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Some recent and future helioseismological inferences concerning the solar convection zone

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Abstract. Several uncertain helioseismic findings of potential interest to Jüri about the solar convection zone are briefly discussed, along with some personal optimistic hopes for the future.

Keywords. Helioseismology, solar abundances, sun's age, solar magnetic field, meridional flow

1. Some Background

Helioseismology can be summoned to yield the seismic structure of the sun: the variation through the sun of those quantities that control the propagation of (the essentially adiabatic) seismic waves, principally density and pressure (which are related by hydrostatics) and the relation between them under adiabatic change, which is characterized by the adiabatic exponent $\gamma_1 = (\partial \ln p / \partial \ln \rho)_{\rm ad}$; I add to that list macroscopic motion and magnetic field, about which some information can be obtained, although they cannot be determined completely by seismology, even in principle. Any property of the sun that cannot be expressed solely in terms of those quantities is not a purely seismic variable, and cannot be determined by seismology alone. I should add that I shall be assuming in my discussion a knowledge of the mass M (at least GM) and the radius R (which I refrain from even attempting to define here), which are obtained by non-seismic means. I am being explicit about this point to draw attention to the fact that isospectral stellar structures exist with different M and R, a property (amongst others) which renders asteroseismology without M or R less informative than helioseismology.

It is common to represent the (almost spherically symmetrical) hydrostatic structure of the sun by $c^2(\mathbf{r})$ and $\rho(\mathbf{r})$, where c and ρ are sound speed and density; c^2 is preferred to c because for a perfect gas $c^2 = \gamma_1 p/\rho$ which is approximately proportional to $\gamma_1 T/\mu$, where p is pressure and μ is the mean molecular mass (to a first approximation the perfect-gas law is an adequate guide); c^2 therefore resembles temperature T. Because the structures of modern models are quite close to that of the sun, it is expedient to consider the small relative deviations $\delta \ln c^2$ and $\delta \ln \rho$ of the sun from a reference theoretical model, such as Model S of Christensen-Dalsgaard *et al.* (1996), rather than the bare values of c^2 and ρ. Those deviations are typically no greater than 0.2% and 1.5% respectively.

Solar models are typically produced by evolution from the zero-age main sequence, adjusting the initial helium abundance Y_0 for a given initial heavy-element abundance Z_0 (or, equivalently, any combination of initial abundances Y_0 and Z_0), and a scaling factor in an algorithm to model convection – typically a mixing-length parameter – to reproduce the observed luminosity L and radius R. That establishes a relation between Y_0 and Z_0: models with lower Z_0 have lower Y_0. It is worth noting that in the models (i) temperature T decreases at fixed radius r as Y_0 decreases, partly because μ is lower globally and partly because there is a greater abundance of hydrogen in the core to fuel the nuclear reactions which therefore provide the observed luminosity L at lower T (and ρ), and (*ii*) as the star ages, a slight depression in c^2 is produced near $r = 0$ because in the central core nuclear reactions have converted hydrogen into helium and thereby increased μ locally. In the early days of helioseismology the former property played an important role in calibrating Y_0, to a value that was then perceived to be incompatible with neutrino observations (those were the days before neutrino transitions had been detected); nowadays the latter property is used for calibrating the age of solar models.

The most prominent property of $\delta\ln c^2$ is a narrow hump near $x = r/R = 0.65$; it is associated with the tachocline, about which I shall say a few words later. There is also a discrepency in the convection zone, probably due largely to an error in the value adopted (implicitly) for R (which I continue to refrain from discussing – but see Takata & Gough, 2003). Finally, there is a large-scale discrepancy in the radiative interior, which is unexplained. I emphasize that c^2 in some models deviates from that in the sun by only a few tenths per cent (errors in the determination of the solar c^2 are even smaller).

2. On the age of the sun

Guenter Houdek and I (e.g. 2007, 2009) have recently been seismically calibrating solar models to characterize the central dip in c^2, and hence to estimate the main-sequence age $t_\odot$ of the sun. Modes of the lowest degree l must be used, for it is they that penetrate the most deeply into the core. We have confined ourselves to only such modes (having $l \leqslant 3$), with the intention of using our procedure for stars other than the sun.

