorrelation analysis of GOLF data reported at this meeting by Gérard Grec. It is commonly assumed that linewidth is a measure of mode lifetime, by which one usually means the characteristic time over which a mode oscillates before its phase is destroyed by the random forcing by the turbulence. If that were actually the case, then the autocorrelation should decay on this timescale too, at least for small temporal displacements τ of the signal. At large values of τ one expects the envelope of the autocorrelation to tend to a constant, the major contribution coming from components of the signal excited at epochs that differ by approximately τ. But Gérard Grec reports that the observed value of that constant significantly exceeds expectation, which is evidence that there is a component of the signal with longer-term coherence. How can that be reconciled with the linewidth data?

One possibility is that the structure of the acoustical cavity is varying with time, perhaps as a result of effective distortion of the upper boundary of the cavity by the magnetic field, either directly, or by sound-speed or convective velocity variations that are modulated by the magnetic field. To be sure, we know that magnetic activity changes oscillation frequencies (Woodard *et al.*, 1991; Bachmann and Brown, 1993). This would cause a deviation of phase from that of a purely sinusoidal oscillator, thereby contributing to the spectral line width, but not necessarily destroying totally the autocorrelation. The result reported by Gérard is therefore very exciting, for, if this suggested explanation is correct, it would indicated that genuine damping is actually less than had previously been suspected, opening up the possibility, with suitable analysis (cf. Chang, Gough and Sekii, 1995), of measuring oscillation frequencies more accurately, and thereby improving our helioseismological inferences about the structure and kinematics of the solar interior.

6. Angular velocity

The reviews by Sasha Kosovichev, Takashi Sekii and Michael Thompson have shown quite clearly that our early inferences of the variation of angular velocity Ω in the sun are more-or-less correct. Broadly speaking, it appears that the latitudinal variation of Ω observed at the surface of the sun is maintained essentially to the base of the convection zone, and then there is a sharp transition to uniform rotation in the radiative interior. What happens deep in the core is not yet settled.

As Spiegel and Zahn (1992) have discussed, Ekman circulation currents are set up beneath the base of the convection zone in the rotational shear layer, which they call the tachocline. These currents exchange material with the convection zone. I shall return to this point later. But I remark now that evidently, if the tachocline extends deeply enough for lithium and beryllium to be destroyed by nuclear reactions, or if it has done so in the past, the tachocline must have an important impact on the issue of the subcosmic photospheric solar abundances of these light elements.

Why the convection zone rotates as it does is a matter that is not understood, but which is presumably intimately associated with the anisotropy of the turbulent motion imposed by Coriolis forces, and the meridional flow it induces. The characteristic timescale for adjustment to external influences is expected to be of the order of only a year or so, which implies that the convection zone determines its own rotational structure. The adjustment of the radiative interior takes much longer. Why should it be rotating uniformly, at least in the outer layers?

The suggestion by Spiegel and Zahn (1992) was that instability of the shear in the stably stratified boundary layer induces small-scale two-dimensional turbulence in horizontal surfaces: the turbulence acts on the large-scale flow like a viscosity (in horizontal surfaces), and thereby leads to rigid rotation at the base of the tachocline. The equilibrium rotation of the interior is therefore also rigid, provided Eddington-Sweet circulation is ineffectual. This view is not unnatural, but it should be remarked that, as Michael McIntyre (personal communication) has emphasized, two-dimensional turbulence in the Earth's atmosphere tends to render potential vorticity rather than angular velocity uniform, at least away from the polar regions. One cannot be sure, therefore, that uniform rotation should be the natural state of the radiative interior under these conditions, unless the interior is pervaded by a magnetic field.

It is also necessary to explain how the equilibrium state of the rotation of the radiative interior is attained. In his review of the dynamics of stellar rotation at this symposium, Jean-Paul Zahn put forward the suggestion that the uniformity of rotation is established by angular-momentum transport by gravity waves generated in the lower boundary layer of the convection zone. This idea is very interesting to me, partly because, to the best of my knowledge, angular-momentum transport by gravity waves in the sun was first discussed at the IAU Colloquium organized in 1976 by Philippe Delache (and Roger Bonnet). However, the conclusion then was that the process was probably not important for the global distribution of angular momentum (Gough, 1977). The reason was that waves that resonate with the largest convective eddies at the base of the convection zone dissipate before they penetrate very deeply into the radiative interior, and waves that resonate with smaller eddies arising from a turbulent cascade and which are able to propagate more deeply have insufficient amplitude to transport an interesting amount of angular momentum. Furthermore, the higher-frequency grave low-degree modes, which might be generated near the top of the convection zone, hardly dissipate at all. Only if the core of the sun were convective would there be dissipation enough to communicate a substantial stress.

