THE FUTURE OF HELIOSEISMOLOGY

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ABSTRACT

Abstract At the request of the organiser, I shall describe what I perceive to be some of the inferences that will be made from the helioseismic data that will be obtained from SOHO and GONG. To that end it has been necessary to procure and dust off a crystal ball, and to bring to bear what threads of evidence there are to stimulate the required brand of clairvoyance. These are, of course, the course of past inferences and the generalization, refinement and testing of data-processing techniques and inversion procedures, using both real and artificial data, that have taken and are taking place. They provide the basis from which I must extrapolate, guided by knowledge of the skills of the scientists that will carry out the work, and the scientific issues by which those scientists are motivated.

Most crucial have been the latest trigonometric points supplied during the meeting. Being a congenital optimist, I have no doubt that, broadly speaking, the predictions that I shall make will come to pass. My only mistakes will be by omission, due either to lack of space or to my failure to foresee new advances that will be made by the new expanding generation of young helioseismologists with minds more agile than my own. Therefore, the further from actuality the overall impression left by my story turns out to be, the happier shall I be.

1. SOME IMMEDIATE ACHIEVEMENTS OF HELIOSEISMOLOGY

It is always a difficult task to predict the future with confidence. However, it has been abundantly clear to me during this meeting that there is great enthusiasm for obtaining the following basic helioseismic data:

(i) more-accurate p-mode frequencies,
(ii) more accurate p-mode frequencies, particularly frequencies of low-order modes of low degree,
(iii) the frequencies of at least a few g modes.

Many of us have already devoted a great deal of effort towards those objectives, and even more of us are committed to devoting yet more effort. Therefore I can safely predict that in the not too distant future, indeed, I hope, from the data from the SOHO helioseismic instruments and from GONG and the other ground-based networks, these three objectives will be achieved. It is self-evident that the new superior projects will achieve goals (i) and (ii). George Isaak has assured me that modes with velocity amplitudes as low as 1 mm s\(^{-1}\) will be detected eventually, and there are amplitude estimates (Fossat et al., 1996; Gough, 1996) of the gravest g modes that (barely) exceed that threshold. Therefore, the third objective will be achieved too. What is not so evident, however, is when it will be achieved. Only yesterday I wagered with George Isaak (for two bottles of premier-grand-cru claret) that it will be achieved, by the observation of some direct signature, before the year 2000.

Of course, accurate frequencies by themselves are of little interest. What is of interest is what we can deduce from them. And, what comes first to the minds of most of us is the determination of the internal structure and rotation of the sun. It is almost self-evident that our knowledge of these would be improved by an increase in the accuracy of the frequencies. But why should we wish to observe more modes, and why, in particular, g modes? The answer is simply that the low-frequency modes sample the solar interior differently from the relatively high-frequency modes which we have measured already, and that consequently less subtle combinations of data in the augmented set are required, especially when we wish to probe the very central regions of the sun. Less severe cancellation is demanded in the inversions, and consequently the corruption of the inferences by data errors is less serious.

This property is illustrated in Figure 1, which shows some of the first structure inversions to have been carried out. The inversions are of eigenfrequencies of an extremely simple model of the sun, for the density difference from a similar reference model. Inversions by two methods are shown: the dots represent optimally localized averages, and the dashed curves were obtained from a least-squares fitting procedure. The continuous curves are the actual difference between the two models. In both cases only 9 modes were used, 10 with \(l = 1\) and 10 with \(l = 2\). In Figure 1a the modes were the 10 lowest-order p modes of each degree. In Figure 1b the p modes of orders 6-10 were replaced by the 5 gravest g modes of each degree: \(g_1 - g_5 (l = 1)\) and \(f, g_1 - g_4 (l = 2)\). The improvement


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provided by the g modes in the ability to infer the structure of the proxy sun is dramatic. A similar improvement is gained also for inversions for angular velocity.

Of course, I could have used a more modern illustration, using a more representative number of modes by today’s standards. But I choose this one because of its appropriate historical interest. It was used in support of the proposed ESA mission DISCO, out of which the SOHO mission arose. Moreover, it makes my point quite adequately: that a helioseismic space mission will determine at least the spherically symmetric component of the structure of the sun throughout, including the energy-generating core. That was certainly an important aspect of the potential future of helioseismology in 1983; it is an equally important aspect of the likely future of helioseismology today.

Before moving on, I should point out another lesson to be learnt from Figure 1. When the inversions do not reproduce the correct function, as in Figure 1a, they yield different results. We found also that whenever they yielded essentially the same result, that result was more-or-less correct, as is the case in Figure 1b. On the whole, subsequent studies have confirmed this finding. And it thereby extends the principle of redundancy enunciated by Jack Harvey at the beginning of this meeting: not only do we need redundant data sets, we need also redundant analyses of them. I must warn, however, that, as is the case also with observations, disagreement guarantees that at least one of the results is wrong, whereas agreement alone guarantees nothing.

2. SOME ADDITIONAL IMPORTANT FUTURE ACHIEVEMENTS OF HELIOSEISMOLOGY

An accurate determination of the structure of the sun per se is not of profound importance. Who really cares exactly what the density of the sun is at the centre, or anywhere else for that matter? It is not a knowledge of the values of density or pressure, or the angular velocity (or anything else) that is of real interest, but the implications of those values, particularly the implications for physics and astronomy. The physics that might interest us is the physics most pertinent to the gross behaviour of the object under study, namely the sun, and the broader realm of physics upon which it impinges. Alternatively, one might become fascinated in the physics of the investigation itself: the physics of the oscillations, normally regarded as a passive diagnostic so far as the gross behaviour of the sun is concerned, or the physics pertaining to the processes involved in the observation of those oscillations. I shall list some specific examples below. So far as astronomy is concerned, I am moved to draw your attention to the now-hackneyed concept of the sun as a star. It is of extreme importance to check the theory of stellar evolution as carefully as we possibly can, so that we are less uncertain (or, dare I say, more nearly certain) of the properties of other stars. Then we certainly must do our utmost to avoid errors in the presentation of our results. The recent silly announcement, attributed, perhaps falsely, to some analysts of Cepheid variables using data from the Hubble Space Telescope, that the Universe is younger than the oldest stars, for example, does little to enhance the credibility of its proponents. A reduction in our credibility would cause our findings to be underrated and underused, and in consequence would lessen the rate of progress of science.

Let us now address some of the issues to which we shall contribute. I list the subjects on the left, and on the right the properties of the sun that they concern.