Were the structure of the sun to be smooth, the cyclic frequencies $\nu_{n,l}$ of the low-degree modes would be given (asymptotically), in terms of $x_{n,l} := \nu_{n,l}/\nu_0$, by

$$x_{n,l} \sim n + \frac{1}{2}l + \varepsilon + \sum_i \sum_{j=0}^{i} A_{ij} L^{2j} x_{n,l}^{1-2i} \tag{2.1}$$

for large order n, where $L^2 = l(l+1)$ and ε, ν_0 and A_{ij} are functionals of the solar structure, independent of l and n. The most deeply probing terms are the most strongly L-dependent, having coefficients $A_{ii} \simeq \int f_{ii} \mathrm{d}r$ for each i, in which, except where $R - r \ll R$,

$$f_{ii}(r) \propto \left(\frac{1}{r}\frac{\mathrm{d}}{\mathrm{d}r}\right)^i c^{2i-1}. \tag{2.2}$$

The L-independent terms, namely ε and $A_{i0}x_{n,l}^{1-2i}$, depend principally on the surface layers, whose influence on the oscillation frequencies is rendered uncertain by the inadequacy of our understanding of convection. The fiducial frequency ν_0 is the inverse of twice the acoustic radius of the star, and is therefore a global indicator.

A first attempt to calibrate $t_\odot$ was made by fitting formula (2.1) to raw observed frequencies. That suffered from 'contamination' by oscillatory (with respect to n) deviations from the smooth formula (Gough, 2001). There was also the problem of having to calibrate the models with respect to two parameters, namely $t_\odot$ and Y_0, whose influences on the unknown coefficients in the formula were not easily separated. (I must acknowledge that at about the same time Bonanno, Schlattl and Paternò (2002) attempted a similar calibration to determine $t_\odot$; however, they assumed a value for Z_0, and did not allow for the chemical composition to vary, so their one-parameter fit was more straightforward, although, of course, less reliable.) The oscillatory deviations are produced by acoustic glitches, caused partly by the (near) discontinuity in $\mathrm{d}^2c^2/\mathrm{d}r^2$ at the base of the convection zone and by depressions in γ_1 caused by the ionization of helium (and hydrogen) (Gough, 1990). They have 'frequencies' roughly twice the acoustic depths τ_g of the glitches. The amplitude of the γ_1-induced oscillation depends on Y, providing a separate datum for calibrating $t_\odot$ and Y_0, and thereby rendering the calibration more stable. The outcome currently is

$$t_\odot = 4.60 \pm 0.02 \text{ Ga} ,$$

with $Y_0 = 0.253 \pm 0.004$ and $Z_0 = 0.016 \pm 0.001$. The present photospheric abundances of the calibrated model are

$$Y_\mathrm{s} = 0.227, \quad Z_\mathrm{s} = 0.0146 .$$

I must emphasize here that this seismically calibrated model, in common with others of its genre (e.g. Turck-Chièze *et al.*, 2001), is not a seismic model, for its structure agrees with only a few limited aspects of the sun, and is not seismically acceptable throughout.

An interesting question that arises naturally from this exercise is how the age of the sun (measured, as we do, from what one can define as the instant of the zero-age main sequence – I shall discuss that instant in an instant) compares with the ages of the oldest meteorites. That can have implications regarding the formation of the solar system, which, it is generally believed, took place over a timespan of some 10^7 years or less (there are some who would say 'more'). Evidently, even if we disregard the systematic modelling errors, we have not yet achieved adequate precision. But the goal is almost in sight. The value quoted above is (perhaps conveniently) not significantly greater than modern determinations of the ages of the oldest meteorites (e.g. von Hippel, Simpson & Manset, 2001).