Several studies have been carried out since, with broadly similar results, but the result of Jean-Paul Zahn is different. It appears that Jean-Paul's estimate of normal wave dissipation is substantially lower than mine, but it is not clear to either of us why that is so. I, for one, will certainly be interested in any further illucidation of the process.

7. Magnetic fields

Measurement of the characteristics of solar oscillations is reaching a level of precision sufficient to enable us to detect the influence of magnetic fields. That magnetically active regions absorb or scatter acoustic waves in the sun has been known for a decade (e.g. Braun *et al.*, 1987), and there is some evidence also for emission by major flares (Haber *et al.*, 1988). Magnetic activity on the surface of the sun also augments p-mode frequencies (Woodard *et al.*, 1991; Bachmann and Brown 1993), reducing the acoustic volume of the cavity either by effectively lowering the level of the upper turning point, thereby diminishing the physical size of the cavity, or by directly augmenting the wave propagation speed. The relation between the oscillation frequency perturbations and the intensity of magnetic activity appears to be essentially independent of the timescale of variation of the activity, which is consistent with the idea that the frequency perturbation is the direct effect of the presence of the magnetic field, B, and is influenced only weakly by the large-scale thermal adjustments associated with the solar-cycle radiance variation. This is consistent with the conclusions of Gough and Thompson (1988b), Goldreich *et al.* (1991) and Balmforth *et al.* (1996), who found that potential magnetically induced modifications to the thermal stratification of the upper boundary layer of the convection zone of a magnitude compatible with the observed radiance variation is insufficient, and of the wrong sign, to account for the oscillation frequency shifts.

There have also been hints of deviations from the standard theoretical models of the hydrostatic stratification of the sun in the vicinity of the base of the convection zone, which Dziembowski and Goode (1989) have interpreted as being evidence for a toroidal B. And at this symposium, Haber *et al.*, using local spectral ring analysis, and Ryutova and Scherrer, using time-distance analysis, provide evidence for anisotropic wave propagation which they interpret as being a possible signature of B. These results are very encouraging for those with an interest in the dynamics of magnetically active regions and the maintanence of the solar cycle. But one must ***B***eware! Anisotropic wave propagation can result also from horizontal thermal inhomogeneity. Such inhomogeniety in the outer layers of the sun can distort the shapes of spectral rings and impart a directional dependence on time-distance analysis. To separate it from B requires careful piecing together of the results of analyses over a network of local areas. Likewise, the signature of the putative toroidal B inferred by Dziembowski and Goode might actually have been produced by a latitudinal variation in the structure of the tachocline, and not by a magnetic field at all.

It is only where the magnetic stresses are comparable (within no more than just a few powers of ten) with the gas pressure that there is some hope of a direct seismological detection. Otherwise one must resort to indirect methods. In my opinion it is therefore unlikely that a direct measurement of the large-scale field that pervades the radiative interior of the sun will be made, at least in my lifetime. My reason is simply that my estimate of that field (Gough, 1990b), which presumably has decayed ohmically from an initial poloidal field of characteristic intensity B_0 of about 1T, provides a stress of at most 10^{-6} of the gas pressure in the radiative core and envelope. But of course my estimate could be widely wrong. It is therefore incumbent upon us to ask the question (using Jamie Matthews' licence to talk in terms of H rather than B):

What is the value of H_0?

That is a question of interest to a community much wider than the participants of this symposium. It is evident that any value even remotely close to my estimate is sufficient to transport angular momentum from the core to the convection zone in less than the age of the sun – actually 10^{-9} T is sufficient – and hence maintain the radiative envelope in a state of almost uniform rotation provided that there is no perturbation on a significantly shorter timescale. One wonders whether it is no more than an interesting curiosity that a field intensity of order 1T would cause a response of the sun to a torsional perturbation on a timescale comparable with the characteristic period of the solar cycle.