Figure 1. Density inversions of 20 low-degree modes. Both the proxy sun and the reference models are piecewise polytropic, having polytropic index $\mu = 3$ to represent the radiative interior, $r < r_c$, and $\mu = 5/3$ in the convection zone, $r_c < r < R$, where $r$ is a radial coordinate and $R$ is the radius of the photosphere. Both models have the solar mass and radius; they differ only by the value of $r_c$. The adiabatic exponent $\gamma$ was taken to be 5/3 throughout. Dots represent optimally localized averages, and the dashed curves are spectral expansions whose coefficients are determined by least-squares fitting of the frequencies, regularized by truncating a singular-value decomposition of a matrix relating the parameters in the representation of relative density difference to the frequency differences. The continuous curves are the actual relative density difference, $\delta \rho/\rho \equiv 2(p_\rho - p_\rho)/\{p_\rho + p_\rho\}$, where $p_\rho$ and $p_\rho$ are the densities of the proxy and reference models respectively. In (a) is illustrated inversions of modes $p_{11} - p_{11}(l = 1, 2)$; in (b) of modes $g_{52} - p_{52}(l = 1)$ and $g_{42} - p_{42}(l = 2)$ (after Gough, 1985; cf. Cooper, 1980).
Perhaps the issue of greatest general scientific interest in the whole of solar physics, and therefore one which we must regard very seriously indeed, is the neutrino problem: that the observed values of $L_N$ are less than the values predicted. I used to wonder whether or not it might be a problem in particle physics, but now there is no doubt in my mind that it is. There are now so many particle physicists who play with neutrino transitions, usually, though not always, within the framework of MSW (Mikheyev, Smirnov and Wolfenstein) theory, that even if the resolution of the problem eventually turns out to be purely astrophysical, with no modification to the standard electroweak theory with massless neutrinos, the impact on particle physicists will already have been significant. But whatever the resolution, it is without doubt incumbent upon us to provide the most detailed and accurate information about the conditions under which solar neutrinos are produced, in order to assist in the interpretation of the data from the next generation of neutrino detectors.

Our most obvious contribution to theories of gravity in general, and the theory of General Relativity in particular, is via the multipole moments $J_2, J_4, \ldots$, of the sun's external gravitational potential. Seismology is by far the most accurate way of measuring $J_2$, and, indeed, we already have a quite reliable value. The greatest uncertainty arises from our uncertainty in the angular velocity $\Omega$ in the inner regions of the radiative envelope (outside the core), where the oblateness kernel is large. Careful analysis of future low-degree data from GONG and the instruments on SOHO to determine the rotational splitting will, I am sure, when combined with the high-degree data, provide $J_2$ to a precision that is well beyond that required for the radar-ranging observations that establish the precession of circumsolar orbits. Indeed, the range of plausible values of $J_2$ that we entertain at present (subject to what most people would regard as quite credible extrapolations of $\Omega$ into the regions of the sun in which $\Omega$ has not actually been measured) leads to uncertainties in the calibrations of theories of gravity that are as small as those resulting from uncertainties in the current orbit analyses. The precision of both $J_2$ and the knowledge of planetary orbits will improve with time.

Knowledge of the protosolar helium abundance $Y_0$ establishes a bound on the primordial helium abundance of relevance to cosmology—the value after the first few minutes of the Big Bang when universal nuclear reactions had ceased. We shall make substantial advances in establishing the value of that quantity once we have determined more precisely the hydrostatic structure of the sun. The first direct step in this endeavour will be a more reliable determination of the helium abundance $Y$ in the convection zone. That is made possible from a seismic measurement of the variation of the adiabatic exponent $\gamma = \left(\partial \ln p/\partial \ln \rho\right)_N$ throughout the HeII ionization zone.

I recall more than ten years ago giving a seminar on this subject at the Institute of Astronomy in Cambridge; I predicted that in ten years we would have the value to 1 per cent (i.e. within $\pm 0.01$). The Director (at that time) of the Institute countered with the opinion that it could never be done, because the measurements would never be accurate enough. We were both wrong; he because Libbrecht's data are certainly good enough to determine $\gamma$ to the required accuracy, and I because of an unanticipated hurdle that was encountered when converting $\gamma$ to $Y$: we do not know the equation of state (EqS) well enough, which brings me to the next item on the agenda. But before addressing that let me make it clear that I firmly believe that my error was merely one of timing: as my wife would readily confirm, I always take much longer than I predict to carry out any piece of research, and there is some evidence that others involved in this effort have made similar (though, I am sure, less extreme) errors of judgement. I have no doubt that we shall in due course arrive at a good value of $Y$, and with it a deeper understanding of the plasma physics required for calculating the equation of state, a bonus very well worth the unforeseen extra effort required for this project.

That there are inadequacies in the equation of state was revealed by inconsistencies in the inversions. Kosovichev, in his review, exhibited one of them: namely that, with the MHD equation of state that we have used to try to determine $Y$, $Y$ varies with depth. Yet the convection zone must be well mixed. The evidence may seem marginal, but there are other criteria which also point to inadequacies. Fortunately, adiabatic stratification, coupled with the knowledge that Reynolds stresses are small in and beneath the HeII ionization zone, poses a functional constraint on the relation between $\gamma$ and the sound speed which permits the testing of certain aspects of the equation of state. Some tests have been made already, notably by Däppen and his collaborators, and more are planned for the near future.

I next list opacity $\kappa$, and its pertinence to atomic physics (coupled inevitably with plasma physics), because it is intimately related, from a microphysical point of view, to the equation of state. It was the first helioseismic determination of the sound speed beneath the convection zone (Christensen-Dalsgaard et al., 1985) that led to the realization not merely that the opacity tables with which astrophysicists had been supplied at the time were inaccurate (an accusation from astrophysicists which was hardly new), but that it was possible to estimate the range of temperature and density within which there was substantial error and by how much the tables were in error. The eventual outcome was to draw Rogers and his collaborators at Livermore into the subject, which has been an invaluable bonus to us all. Rogers is contributing not just to opacity, but is also providing us with valuable insights into the physics of the equation of state, and valuable tables for us to construct our models too.
Before leaving this topic, I wish to add a little more to this story, in order to illustrate again how the sun will be used in the future. After careful comparison between the Livermore opacity calculations and those at Los Alamos that had been used to provide the astrophysicists’ tables, it was discovered that the error in the Los Alamos tables that the helioseismological sound-speed determination had revealed had resulted from an inadequate account of spin-orbit coupling in atomic and ionic radiative transitions. This was, perhaps, the first successful use of the solar interior as a laboratory to study microphysics. One of the goals to which some of us are striving is to establish the sun as an arena in which we can make many more inferences concerning the behaviour of matter under conditions that cannot be attained, in a controlled way, on earth. It is perhaps also worth noting, in passing, that the spin-orbit coupling error had an even greater influence on the opacity at lower temperatures. Those temperatures occur in adiabatically stratified regions of the solar convection zone where opacity is irrelevant to solar structure. But it is very relevant to hotter stars, and the correction of the error has led to the resolution, notably by Dziembowski and his collaborators, of some longstanding puzzles related to the pulsations of other stars, including classical Cepheids, β Cephei stars and the slowly pulsating B stars.