Finally, a word about an origin of solar time. On the main sequence the characteristic evolution timescale of the sun exceeds the thermal diffusion time by a factor 300 or so. Therefore, once established, the sun is (probably) in thermal balance to quite a good approximation, the rate of generation of nuclear energy in the core equalling the radiant luminosity a the surface. The sun arrived on the main sequence by gravitational (Kelvin-Helmholtz) contraction moderated by thermal diffusion, on a timescale $t_\mathrm{KH} \simeq 10^7$ a. This is comparable, not entirely fortuitously, with the formation timescale of the planetary system. The nuclear generation of heat halted the contraction, but only gradually. So one might wonder whether it is even meaningful to define a precise instant from which to measure main-sequence age. In fact it is, because it turns out, again not entirely fortuitously, that on the main sequence the relative abundance X_c of hydrogen ($X = 1 - Y - Z$) at the centre of the sun is very nearly a linearly decreasing function of time (Gough, 1995). Therefore one can extrapolate $X_\mathrm{c}(t)$ backwards quite reliably to its initial value X_0 (gravitational settling in the core over a time t_KH is tiny) to define an origin.

3. On the heavy-element abundance of the sun

The value $Z_\mathrm{s}/X_\mathrm{s} = 0.0193$ obtained by the model-fitting procedure described in the previous section is substantially lower than values that were fashionable a while ago – e.g. $Z_\mathrm{s}/X_\mathrm{s} = 0.0274$ (Anders and Grevesse, 1989); Model S has an abundance ratio $Z_\mathrm{s}/X_\mathrm{s} = 0.0245$ – although it is somewhat greater than the value promulgated recently by Asplund *et al.* (e.g. 2005), who carried out a new spectroscopic analysis of the (near photospheric) solar atmosphere taking the turbulence produced by convection into account, and recommended $Z_\mathrm{s}/X_\mathrm{s} = 0.0165$. A brief account of the issue raised by the new analysis is not inappropriate here.

It is normally presumed that the photosphere, which is well mixed in by convection, represents the composition of the radiative envelope beneath the convection zone, aside from a small modification from gravitational settling. This exposes a discrepancy with solar modelling that has exercised many minds in recent years. The adjustment of Z_0 suggested by the new spectroscopic analyses is large: more than 30%. But the effect on the equation of state is small, since heavy elements constitute less than 2% of the solar material. Therefore the effect of lowering Z_0 in a solar model is essentially just to reduce the opacity by a similar amount. That reacts on the temperature gradient required to maintain the heat flux, producing a change in T and a comparable change in c^2, throwing the model out of agreement with the sun. Any model calculated using this low value of Z_0, with $t_\odot$ roughly 4.6 Ga, using generally accepted microscopic physics (equation of state, opacity, nuclear reaction rates) and adopting the usual so-called standard tenets of stellar-structure theory, must necessarily be ruled out by seismic observation. Although that may seem obvious, there has been a spate of publications labouring the point, and presenting numerical examples of the seismic disagreement with low-Z models often without edifying comment; they have been catalogued recently by Basu and Antia (2008). I hope it is hardly necessary for me to point out that the seismological analysis is based on extremely simple and well understood physics (at the level required for the present discussion), and therefore is not open to serious doubt.

The disagreement can be presented in a variety of ways. Perhaps the most acceptable amongst discussions adopting the tenets of standard stellar-structure theory is to retain the equation of state (it surely cannot be wrong by as much as 30%) and, I recommend, the nuclear reaction rates, for they have been studied extensively in the last decades in connexion with the neutrino problem (which is now, at least in its basic form, resolved; I should acknowledge, however, that, as has been pointed out by several critics, the p-p cross-section determining the slowest, controlling, reaction of the chain has been determined only theoretically). Assuming no mixing in the deep interior, one can easily scale Y_0 from a model to estimate $X(r)$ in the sun today with adequate precision for the purpose in hand, hence obtain $T(r)$ from $c^2(r)$, and thereby compute the opacity required for transporting the required amount of heat by radiative transfer. Not surprisingly, the outcome (e.g. Gough, 2004) is close to the value in reference model S of Christensen-Dalsgaard *et al.* (1996). The problem posed by Asplund and his colleagues is therefore to reconcile that value with the photospheric chemical composition.

Several possibilities for resolving the disagreement come immediately to mind. Perhaps the sun condensed gravitationally in its primordial interstellar gas cloud around a seed giant-planet-like condensation which had already shed some of its hydrogen and helium. Then the sun's radiative interior could have been rich in Z, and the difference, being stable to double-diffusive convection, could have survived to the present day. This is a mechanistic justification for having an appropriate compositional variation in the outer reaches of the sun's envelope, an hypothesis suggested originally by Guzik, Watson and Cox (2005). The density variation caused by the composition variation consequent on this hypothesis, even were it confined to a thin interface, would contribute no more than about 30% to the amplitude of the associated oscillatory signature (see §2) in the eigenfrequencies. This is probably too small to be detected unambiguously with currently available data. But maybe in the future, with a sufficiently sophisticated analysis, it could be disentangled from other aspects of the stratification near the base of the convection zone.