8. Telechronohelioseismology

One of the highlights of this symposium was the report by Sasha Kosovichev on his work with Tom Duvall on measuring the flows in the upper layers of the convection zone. The earliest results of time-distance techniques essentially reproduced what we had learned already from analysing the presumed eigenfrequencies of free oscillation modes. But here we have witnessed a great leap forward in the diagnosis of lateral inhomogeneity. This, no doubt, is but a first jump beyond what we have achieved by our older methods, and I am quite sure that there is a great deal more kilometrage yet to be gained from the technique. Indeed, the technique is likely to provide the most powerful tool available for studying the dominant energy-containing eddies in the upper reaches of the convection zone.

In principle, telechronoseismology can be used for diagnosing any structural or kinematical aspect of the sun. Whether it is more prudent to use this technique or, for example, a normal-mode technique will depend partly on the relative difficulty in carrying out the observations and the data analysis, and partly on the extent to which the results are contaminated by noise. My guess is that, at least at first, the technique comes into its own in diagnosing near-surface inhomogeneities. It could be particularly powerful when those inhomogeneities are not immediately beneath part of the visible hemisphere. For example, one can construct an acoustical lens, by cross-correlating signals with suitable time lags from diametrically opposite portions of circular annuli on the visible surface of the sun and averaging around the annuli, to focus on an antipodal region of the solar surface. In this way one should expect to detect active regions forming on the far side of the sun, and thereby have a truly global image of at least the sun's outer layers. Coupled with observations discussed by Judit Pap and Anne Vigouroux at this symposium of changes in radiance, apparent solar diameter and other surface properties, it should help us gain a deeper understanding of the overall mechanism of solar variability. An obvious immediate potential use to others might be as an early warning for space storms. Of perhaps less immediately obvious importance to us might be, especially if g modes are not detected, its use for monitoring the emergence of major perturbations to the surface of the sun's acoustic cavity. It may then be possible to correct for the frequency modulation of the p modes of low and intermediate degree, in order that we might improve the frequency estimates and thereby measure more accurately the structure deep in the solar interior.

9. A few remarks on extrasolar observations

I shall not dwell long on asteroseismology or planetary seismology, because both disciplines are in their infancy and the current status of them has been well discussed by the reviewers. Both disciplines are healthy, which is evident from the optimism of the speakers, even though resources may not be as copious as one would like. Indeed, from Søren Frandsen we learned that in two years from now the observational situation will have improved to the extent that asteroseismology will have become a real subject.

The sun has been an excellent playground on which to have learned the basic techniques of seismological inversion. We are thus well equipped to venture into the wider arena, where oscillation amplitudes are sometimes greater or where deviations of the basic structure of the star from the relatively simple spherically symmetrical state are large. Wojtek Dziembowski announced that the nonlinearity experienced in large-amplitude oscillators is a disaster. On this point I cannot agree, firstly because our younger colleagues need challenging obstacles against which to pit their wits, and secondly because nonlinear dynamical behaviour is always richer with information than is its linear counterpart. Of course, in order to extract that information, whether it be from linear or nonlinear oscillations, we must necessarily be able to identify the modes of oscillation that are being observed, as Nathalie Audard, Wojtek Dziembowski and Jaymie Matthews, amongst others, have emphasized. Large-amplitude deviations from spherical symmetry also produce interestingly richer behaviour. An example is rotational splitting of g modes in stars whose angular velocity is comparable with the frequency of oscillation. Another example is the existence of only prograde modes in some rotating stars, which Søren Frandsen reported. I have always suspected that that might be a result of critical-layer absorption of the retrograde g modes, but I've never worked hard enough on the problem to demonstrate whether or not the idea is plausible. A third example is the rapidly oscillating Ap stars, which appear to have large magnetic spots in which the stratification is presumably quite different from that in the quiet regions. As Jaymie Matthews explained, the magnetic field may have a major influence on the structure of the core too, as well as on the dynamics of the oscillations. The situation is apparently quite rich in possibilities, and interesting inferences are bound to emerge once the various influences on the pulsation frequencies have been unravelled. Finally, amongst the stars, are the white dwarfs. As Gérard Vauclair has explained, a period-mass-luminosity relationship enables one to determine their distances, analogously to the Cepheids. Measurements of rotational splitting of PG 1159 and pulsating DB dwarfs are reaching a level of detail to permit differential rotation in those stars, when it is present, to be discernible. All these and many more new advances will supply asteroseismologists with plenty to think about in the years ahead.