To conclude this part of my discussion, let us return to the issue of the protosolar helium abundance $Y_0$. Notwithstanding our deficient knowledge of the equation of state, it has been possible to estimate the value of the helium abundance $Y$ in the convection zone sufficiently well to realize that it is substantially lower than the value of $Y_0$ required by standard solar evolution theory to reproduce the correct present solar luminosity. This made the practitioners more acutely aware of what some other stellar astronomers had been telling us for some years: that the gravitational settling of heavy elements is not negligible. Perhaps some 10 per cent or so of the helium in the convection zone has settled into the radiative interior. The model calculations by Christensen-Dalsgaard and his colleagues (1993) in which some account of helium settling is taken certainly improves the agreement with the latest inversions, as the poster on the structural inversion of Tomczyk’s LOWL data illustrates. But it is not solely helium that settles. The settling of the other heavy elements (with abundances $X_i$) has some, though only slight, additional influence on the equation of state. However, there is a substantially greater influence on the opacity. Further settling calculations are called for, and I am reasonably confident that the Models Team will attack that problem in due course. Settling modifies how the sun, and other stars, evolve off the main sequence, and has significant consequences in the later stages of evolution. The depth $d_e$ of the convection zone, and the radii $\tau$ of convective cores particularly in stars somewhat more massive than the sun, both of which can be measured seismologically, provide convenient simple diagnostics of the structures of those stars, and can be used to test and calibrate the theory of stellar evolution.

Coupled with the settling of helium, $Y_1$, and of the other heavy elements, there is also the potential for material to be redistributed by macroscopic advection. So perhaps now I should address both that and some of the wider issues of fluid flow.

**3. ON SOLAR FLUID DYNAMICS**

There is a very wide range of fluid dynamical phenomena that are present in the sun, and I shall only touch on a few of them:

- **Convection:** large-scale flow: ‘giant cells’, small-scale turbulence, overshooting;
- **Rotation:** the form of $\Omega(r, \theta)$, its implications.

Many of the aspects of convection that are pertinent to the structure and overall dynamics of the sun have been nicely summarized by Toomre, and I shall add only little more. There is already a good chance of measuring large-scale convective flow seismologically, and Patrón et al. (1995) have made great strides leading to the possible detection of spiralling convective motion. I shall mention other work that is being carried out when I discuss local helioseismology. The small-scale turbulence in the upper boundary layer has a greater effect on the oscillations, and at the moment it is seen by most of us as an irritating source of uncertainty that folds into our inversions. Efforts are made to eliminate the effects of the small-scale turbulence from the data, and more effort will be made in the near future. Of course, once we are successful, we shall know as a byproduct the contaminating effects on the oscillations that we have eliminated, and that will no doubt in the future be used to study theories of convection.

Another role played by the small-scale turbulence in the convection zone is to excite, damp and scatter the solar oscillations. These processes shift the frequencies of the modes away from their ideal values, and distort the shapes of the lines in the oscillation power spectrum. For more accurate free-oscillation frequency determinations, these processes need to be understood and incorporated into the data analysis. It is my opinion that merely analysing acoustic power spectra will be superseded by more sophisticated analysis techniques that also take phase into account: not merely the phases of modes considered in isolation but also the relative phases of geometrically similar modes. There are several posters on various aspects of this subject at this meeting, and I predict that much more progress along these lines will be made in the coming years.

Overshooting at the base of the convection zone is also a subject of considerable fluid-dynamical interest, and its seismic signatures have been discussed and measured by several groups over the last three years. The signature sought is that of a lower-order discontinuity in the stratification than is present in standard models and which would naively be expected to occur in the presence of overshoot, if the corrugations imposed on the interface between the convecting fluid and the relatively quiescent
radiative envelope were small. I think it is probably fair to say that overshoot has not been unambiguously detected. What evidence there appears to be has been derived under the assumption that, notwithstanding any small-scale corrugations, the sun is essentially spherically symmetrical. However, there is slight evidence that the base of the convection zone deviates on a large scale from being spherical. If that were actually so, the overshooting analysis would need to be revised. Indeed, in order to unravel the two properties it will need to be more subtle. It is interesting to note that irregularities in the base of the convection zone would smooth the transition in the spherically averaged stratification, and thereby mask the signature of the overshooting. The new data from SOHO and GONG are bound to lead to a sharper resolution of this region. The overshoot region plays only a very minor role in determining the overall structure of the sun as a star. However, it is important for us to understand the phenomenon, because that might improve our ability to model the edge of convective cores in other stars in which overshoot plays a much greater part in determining the course of the evolution. I must not fail to mention the magnetic field:

\[ B, \]

which might not only upset our diagnostic inferences concerning overshoot, but also play an important dynamical role, particularly in a thin shellular region encompassing the boundary layer at the base of the convection zone. This is certainly a region of great interest to dynamo theorists.

Also of importance is the distribution of angular velocity \( \Omega \) throughout the sun's interior. Rotation is important not only for its intrinsic dynamical interest, but also by virtue of the disruption to pressure balance that it causes, thereby inducing meridional flow. Most studies estimate the meridional flow induced by rotation to be too weak to be of interest; but maybe they are wrong. I think it is unlikely that steady rotationally induced meridional flow in the radiative envelope or the core will be detected directly before I retire. On that issue I would be delighted to be proved wrong. Oscillatory circulation (\( \nu \)) might, however, be found, albeit indirectly. Another issue on which I doubt the seismic determinations of the sun's present angular velocity will shed light is why stars rotate so slowly. The angular momenta of stars are minuscule compared with what one estimates the material that comprises them to have been before the gas cloud from which they formed began to collapse. We do not really understand how protostars lose their angular momenta, though there is substantial evidence that it is to rotating accretion disks out of which, perhaps, planetary systems form. But in the case of the sun, that was all so long ago that I believe there is essentially now no recognizable legacy of those early times.

Before leaving this subject, I should point out that all pertinent stability calculations that have been carried out since 1972 have found the sun either to be unstable now, or to have been unstable in some epoch of its main-sequence evolution, to grave low-degree g modes driven by the \( ^3\)He-destroying nuclear reactions in the core. Therefore, strictly speaking, all standard solar models are incorrect. Moreover, because the source of the instability is in the core, anyone concerned with the solar neutrino problem should be especially aware of it. However, most workers in the field choose to ignore it, presumably because, so far as I can see, they find it uncomfortable to do otherwise.

4. WHAT WE CAN LEARN FROM FREQUENCY INVERSIONS

To a first approximation — and a very good approximation it indeed is — solar oscillations are adiabatic. The dynamics is described completely by pressure-gradient fluctuations driving the motion of fluid, which has inertia. Therefore it concerns only pressure \( p \), mass density \( \rho \), and the adiabatic exponent \( \gamma \) that relates their fluctuations to each other. Consequently, the only direct information that can possibly be obtained from the oscillations concerns just \( p, \rho, \gamma \), and, of course, any function solely of them, such as sound speed \( c \) and buoyancy frequency \( N \) (or the scaled so-called Schwarzschild discriminant \( A^* \) discussed by Kosovichev in his review of the difficult surface layers). Such information can come from what I choose to call primary inversions of the oscillation data. Inferences concerning other quantities, such as temperature, require the introduction other information, or assumptions. In particular, one might infer temperature if one knows the chemical composition and, of course, the equation of state. Unfortunately, both are uncertain.