It has been suggested that alternatively there could be mechanisms other than radiative transfer, such as gravity waves, to transport the heat; but wouldn't it be incredible for such processes to mimic the functional form produced by the physics of the radiatively induced atomic transitions that determine opacity? That objection can be levelled against most other suggestions too. So one is, perhaps reluctantly, led to wonder whether the abundance determinations by Asplund and his colleagues are correct. It is interesting to note that recently their value has been revised upwards a little – $Z_\mathrm{s}/X_\mathrm{s} = 0.0181$ (Asplund *et al.* 2009) – and that Caffau *et al.* (2010) have carried out a parallel spectroscopic analysis, obtaining a somewhat higher value still, namely 0.0211. These two values bracket that obtained by the model calibration reported in §2. However, I reiterate that the calibrated model is not a seismic model.

It behoves us to seek some independent way of determining Z. One might attempt that seismologically, by measuring $W := (r^2/Gm)\mathrm{d}c^2/\mathrm{d}r$, where $m = 4\pi\int\rho r^2\mathrm{d}r$. In the adiabatically stratified regions of the convection zone, $W \simeq \Theta := 1 - (\partial\mathrm{ln}\gamma_1/\partial\mathrm{ln}\rho)_p - \gamma_1[1 + (\partial\mathrm{ln}\gamma_1/\partial\mathrm{ln}p)_\rho]$ (Gough, 1984), which has humps where γ_1 is lowered by ionization. We first determined Y by that method. I recall announcing the intention to carry out the determination at a meeting in Cambridge in 1985. Donald Lynden-Bell said he thought it was impossible, and wagered that in any case it would not be accomplished within 10 years. He was right. But for the wrong reason. He thought that we would be unable to measure the helium hump in W with adequate precision. But actually we measured it so precisely as to show that it was incompatible with equations of state of the time (Kosovichev *et al.*, 1992), implying that those equations could not be trusted to convert W into a reliable value of Y. However, we were able to refine previous estimates using currently available equations of state, and found Y to be lower, by several per cent, than the value of Y_0 used in typical theoretical models – a finding subsequently corroborated by others (e.g. Serenelli and Basu, 2010) – thereby emphasizing the need to consider the influence of gravitational settling. Christensen-Dalsgaard, Proffitt and Thompson (1993) demonstrated that models incorporating gravitational settling can be enormously closer in structure to the sun than those that do not. Gravitational settling was therefore included in Christensen-Dalsgaard's model S.

So now I suggest that history be repeated. My colleague Katie Mussack and I will try to measure the minute humps in Θ produced by the ionization of principally C, N and O beneath the region of appreciable HeII ionization (Mussack & Gough, 2009). Of course we cannot expect a precision comparable with what can be achieved for helium, but a robust, albeit roughly determined, amplitude of the ionization-induced variation in $W(r)$ about a background – whose value is rendered uncertain by our inadequate understanding of the van der Waals effects from bound species of hydrogen and helium (Baturin *et al.* 2000) – should be achievable; the current debate might then, at least partially, be settled.

4. Adiabatic stratification of the deep convection zone

The first numerical simulations of solar convection, predecessors of calculations for which Jüri is now famous, did not provide a reliable indication of the stratification deep in the convection zone. Yet we all know from laboratory experiments with convection that the lapse rate approaches its neutral value at large Rayleigh number. One expects $\Delta := \gamma_1^{-1} - \Gamma_1^{-1} \simeq \nabla - \nabla_\mathrm{ad}$, where $\Gamma_1 = \mathrm{dln}p/\mathrm{dln}\rho$, to be extremely small, and indeed mixing-length theory predicts values of order 10^{-6} deep in the solar convection zone. However, it is certainly of interest to seek independent, seismological, evidence for the smallness of Δ. That is a difficult, because one cannot measure Δ directly: one must be content with what is essentially a measurement of Γ_1^{-1} subtracted from the corresponding adiabatic value. An upper bound is therefore the best one can expect.