Finally, I come to Benoît Mosser's discussion of the seismology of giant planets. I draw attention to this subject partly because there is some interesting physics to be learned from its study, and partly because the seismic observations were instigated by Philippe Delache (Schmider *et al.,* 1991). The greatest interest at present is in Jupiter, because, unlike Saturn, there is good positive observational evidence that it is pulsating. There seems to be less known about the structure of Jupiter than, for example, the structure of the sun. To be more precise, I suppose I should really say that we are less confident in the theoretical models of Jupiter, and that the physics of those models has been less thoroughly studied. Observationally, we known the mass and the radius, of course, as we do of the sun, together with some moments of inertia deduced from multipole moments of the gravitational potential. Theory suggests there has been chemical differentiation leading to a rocky core, possibly surrounded by a layer of ice; and acoustically midway between the centre and the surface there is probably a plasma phase transition between metallic and molecular hydrogen, which may or may not occur in a discontinuity. Moreover, substantial gravitational settling of helium appears to have taken place, judging from the low abundance of helium observed in the surface. How extensive that settling is is uncertain. So also is the extent of the settling of heavier elements, so the very existence of the rock and ice cores, with distinct boundaries, is a matter of debate.

If the cores do exist with sharp boundaries, and also if the boundary determined by the plasma phase transition is abrupt, then there must be oscillatory signatures imparted on the oscillation eigenfrequencies, whose frequencies (with respect to frequency) and amplitudes depend respectively on the acoustical radii of those boundaries and on the magnitudes of the discontinuties in the equation of state across them (Provost *et al.*, 1993; Gough and Sekii, 1995). However, unravelling those signatures will require extremely accurate frequencies. The reason is twofold. First the plasma phase transition does not perturb the frequencies by very much. And second, the core boundaries are acoustically so close to the centre of the planet that very little of the oscillatory perturbations, perhaps less than a period, can fit into the frequency range, which is presumable bounded above by the critical cutoff frequency of the atmosphere. Consequently it will be very difficult to unravel the two periodicities, if indeed they are even present. However, the situation is mitigated by the fact that the magnitudes of the discontinuities in the core are quite large, leading to greater complexity in the oscillatory signatures, which carry more information per datum than sinusoids do. Of course if the transitions are not sharp, the signatures will be smoothed out, and inference will be correspondingly more difficult.

Further observations are planned for the future, of both Jupiter and Saturn, and it is to be hoped that soon we shall have a seismic spectrum to study. I might point out that David Thomson (personal communication), whose claim that I mentioned earlier in connexion with g modes to have discovered waves in the solar wind produced by solar oscillations (Thomson *et al.*, 1995), says he now has evidence also of a Jovian signal. If that evidence is convincing, perhaps we shall have data earlier than we anticipated.

10. The golden path to happiness

The consolidation of the data from the well established ground-based networks BiSON and IRIS, and the rapidly accumulating data both from the ground-based GONG, TON and the other new networks and from the seismic instruments on SOHO, all of which have been reviewed in Session I of this symposium, must surely lead to an escalation of publications of a host of inferences. At first we shall commonly see inversions of the kind we have witnessed in the past, but no doubt improved by virtue of the increase in both quality and number of data and of new refinements in the techniques to analyse them. From those inversions will be raised new questions. And then it will often be the case that subsidiary methods of data analysis will need to be developed in order to answer them.

It is easy to ask questions if one adopts the following maxim:

Always overinterpret the data.

Indeed, it is a scientist's responsibility to do so, provided that it is not done irresponsibly. To discuss hints of phenomena at the threshold of detectability whets the appetite for improving the data and their analysis so that the threshold is pushed back and the phenomena are revealed (or not, as the case may be). So I conclude by indulging in the activity, as Philippe Delache was wont to do. One must be aware, however, that the indulgence is in speculation, not in deduction. Therefore its purpose is simply to provide discussion, not to make claims.