One way of proceeding is to adopt the assumption that the sun is in thermal balance. Then, provided one is prepared to accept the calculations of opacity and nuclear reaction rates (and account correctly for the gravitational settling of heavy elements), one can then deduce the distribution of temperature \( T \) and the abundance \( Y \) of helium. Such inversions I call secondary inversions. A variety of such inversions have been carried out over the last six or seven years, some of which, as has been reported at this meeting, suggest that the variation of \( Y \) in the core might be smoother than what is predicted by the standard theory. I should remind the reader that the possibility of an intense magnetic field \( B \) in the solar interior should not be forgotten. Although there are cogent theoretical arguments to suggest that it neither contributes substantially to hydrostatic support, nor plays a very large role in the dynamics of the oscillations, it can certainly upset subtle secondary inferences.

Finally, I should add that the bold might believe standard solar evolution theory to the letter, and incorporate all the constraints of that theory into the scheme, to yield what have been called tertiary inversions. Only few such attempts have been made; even fewer have been published; and I'm not sure whether any can be considered to have been successful. But it is hardly profound to predict that more will be carried out in the future, and that reliable inferences will certainly be drawn from them, though I am tempted to suggest that the most re-
liable of those predictions will be none other than that standard solar evolution theory, despite its apparent success to date, is not entirely correct.

Let me not continue without qualifying a statement I made at the beginning of this section. When I mentioned pressure I meant it in a generic sense. I had predominantly in mind, of course, gas pressure $p$, which provides the major contribution to the stress. If it were the only stress, then $\gamma$ would be a thermodynamic quantity. But in reality there are also stresses due to turbulence and magnetic fields. These are both tensors, and their perturbations associated with the seismic motion are not related in a simple fashion to the corresponding perturbation in density. If the only stress additional to gas pressure were to arise from Lorentz forces, then, throughout most of the sun where magnetic diffusion is negligible on the scale of the oscillations, there would be a tensor generalization of $\gamma$ that relates stress to strain. But in regions of convection, the relation between Reynolds stress and strain depends on the dynamics of the coupling between the oscillations and the turbulence, which is very much more complicated. I should point out that even in the relatively simple case of having solely a magnetic addition to the gas pressure, the more complicated electromagnetic contribution does not lead to an identifiable complication to the eigenfrequency spectrum. The reason is that, at least from a mathematical point of view, all conceivable complexity in the spectrum can be produced formally by a solar model in hydrostatic equilibrium with no magnetic field (with an appropriate distribution of temperature and chemical composition). However, for a given frequency spectrum that model might be quite implausible, whereas a seismically equivalent model with a magnetic field might be relatively credible. Purely seismic inference must therefore be supplemented with additional nonseismic information, be it from nonseismic data or theoretical prejudice. In any case, I predict that, once we obtain reliable measurements of the aspherical seismic component of the sun, a great deal of theoretical model-comparison and model-calibration will take place.

5. THE SOLAR CORE

Knowledge of the structure of the energy-generating core is amongst the most important of the goals of helioseismologists. Not only is the dynamics of that region of fundamental interest, and of importance to any study of stellar evolution, but it is important also for controlling the conditions under which neutrinos are produced. The immediate problem posed by solar neutrinos should be viewed not merely in the narrow sense of finding a variant of the standard models (of the sun or of electroweak particle physics or both) that fit contemporary data, as has commonly been the case in the past. Instead, it should be viewed as finding the means of using the enormously richer data that will soon become available from the next generation of neutrino detectors currently under construction, coupled with the imminent new seismic data from SOHO and the ground-based networks, both to study in much greater depth the dynamics of the solar interior, and to investigate whether even a variant of the standard model of particle physics is viable or whether it needs to be abandoned. Symbiotic relationships between mature scientific disciplines — and I think it is not inaccurate to consider helioseismology to be approaching adolescence — often lead to breakthroughs.

I have already pointed out that the solar core is unstable to g modes. It really is quite amazing to me that so little attention has been paid to what might have been the consequence of that, for it profoundly impinges upon the pronouncements about theoretical neutrino fluxes. There have been two or maybe more papers published by people who have calculated neutrino fluxes arguing that the implications of the instability to neutrino production is not significant; but, interestingly, even though those papers put forward interesting evidence that should not be ignored, they are both, if I recall correctly, logically flawed. One wonders whether the authors are urging us all to share their blinkers. To my knowledge, the only serious investigation to suggest that the amplitudes of the unstable g modes are so severely limited as to be of no material consequence to the structure of the sun was that carried out by Dziembowski (1983): he estimated that the coupling to resonant pairs of stable high-degree g modes is so strong as to limit the velocity amplitude of the unstable mode to about 1 cm s$^{-1}$, which is truly miniscule. However, the calculation was carried out for highly idealized circumstances, and one wonders what the outcome would be if temporal and horizontal spatial inhomogeneity were taken into account, not to mention the interaction with yet other modes which might upset the phase coherence that is required for the energy-transfer mechanism to operate at optimal efficacy.

There has been some evidence produced in the past that all is not wholly well with the standard model of the solar core, and whilst there is no satisfactory account for that evidence, one should always, in my opinion, be receptive to the idea that the core is dynamically more active than standard solar modellers would have us believe.

I have also mentioned the secondary seismic evidence that the variation of $Y$ is not as great in the core as the standard models predict. Could that have come about by a redistribution by material motion after the helium was produced? Perhaps. But the outward decrease of $Y$ could also have been offset by gravitational settling against diffusion; I have not made a study of the implications of settling, but I presume that it leads to an accumulation of helium in the lower reaches of the radiative envelope, which must need to be offset somewhat by a reduction in the initial helium abundance of the model required by the calibration to present conditions, mainly by the constraint on the luminosity. How that and the consequent readjustment of the entire model influences the secondary seismic comparison I cannot say. No doubt this issue will be investigated further.

Other evidence for the degree of dynamical activity of the core will come from a seismic study of its rotation. Let us suppose for the moment that an unstable g mode is excited to substantial amplitude, and let us suppose also that it is a zonal mode ($m=0$), whose motion is ax-
isymmetrical about the rotation axis. Then we are led to ask:

Is the sun a tornado?

In such a motion, angular momentum is conserved by all fluid elements: any redistribution of angular momentum would spin up the core and slow down its surroundings. An example of a theoretical solar model with a mean flow resulting from such a (laminar) oscillation about an initially uniformly rotating state was analysed recently in a GONG hare-and-hounds inversions exercise. But is that picture representative of the sun? What would be the outcome if the g mode were not axisymmetrical, or if the motion were not a g mode at all but a convective mode in a shell surrounding the core, the existence of which has been postulated by Ghosai and Spiegel (1991)? If the motion were a transient disturbance to uniform rotation, an inward concentration of angular velocity would again result. To those with such ideas in mind, the first seismic inversion for the angular velocity of the radiative interior of the sun (Duvall et al., 1984) was very provocative, because it showed just that behaviour. The crucial property is not the rapidly rotating core, for that had already been predicted as a relic of an earlier more rapidly rotating epoch of the sun's evolution; it is that the region around the core was rotating more slowly. This was not only seen in the original seismic analysis, using data obtained from the South Pole by Duvall and Harvey (1984), but it is evident also in the inversion of more recently acquired data illustrated in Figure 2. It is interesting that that property had been predicted by nobody.