The motivation is to raise one's spirits in the face of the depression that some of us have suffered at the sight of Figure 1. A more optimistic view

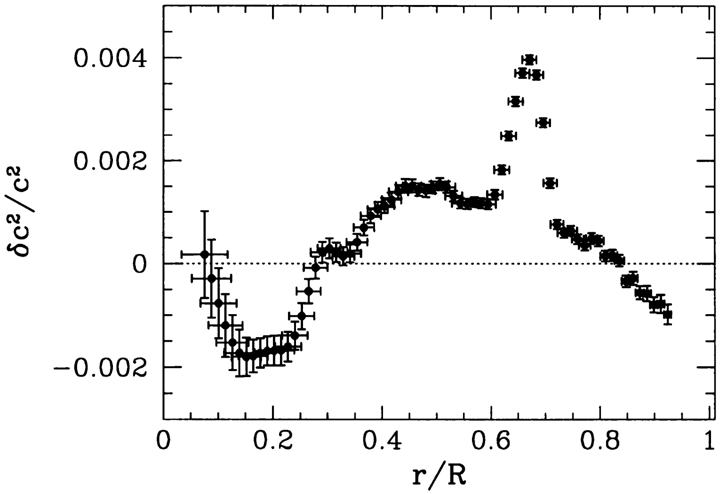

Figure 5. Relative differences $\delta c^2/c^2$ between the squares of the sound speed in the sun and in a theoretical reference model, using data from the SOI/MDI instrument on SOHO (from Kosovichev *et al.*, 1997). The meaning of the symbols is the same as in Figure 1. The reference model is the same as that used for the GONG structure inversions reported in these proceedings. It is a standard model in which some account is take of the effects of gravitational settling of helium and heavy elements.

is provided by Figure 5, which is a recent sound-speed inversion of MDI data (Kosovichev *et al.*, 1997). The structural inversions of GONG data are superficially similar (Anderson *et al.*, these proceedings). To be sure, the deviation of the sound speed c from that in the reference theoretical model is typically less than a part in a thousand; that value is exceeded in only about 5 per cent of the radius range, and then by no more than a factor of two or so. Therefore, one might argue, the theoretical solar model is very nearly correct, which is the message that Figure 1 was intended to convey.

Is that conclusion actually correct? The important point to notice about Figure 5, which differs qualitatively from the false view offered by Figure 1, is that the inversions actually indicate a very significant deviation from the reference model. Indeed, in places that deviation exceeds 20 standard deviations of the formal random errors. So clearly there is a sense in which either the standard reference model or the inferences from the data are very bad indeed. The errors must be explained. How do we go about it?

The boring way to try to account for the disparity between the sun and the reference model is to argue that minor adjustments need to be made to the theory, by tweaking the equation of state, the opacity and the nuclear reaction rates. With such small deviations to reproduce, one might not unreasonably think that that route might lead to the desired end. And

I am quite sure that in due course the modelling industry will carry out the necessary calculations. But it is much more interesting, and no doubt more realistic, to take a more global view of the situation.

Before proceeding it might be worth determining what the desired end is. After all, one needs to be able to tell whether or not one has arrived at one's destination. There are some, possibly lucky, people who would be content once Figure 5 really does look like Figure 1. They want merely to find a theoretical model that can account for the data. I recall that two decades ago this was a common attitude amongst those in search of resolving the solar neutrino problem: some of the experts in the field took the view that if a phenomenon was not 'needed' for reducing the neutrino flux its role should not be entertained when trying to 'explain' the workings of the sun. And it seems that that view has not yet completely disappeared. However, I presume the universal view at this symposium is different. The objective is to understand the sun, and, if two models that differ in structure both reproduce the observations that are available, new distinguishing observations must be sought. And, of course, one must continue to try to refute the surviving unique theory, once we find it.

Let us begin our scrutiny of Figure 5 with the greatest anomaly, situated immediately beneath the convection zone. I have already mentioned that the Ekman meridional circulation in the tachocline, discussed by Spiegel and Zahn (1992), exchanges material with the convection zone, and, if penetration into the radiative interior is sufficient, reduces the photospheric abundances of lithium and beryllium by nuclear transmutation. It must also reverse some of the gravitational settling of helium from the convection zone, which in the theoretical models is impeded only by diffusion. Replenishment of the relatively hydrogen-rich material of the convection zone would reduce the mean molecular mass in the tachocline, thereby increasing the sound speed. Indeed, measuring the sound-speed anomaly in Figure 5 is at present probably a more accurate way of measuring the extent of the tachocline than is fitting to the rotational splitting data a functional form for the angular-velocity transition. I must point out, however, that the observations indicate that the hydrogen has not been completely replenished, which is consistent with the helium settling rate in the tachocline being comparable with advection. The resulting composition gradient exerts a retarding negative buoyancy force on the Ekman flow, which has not yet been taken into account in the dynamical studies. Nevertheless, present indications are that substantial lithium depletion may result, although more refined dynamical and seismological studies will be required before we can be sure.

It should also be pointed out that Stokes drift associated with the gravity waves generated near the base of the convection zone can also transport material. I discussed a simple estimate of the effect some years ago (Gough, 1988), and found it probably to be unimportant on a global scale. But in the light of the more complicated coherence in the transport discussed by Knobloch and Merryfield (1992), and the discussion at this symposium by Jean-Paul Zahn, the matter needs to be reexamined.

An important feature of the anomaly immediately beneath the base of the convection zone is its thinness. This suggests that the cause is not thermal, such as one might find with a simple modification (e.g. one whose sign does not change) to opacity, for opacity changes tend to produce a broad response (e.g. Christensen-Dalsgaard, 1996). Of course, one can always contrive an artificial opacity perturbation of a form that would reproduce the anomaly, and such a perturbation should not be ignored. However, because model builders have on the whole followed a path determined by physics, even though they have been led largely by the observations, one expects that the final route to reality is unlikely to involve contrivance. A localized change to the $p - \rho - T$ relation determined by the equation of state can produce a more localized sound-speed response, and my suggestion is that that has been brought about via a change in the helium abundance. I should point out, however, that if the sun were to have lost sufficient mass during its main-sequence evolution, there could have been a similar outcome: the material immediately beneath the convection zone would previously have been at greater depths where helium-abundance augmentation by gravitational settling was slower (and lithium and beryllium destruction more rapid). Indeed, an appropriate amount of mass loss reduces the disparity between the theoretical model and the sun substanially (Gough *et al.*, 1996), though probably not entirely. Therefore there are at least two candidate mechanisms for the proposed abundance anomaly, and both must be investigated. One must bear in mind, however, that the candidates do not have the same footing. There is little evidence for the sun having lost a substantial amount of mass during its main-sequence evolution phase, whereas the dynamical evidence for the existence of a tachocline circulation is overwhelming. What is uncertain on theoretical grounds, however, is how deeply that circulation should penetrate. It is encouraging to observe that the apparent thickness of the tachocline – roughly 5 per cent of the solar radius if indeed essentially the whole of the anomaly is due to it – is similar to the value suggested by Spiegel and Zahn (1992) prior to its measurement.

I conclude by drawing attention to the second most prominent anomaly, namely that in the energy-generated core. Hence I really show my prejudice by finding that anomaly to be more interesting, and hence more prominent in my mind, than the discrepancy of similar magnitude in the radiative envelope beneath the tachocline: $0.3 \leq r/R_\odot \leq 0.65$. But that interest was shared with Philippe Delache, as was the dynamical result I am about to remind you of, which is one of the reasons why Philippe worked so hard looking for g modes. So my ending is not unfitting.

According to Figure 5, the sound speed in the central region of the solar core appears to be greater than that in the reference model, and that in the surrounding shell is lower. Of course, this anomaly too would no doubt be accommodated by an appropriately designed modification to the opacity and the nuclear reaction rates, and perhaps such modifications do play a role. But as in the tachocline, there are dynamical considerations which might lead one to suspect that the matter is not so straightforward.