In retrospect, I can imagine the angular-velocity variation to have come about most naturally either by wave transport or by Reynolds stress due to essentially two-dimensional turbulence in almost spherical shells. Alternative, the Reynolds stresses in the convection zone might be transmitted into the radiative zone via a magnetic field. Notice in Figure 2 that the region in the vicinity of \( r = 0.3 R_0 \) rotates faster than the polar regions of the convection zone, so we are certainly not forced into concluding that the phenomenon necessarily demands interesting dynamics intrinsic to the region in and around the core.

It is extremely interesting to me that the recent rotational splitting frequencies of low-degree modes obtained by the BISON (Elsworth et al., 1995) are substantially lower than previous values. The implication is that at the time of the observations there must have been a region of the radiative interior rotating substantially more slowly than the equatorial regions of the convection zone, and probably more slowly than that indicated in Figure 2 for the deepest regions for which there are data. The BISON data were obtained from low-frequency modes whose spectral line widths are less than the splitting, so the individual components of a multiplet can be resolved. Therefore the splitting can be measured directly, rather than inferred statistically as is normally the case. Does this mean that these new data simply supersede the old? Or should one regard the discrepancy as evidence for temporal variation associated with the solar cycle? These questions will be relatively straightforward to answer in the not-too-distant future once we have the high-quality data from the ground-based networks over a sufficiently long interval of time.

Finally I must mention

\[ 159.99 \ldots \text{ min}, \]

which, if it concerns the structure of the sun, must surely concern the core. This period, or one very similar to it, has been with us as long as helioseismology. And still it remains unexplained; indeed there are many who doubt its very existence. Surely the imminent new data, particularly those from SOHO of which we would hope some to come from instruments that do not have 160 min as a characteristic period in their operating sequence, will at least settle the issue of whether the period is of solar origin. While I am on the subject, I might mention a possible 160 sec oscillation, to which I shall return later.

6. ON THE LARGE-SCALE DYNAMICS OF THE CONVECTION ZONE

According to Figure 2, the angular velocity \( \Omega (r, \theta) \) of the sun appears to be only weakly dependent on latitude beneath the convection zone. There is a sharp transition near the base of the convection zone to a latitudinal variation of \( \Omega \) within the zone, which is very close to the variation observed directly by noneisometric means at the surface. It seems likely that values plotted in the figure continue with hardly any change to the surface. This conjecture is consistent with some inversions of frequencies of high-degree modes which are confined beneath the photosphere to within a few per cent of the solar radius.

Contours of constant angular velocity are depicted in Figure 3. I illustrate two inversions, which look superficially quite similar when plotted in the manner of Figure 2. Inversion (a) is more typical amongst those that have been published. The oft-cited qualitative description of
it is that the angular velocity is essentially independent of radius in the convection zone and is essentially independent of latitude in the radiative envelope. When this result was first reported it caused some consternation amongst solar dynamo theorists and modellers of the convection zone, who had believed previously that the angular velocity is roughly constant on those cylinders aligned with the axis of rotation in the equatorial region that remain entirely within the convection zone; at higher latitudes, where the cylinders penetrate the radiative interior, opposition to vorticity stretching is less constraining on the zonal motion in the convection zone, and the flow is likely to be more irregular. The helioseismologists’ advertisement that instead the angular velocity in the convection zone is essentially constant on the surfaces of cones caused dynamo theorists to retreat to the shear layer at the base of the convection zone, a location which, incidentally, Spiegel and Weiss (1980) had much earlier suggested to be the seat of the putative solar dynamo. It is interesting to me that it is the region of mild shear evident in Figure 3a near the equator that is preferred for the dynamo, rather than the region of intense shear near the poles, which is largely ignored. Perhaps that is because traditionally it is the sunspots that have, quite naturally, evoked the most discussion in connexion with magnetic activity; and they reside at low photospheric latitudes.

When a result such as this has such a severe impact on the activities of a scientific community, it is mandatory that the scientists who transmit it are sure of what they say. In this instance, I believe we are not yet sure. By way of illustration I present a second inversion of the same data in Figure 3b. It resembles much more closely what dynamo theorists had previously believed. Indeed, it was out of a sense of responsibility to dynamo theorists that the inversion was carried out. Precisely how the inversion was accomplished is of no concern here: the only important point to make is that all helioseismic inversions, when the results are presented as functions, are intrinsically indeterminate, because only a finite quantity of data are available, and to carry them out requires those data to be supplemented by some prejudice, which formally can be regarded as containing an infinite amount of information. Usually that prejudice — more commonly called regularization — is one of preferring smoothness, which is imposed by penalizing rapid variation in an optimization calculation. The reasoning behind such regularization is that because the data cannot usually resolve rapid variation, the solution to the inverse problem should not contain it either; the hope is that the solution is a reasonably faithful, though possibly smoothed-out representation of the underlying function being sought. The penalizing function used in obtaining the representation in Figure 3a was chosen for mathematical convenience to us, the inverters, that convenience being a property of the manner in which the data had been analysed and presented by the observers, which in turn was largely a matter of convenience. The penalizing function used for Figure 3b was designed expressly to favour functions that are nearly constant on complete cylinders in the convection zone. Thus it was motivated physically rather than being apparently almost accidental. Mathematically, it was substantially more complicated to implement. For this reason the inversion in Figure 3b has been judged by some people to be less natural than that in Figure 3a. I must emphasize that at face value that judgement is quite unsound. What must be realized is the obvious fact that Nature does not carry out mathematics in the inelegant fashion that we do — indeed, she may not even do mathematics at all, but that I doubt — so what seems most natural to us can hardly be genuinely the most natural.

The mismatch with the data of the rotational splitting frequencies implied by the two functions \( \Omega \) depicted in Figure 3 are on average about the same. However, the discrepancies related to 3b appear more systematic than those related to 3a, which would suggest that 3a is more likely if the discrepancies were independent. But probably they are not. A proper error analysis has not been carried out, and might well be a harder task now than acquiring more extensive data. Those data, I predict, will in future give us a much clearer picture of the angular velocity. What will be essential to know are the splittings associated with modes with a wide range of \( m/l \) in order to resolve the latitudinal dependence of \( \Omega \) well enough to distinguish between the various possibilities such as the two in Figure 3.

Temporal variation of \( \Omega \) through the solar cycle is of considerable dynamical interest. We know already that the splitting data described by Libbrecht and Woodard (1990) vary significantly (i.e., by more than the published uncertainty) with time, which implies that \( \Omega \) must vary. Unfortunately, the data sets are separated too widely in time to determine whether the variation, which is presumably oscillatory, has the appearance of a nonlinear stationary wave or whether it appears to propagate, as many dynamo theories predict. I should point out also that Bachmann, Schou and Brown (1993) find temporal variation in their rotational splitting measurements too, but suggest that a major component of it could be
an artefact of their data analysis. Unfortunately, they have failed even to model a variation that resembles what they obtain from the real data, and are left unsure of the meaning of their results. So we must take heed and entertain some doubt about what we can deduce at the moment. Once again, however, I am quite sure that the imminent future holds the answer in store.

I have restricted attention in this section to the axisymmetrical component of the azimuthal flow: what we normally call rotation. We believe there is also nonaxisymmetrical convective motion on all scales down to the unobservable dissipation scale. It seems to me to be most likely that, unlike in the case of rotation, progress in studying such motion is more likely to be made with local analyses. I shall discuss these in a subsequent section.

I conclude this section with a word about the solar dynamo. I wish to point out to those who don't already know that this is a very technical term in the subject: jargon, one might almost say. I have it on good authority that the 'solar dynamo' is an accrual of processes called 'dynamo action', dynamo action being the processes that are studied by dynamo theorists and, so far as I can judge, encompass essentially the whole of magnetohydrodynamics. The aim of solar dynamo theorists is to construct theoretical dynamos with symptoms that resemble the magnetic behaviour of the sun. But the dynamo theorists do not usually ask whether the sun really is a real dynamo. Perhaps it doesn't matter. After all, the decay time of the largest-scale component of the solar magnetic field is comparable with the age of the sun. Therefore, if the field observed at the photosphere penetrates into the radiative zone beneath the convection zone, whether the modulation by the dynamo action augments or diminishes the total field energy is probably of little concern in understanding the plethora of phenomena seen presently at the surface. It probably has implications for the evolution of the sun in the longer term, however, and of other stars that exhibit activity cycles. Of course, if the photospheric magnetic field were not to penetrate into the radiative interior, a dynamo must be operating, for the characteristic magnetic decay time associated with the convection zone alone is quite brief.

Whether the field in the convection zone penetrates deeply into the radiative interior is evidently a crucial issue in the role of the putative solar dynamo. Some degree of penetration would seem to be necessary if the region of slow rotation in or around the solar core is to be explained by electromagnetic transmission of the turbulent stresses present in the convection zone. I doubt whether that issue will be genuinely resolved in my lifetime. However, I suspect that helioseismology will add much more useful grist to the dynamo theorists' mill.

7. OTHER SOLAR-CYCLE VARIATION

I think it is fair to say that the overall dynamics of the solar cycle is still not understood. As I pointed out in the previous section, much attention was shifted to the interface between the convective and radiative zones when it was thought that $\Omega$ is independent of $r$ in the convection zone. It seems now that perhaps that shift might have been a little premature, at least when considered in relation to the reason that caused it. But might one not ask whether the shift was even sufficient? It is still the case that hardly anyone studies the role played in the solar cycle by the body of the radiative interior. It is certainly true that if all the action takes place in the convection zone and in the vicinity of the lower interface, then because the radiative interior has such a high moment of inertia compared with the convection zone, its response to any change in the rotation of the convection zone is likely to be small — unless the radiative zone itself is undergoing some kind of self-sustained torsional oscillation. Such an oscillation, with a magnetic restoring force, was, I believe, the first idea to have been put forward, by Walén (1946), to account for the cyclic behaviour. The idea has since been abandoned, not least because it seems unlikely that such an oscillation can be sustained. To obtain an oscillation with a period as long as 22 years the motion must be essentially purely rotational: vertical motion would experience buoyancy, with a characteristic restoring time of an hour, and horizontal meridional motion would stretch the vorticity of the basic rotation, which would lead to an oscillation with a timescale of a few months. However, the problem with having purely rotational motion is that material attached to each field line essentially oscillates on its own timescale, and except in the vicinity of an O-type neutral point the timescales of neighbouring field lines are likely to be different. This leads to phase mixing and consequent extremely rapid dissipation. There is a slight possibility, in my mind, however, of the motion associated with neighbouring field lines being coupled by vortex stretching in the vicinity of the equatorial plane where the angular velocity vector is perpendicular to the potential-density stratification. The calculation of the coupling has not been carried out, but I suspect that the outcome would be that its magnitude is so small that the characteristic length scale perpendicular to the field would be found still to be very small, and dissipation, therefore, though not as great as the original simple argument suggested, would still be found to suppress the oscillation. Nevertheless, I still consider it to be a worthwhile pursuit to investigate the matter further.

There are other aspects of the solar cycle, not usually considered by dynamo theorists, that may shed light on the basic mechanism of the cycle and which must certainly be explained by a complete theory. One is the relation between variations in magnetic activity, the radiant energy flux from the photosphere, and the p-mode frequencies. That issue was first looked at in connection with latitudinal variation (Gough and Thompson, 1986), with a view to discussing the even component of degeneracy splitting first announced by Duvall, Harvey and Pomerantz (1986). Essentially the same discussion was subsequently applied to the solar cycle, in somewhat more detail, by Goldreich et al. (1991). My own hope had been that a single coherent explanation of the variation of all three phenomena would emerge: an augmentation of the efficacy of convection by magnetic buoyancy would simultaneously both increase the heat flux at the photosphere and modify the reflecting layers of the
acoustical cavity to account for the observed tendency of p-mode frequencies to increase in association with increased magnetic activity. However, it was found that that cannot be. The increase of convective efficacy, at least when it is modelled by local mixing-length theory, actually decreases the acoustic frequencies, and by an amount which is too small by a factor of nearly 50. It seems to be that the only remaining plausible possibility is that the frequency change is due mainly to the direct effect of the Lorentz forces on the dynamics of the oscillations. Greater magnetic field intensity in the surface layers at sunspot maximum is likely to augment the restoring force associated with the acoustic oscillations and thereby increase the eigenfrequencies. The magnetic influence could be immediately beneath the photosphere, or it could be in the atmosphere, as Roberts and his colleagues advocate. For a complete theory we need to be able to calculate by how much the field increases. Alternatively, we might use the seismic frequency variation to calibrate theories. This is an active area of research, and is bound to advance further with the imminent new seismic data.

An interesting detail of the temporal frequency variation observed by Libbrecht and Woodard is evident in Figure 4. It is normal to draw a smooth curve through the data, such as the continuous theoretical curve in the figure. But the data do not lie exactly on that curve. The deviation at low frequency appears to be systematic: I have drawn a dashed curve as a guide. The periodic undulations may require the eye of faith to accept, but if they are accepted, they imply a variation in the acoustic properties of the solar envelope at an acoustical depth of about half the cyclic frequency of the undulation, namely 670 seconds. This corresponds roughly to the He II ionization zone. That is interesting because the conditions in that region are such as to make $\gamma$, and consequently the sound speed, particularly susceptible to changes of stress. So perhaps the undulations are a signature of temporal variation of the subsurface magnetic field.

Additional progress is likely to be made from analysing finer details of the oscillations themselves, such as are manifest in the shapes of the lines in the power spectrum. There is direct observational evidence that the scale of granulation varies with the solar cycle. One would expect that modifications to the intrinsic excitation and damping of the modes, and, particularly in the case of high-frequency modes, the scattering, would lead to amplitude changes and changes to the shapes and widths of the lines in the oscillation power spectrum. One would hope that in the distant future these changes could be used to investigate how the convection changes within the context of a full theory of solar variability.