The cores of solar models are linearly unstable to grave gravity modes (e.g. Christensen-Dalsgaard, Dilke and Gough, 1974). Although the property has been known for more than two decades, it is largely ignored by the solar modellers. If the sun has suffered such instability too, then the nonlinear development of the instability must have influenced the structure of the core in a manner that is not incorporated in the standard solar models. The important issue with regard to Figure 5 is whether or not the magnitude of the effect is great enough to have a significant bearing on the mean stratification. There is, of course, also the more general issue of understanding the dynamical development of the instability. There have been a variety of studies, with a variety of suggested outcomes, including transition to direct convection and nonlinear limiting of the oscillations at an amplitude great enough to play a significant role in the nuclear evolution of the core, and also nonlinear limiting, via a triad interaction with a pair of stable g modes, at a structurally insignificant amplitude. However, all of the calculations are too idealized for one to carry over the results to the real sun. The issue is still open.

The issue is also currently topical, as a result of a recent provocative investigation by Cumming and Haxton (1996). The work was motivated by the desire to address the apparently discordant solar neutrino measurements. One can broadly rationalize the results of the ^{37}Cl detection, the Kamiokande experiment and the SAGE/GALLEX ^{71}Ga experiments by asserting that the pp flux is apparently equal to the theoretical prediction, that the ^{8}B flux is about 40 per cent of that of current solar models, and that the ^{7}Be flux is essentially nonexistent. Studies based on a broad range of spherically symmetrical solar models, in all of which the reaction chains are presumed to be very nearly in balance, reveal no adjustment that can reproduce this result. Therefore, it has been concluded, neutrino transitions must take place irrespective of astrophysical considerations. Indeed, the conclusion now supports a small industry studying neutrino mass ratios and mixing angles, the possible influence of helicity flipping and other exotic phenomena. That is very healthy, for one must never become too complacent with the standard models, be they of electroweak particle interactions or of the sun. However, the strengths and weaknesses of the arguments against an astrophysical solution to the problem must not be forgotten. In particular, if one admits that the balance between the chains in the nuclear reactions can be broken, the present case for the necessity of neutrino transitions collapses. Neutrinos might still be massless.

Dynamical instability is precisely the mechanism that unbalances the nuclear reactions, by imparting a time dependence that is more rapid than nuclear equilibration, or by advecting intermediate products of the reactions away from their sites of creation. Indeed, the original discussion of the instability was motivated by this very fact. Now Cumming and Haxton have produced a new twist. They point out that if the instability gives way to direct convection with a not implausible asymmetry in the geometry of the flow, then the results of the different neutrino measurements might be reconciled. The toy model they discuss, if taken literally, is obviously in conflict with the helioseismic inferences, and no doubt there will soon be a paper to point that out.* However, there may be variants of the model that represent the actual conditions in the solar core more faithfully than the standard models do, even if they don't actually resolve entirely all the issues concerned with the neutrino observations. Indeed, the reversal in the gradient of $\delta c^2/c^2$ near $r/R = 0.3$, where the ^{3}He abundance is greatest, is indirect evidence for an advecting shell. Care must be exercised when using seismic comparisons with spherically symmetrical models when making deductions about reality. In this context it is interesting to note that the initial nonlinear development of the direct ^{3}He-driven convective mode, if it were to be unstable, does not influence the sound speed, so it may be necessary to look for the motion more carefully than one might have suspected. Indeed, actuality may be significantly different from the standard models of both the sun and electroweak interactions, even though a change in only one of them may be required to erase the disagreement between theory and neutrino-flux observations.

I am sure that it will not require enormous ingenuity to find some minimal modification to the standard solar model to eliminate the disparity plotted in Figure 5. Our real challenge will be to distinguish between the

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*After the presentation, Jørgen Christensen-Dalsgaard told me that such a paper is already in preparation (Bahcall *et al.*, 1997). Notwithstanding the enormously statistically significant disparity between the sun and the current standard solar models (illustrated in Figure 5), it is argued that the remarkable agreement between standard 'predictions' and helioseismological observations essentially rules out solar models with temperature or mean molecular mass profiles that differ significantly from the standard profiles. It is also pointed out that standard models 'predict' the measured properties of the sun more accurately than is required for applications involving solar neutrinos. No case against spherical asymmetry and associated nuclear imbalance is made.

many ways of doing so. That was the challenge that Philippe Delache always had in mind.

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