8. LOCAL ANALYSES

My discussion so far has concentrated on making inferences from the temporal behaviour of normal modes of oscillation. But some aspects of the properties of the sun cannot easily be investigated by these means, and in practice some cannot be investigated at all. Examples are the north-south antisymmetrical component of the angular velocity $\Omega$, and some aspects of the structural asphericity. The leading term in the perturbation expansion about the spherically symmetrical state yields a rotational frequency degeneracy splitting proportional to an integral of $\Omega$ weighted with a quadratic function of the displacement eigenfunction of the corresponding nonrotating solar model, which is an even function of latitude. North-south asymmetry does lead to a first-order distortion of the eigenfunction, however, so a measurement of that would more readily provide appropriate diagnostic information.

Most of the work in this field has been carried out using local analyses. The oscillations are regarded as waves propagating in a wave guide: the spherical shell between the lower and upper turning points. The horizontal structure of the wave at a given temporal frequency $\omega$ is then measured, from which it is hoped that the structure of the wave guide can be inferred. Strictly speaking, this is the means by which the distortion to a normal mode is measured.

The only technique actually to have been put to work on real data to measure the distortion is the most naive of all. An area on the surface of the sun is selected which is small enough for the seismic properties, such as horizontal advection velocity, to be safely regarded as being horizontally uniform, yet large enough for sufficiently accurate measurement of the horizontal component of the wave number to be possible. The angular variation of the wave number is then measured, by analysing the shapes of rings of power associated with each order $n$ in a slice of the spectrum at constant $\omega$, from which an advection velocity is deduced. After having carried this analysis out
for a range of \( \omega \) and \( n \), corresponding to modes that sample depth differently, Patrón et al. (1995) have been able to map out the horizontal velocity as a function of depth. Repeating this process over different patches of the solar surface then yields a three-dimensional image of the horizontal flow. That flow comprises basically the largest of the convective eddies, whose structure it is possible to resolve by this procedure. In principle, sound-speed variations could also have been mapped, by measuring the variation in the mean radii of the rings. Any large-scale north-south asymmetry must, of course, emerge automatically from this procedure.

Ring analysis, in its present form, makes no explicit accommodation for the local distortion of the wave that actually takes place, except through the different values of the averages over different surface patches that are obtained. Attempts have been made to take distortion into account with artificial data, tracking the phase variation defined in terms of a Hilbert transform. Reasonable success has been achieved in one dimension, but the two-dimensional problem has not yet been adequately resolved. The major problem appears to be the inability to remove artefacts arising from interference between two nearly parallel waves. It should be noticed, however, that interference occurs whether one recognizes it or not, and that therefore the analysis of the rings in the 2-D power spectrum must suffer from a similar intrusion. What we must certainly do in the immediate future is to test the ring analysis on artificial data.

An alternative procedure is to correlate the oscillations of small separated patches, A and B, on the solar surface: so-called time-distance helioseismology. It is presumed that the patches are connected by acoustic rays, either directly or by rays that suffer one or more intermediate reflections at the surface. Determining the time delay in a cross-correlation, or something mathematically equivalent, then provides a measure of the phase propagation time from A to B. Measurements have been carried out by Duvall, Jeffries, Harvey and Pomerantz (1993), and appear to yield results comparable with the usual normal-mode projections. However, it may turn out that this procedure handles noise differently, and possibly to greater advantage for inversion. It is bound to be more convenient for studying localized inhomogeneities, such as sunspots and the convective downdrafts one expects to find at the boundaries of giant cells or in the vicinity of magnetically active regions. Unlike standing waves (which are superpositions of waves travelling in opposite directions), waves propagating in a given direction can sense vertical motion in leading order, provided that the motion is encountered predominantly on either the descending or ascending branch of the ray. Consequently for such investigations, time-distance analyses might be more productive than normal-mode analyses. They should permit one to differentiate between upwelling, downdrafts and other agents such as sound-speed perturbations or magnetic fields that cannot distinguish up from down. By combining data from waves travelling in different directions, and reflecting at different points on the surface, it should be possible to unravel much of the structure of the flow, and certainly to calibrate models that make certain presumptions about the flow geometry.

A property of the waves that time-distance analyses might handle straightforwardly is the acoustic reflectivity of the solar surface, and possibly of subphotospheric boundaries of sunspots. However, genuine reflection coefficients have not actually yet been measured. These and other correlation studies are in their infancy, but I am sure that we shall see much development in this kind of treatment after the more obvious inversions of the SOHO and GONG data have been carried out.

9. MODE EXCITATION

It is believed that turbulence in the upper convective boundary layer generates, scatters and absorbs the acoustic waves that constitute the normal modes which we use for diagnosis. Kosovichev has already discussed these processes at this meeting. And we both have mentioned how at present we try to avoid contamination of our inversions by uncertainties in this complicated region, partly by removing a contribution from the data that we believe contains the contaminating effects, and partly by restricting attention to the lower-frequency modes which have deeper upper turning points and consequently are less susceptible to the vagaries of the turbulence. However, there is considerable interest in augmenting the data sets we currently use with some higher-frequency modes, since formally these penetrate more deeply into the energy-generating core. To this end it is necessary to use a model of the excitation process in analysing the data, in order to try to extract the frequencies the modes would have had were they not being driven or damped, namely the adiabatic free-oscillation frequencies, for it is those that are used for inversion. Several approaches are being developed at present, from extracting asymmetries in the lines in ordinary Fourier power spectrum to using different representations of the oscillations, such as the wavelets of Baudin et al. (1993) and the models of sets of continuously excited oscillators that have been discussed at this meeting by Chang and his collaborators.

To be assured that such methods work, it is necessary not only to test them on artificial data but also to compare the statistics of the artificial data with real data. We have seen at this meeting superficial similarities and potential discrepancies between solar data and the sets of randomly excited oscillators that have been used to model them. A more careful comparison will surely be carried out in the very near future to test the models. Note that once a satisfactory model has been found, the analysis of the solar data will provide us not only with the free-oscillation frequencies but also with valuable information about how those oscillations have been forced.

The latter will be used to test and calibrate models of turbulent convection and its interaction with the oscillations.

I should mention that a totally separate fruitful route to this goal that is being pursued is to study numerical simulations of the convection-pulsation interaction,
such as those being carried out by Stein and Nordlund (1991). The solar data could be used to calibrate the simulations, and the simulations can be scrutinized in great detail to determine which are the most important processes and how they operate. This will not be a simple task. Even to divide a known flow unambiguously into what might legitimately be called turbulence and what one understands to be acoustic and gravity waves is not easy, and in any case is somewhat arbitrary. This is so even for flows that are only mildly nonlinear. Solar convection is strongly nonlinear: the turbulent perturbations to the background state on which the waves are considered to propagate are large, so much so that transient acoustic holes develop in the acoustically reflecting layers, permitting p modes whose frequencies are well below the mean acoustical cutoff frequency temporarily to propagate up to the photosphere. This phenomenon is likely to enhance locally the oscillatory power in Doppler and intensity observations, adding to a well-known phenomenon that indeed was originally thought to be due only to a process not very dissimilar from this, but which at present is normally regarded as being produced solely by linear mode interference.

10. SOLAR DERMATOLOGY AND BEYOND

The bastard title of this short section is justified on the ground that it suits the subject. The study of waves in the solar atmosphere, both propagating and evanescent, is an extremely complicated subject, because we can observe the phenomenon. The atmosphere is inhomogeneous and very complicated, and consequently so are the waves that propagate through it. Short-period waves have been studied locally and extensively for a very long time, and although a great deal has been learnt from both observations and theory, we are all very aware that we are far from understanding in detail what is going on.

Perhaps global waves are simpler to think about. Although low-degree modes are scattered into modes of high degree by the inhomogeneities in the atmosphere, they should retain some of their character. It was with this in mind that nearly 15 years ago a group of theoretical helioseismologists visiting Nice after a conference saw in the power spectrum of the whole-disk observations that had been carried out at the South Pole by Grec, Fossat and Pomranz (1981) a peak at a frequency beyond the range of the spectrum that had been published. When the period was measured to be 160 sec, further discussion was irresistible. Could it be that it was a chromospheric mode? Such p modes trapped in the chromosphere are predicted theoretically in models of smooth chromospheres. But observationally, chromospheric modes are elusive, presumably because the real, highly inhomogeneous chromosphere is acoustically too leaky. However, perhaps the modes of lowest degree could retain enough coherence to resonate in the spherically averaged chromospheric cavity. A letter to Nature was quickly drafted in a café in collaboration with the observers, but it was never submitted because the observers were subsequently too cautious — or perhaps I should say too wise — to trust a small unconfirmed peak that could have been noise. It should first be confirmed.

So far as I am aware, it never has.

So why am I telling this story so long after the event? The reason is that there is now evidence that coherent information does propagate through the chromosphere and corona into the solar wind. Recent analyses of particle-flux data from the spacecraft Ulysses by Thomson, MacLennan and Lanzerotti (1995) has revealed a sharp-line spectrum, with the frequencies of most of the low-degree modes that we know to be present, at least after an upwards adjustment 0.08 per cent of the wind frequency. This is such a surprise that there is scepticism about their interpretation as a signature of solar oscillations. But if it turns out that the interpretation is correct, there will be much excitement, because there are also frequencies in the g-mode range. If the modes responsible could be identified, then we shall have achieved the third of the desires listed in the first section, and probably before the year 2000. It takes little clairvoyance to perceive that here is an arena of intense future activity. I hope it turns out to be an activity in helioseismology.

11. THE SOLAR LABORATORY

The objective of helioseismology is to determine the stratification and the kinematics, even the dynamics, throughout the solar interior. But that is merely a means to a greater end. Helioseismology is just another tool for scientists to add to their arsenal. It has been fun developing it, and I am quite sure that there is still much further fun to be had. Indeed, the study of the oscillations themselves is a not unimportant area of scientific research. However, for most scientists active in the field the greatest excitement comes from the discoveries that our new tool affords us: discoveries in astrophysics or in other branches of physics. I close with an example of the latter:

I mentioned early in this talk the present failure to determine the helium abundance because of our inadequate knowledge of the equation of state. In Figure 5a is compared a thermodynamic quantity $\Theta$, which is a function of $\gamma$ and its derivatives with respect to thermodynamic state variables, measured by Baturin et al. (1994) in the sun and in a most closely matching theoretical model solar envelope. I use the term ‘measured’ because what I am actually comparing are the two continuous lines, both of which are inversions of frequencies of the same set of modes, the one with vertical error bars being from observed solar frequencies, the other being from the eigenfrequencies of the theoretical model. The inversions were actually carried out asymptotically for the quantity $W \equiv (r^2/GM_\odot)\sigma^2/dr$ which is essentially equal to $\Theta$ in adiabatically stratified stellar envelopes of small mass, but that detail is really beside the point. What I am really doing is making a comparison between two sets frequencies that have been processed in an identical nonlinear fashion. The reason for processing them in this way is to eliminate most of the contaminating effects of extraneous factors, leaving a signature of essentially just the quantity of interest. (As helioseismologists

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know well, any direct comparison of raw frequencies, unless interpreted very carefully, can be even worse than useless, because extraneous factors are liable to confuse the comparison and lead to erroneous interpretations.) The extent to which the signature really does represent the quantity of interest can be judged by comparing the upper continuous curve with the dashed curve, the latter being the exact value of Θ for the theoretical model. Attention should be restricted to the region indicated by the horizontal line immediately above the abscissa, which indicates where the signature is reasonably faithful. The theoretical model was adjusted (by varying Y and the mixing-length parameter α) to obtain a best fit. Evidently the equation of state, which Θ characterizes, is incorrect. The result of modifying the equation of state by adding a density-dependent ionizing term to the partition function, modelled by giving atomic hydrogen and helium and singly ionized helium finite radii rC, such that their (outer) electrons are stripped as the mean interparticle distance approaches rC, is shown in Figure 5b. The radii chosen, rC = 1.67rB, where rB is the appropriate Bohr radius, is that which gives the best fit. Compared with before, agreement is now excellent.

I am not claiming that the effective radii of the atomic species and singly ionized helium under the dense-plasma conditions of the solar convection zone have actually been measured. There are no doubt factors of similar importance that have not been taken into account in the computation of γ and its derivatives. But what this exercise does demonstrate is that solar data are accurate enough for us to measure an effect as small as that produced by the nonzero size of an atom or ion. Moreover, our relatively sophisticated and well understood diagnostic procedure makes us reasonably sure that what we have calibrated is actually a property of the equation of state, and not something else, such as the opacity in the photosphere. So here are the beginnings of the establishment of the sun as a physics laboratory. There is no doubt in my mind that our use of that laboratory will be refined greatly in the coming years, and will become a very valuable arena for research.

12. CONCLUSION

I include with this report only a very short list of references: just a few to illustrate some of the things that have been done in the past in order to set the scene for the future. Most of the references I have omitted are to work in preparation, variously authored, I hope, by every scientist present here and, I know, by more besides. As in most branches of astronomy, there are three broad categories of scientist: observers, data analysts and theorists. The most immediate task of the observers will be to persuade others that their data are of sufficient interest to warrant serious attention. In most cases that will not be difficult. However, manpower is scarce, so it cannot be guaranteed that all the data will receive the attention they deserve. The task of data analysts will be to expend, if necessary, at least as much effort in analysing the data as the observers spent in acquiring them. Although that is self-evident to most of us, in some quarters it seems to be a heretical view. And finally, the task of the theorists is to try to assemble the information in a way that leads to new understanding, and to transmit that understanding to the rest of the scientific community in an easily digestible form. All three tasks, I am sure, will be achieved.

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