The only attempt of which I am aware was carried out in the early days of helioseismology when the data were much less precise than they are today. The result was

$$\Delta < 0.03$$

(Gough, 1984). It would be interesting to see by how much this bound can be tightened with more modern helioseismic data. What we tend to do today is to assume that Δ is utterly negligible, and use that result to infer the thermodynamic quantity $\Theta \simeq W$. It was principally by measuring the hump in W in the second ionization zone of helium that the helium abundance has been measured (to within the undetermined errors in the equation of state). The adiabatic constraint reduces the function space in which one isolates the thermodynamic diagnostic, thereby eliminating some of the extraneous contaminating properties of the stratification.

5. Stratification of the tachocline

As is well known, the convection zone rotates differentially. Described in very broad terms, the latitudinal variation of the angular velocity Ω observed at the surface persists throughout the convection zone, and is separated from a uniformly rotating radiative interior by a thin shear layer called the tachocline. Spiegel and Zahn (1992) demonstrated that had the convection zone abutted directly onto the radiative interior (presumed to be nonmagnetic) the differential rotation would have burrowed into the interior within the sun's lifetime. They concluded that some mechanism in the tachocline must isolate the interior from the shear. They suggested the presence of a thin layer of horizontally isotropic essentially two-dimensional turbulence, of sufficient vigour to overcome the shear. McIntyre and I (1998) argued subsequently that two-dimensional turbulence in a rotating flow does not behave in that manner, as I have believed for a long time (e.g. Gough and Lynden-Bell, 1968), and we cited some more recent evidence in support. We argued that the only conceivable way that the interior could rotate uniformly is for it to be rigid, held by a large-scale (primordial) magnetic field. I still hold that view, although I hasten to add that it is far from being generally accepted (e.g. Brun and Zahn, 2006), although the conclusion that some agent rigidifies the interior is coming to look more and more likely.

Whatever causes the rigidity of the interior, it is inevitable that gyroscopic pumping in the convection zone must produce a proclivity for a meridional circulation connecting the convection zone with the tachocline, in at least all but the lowest latitudes. Indeed, Spiegel and Zahn (1992) analysed such a flow in their two-dimensionally turbulent tachocline. That flow transports to the convection zone helium that had settled under gravity, homogenizing the tachocline with the convection zone above. The outcome is to reduce the mean molecular mass in the tachocline, and thereby raise the sound-speed. That process is no doubt the cause of the sound-speed anomaly beneath the convection zone which I mentioned at the end of my introductory background discussion. Julian Elliott and I (1999) attempted to calibrate the thickness of the tachocline by fitting the anomaly to a solar model with an artificially mixed layer, obtaining a value $0.02\mathrm{R}_\odot$.

The reason I say we attempted (rather than succeeded in) performing the calibration is that although the final model that we obtained deviated from model S with an anomaly essentially identical to that observed, it was not quite in the right place. What we failed to point out is that simply moving it to the right place by adjusting the depth of the convection zone would have produced a large-scale deviation in sound speed throughout the radiative interior. This is a phenomenon that had been known for a long time (e.g. Christensen-Dalsgaard *et al.*, 1985) and is no doubt why Brun, Turck-Chièze and Zahn (1999) had had trouble fitting their evolved solar models to the seismic inferences.

An investigation by Takata and Shibahashi (2003) and a more recent unpublished investigation by Jørgen Christensen-Dalsgaard and myself have failed to produce a seismically acceptable spherically symmetrical model with a partially mixed layer that resides completely beneath the convection zone. The implications are unclear at present, although the result may be evidence for tachocline asphericity (although I hasten to add that the essentially hydrostatic balance of forces implies that at least the base of the tachocline, as denoted by the molecular-mass gradient, must be very nearly spherical).

6. Solar-cycle variation of the stratification of the convection zone

There is much discussion at this conference on the dynamics of the solar cycle. Are there seismological consequences that could be used to test the theories? Libbrecht and Woodard (1990) have presented seismic frequency changes of low- and intermediate-degree modes during the rising phase of cycle 22. They found that the changes were approximately inversely proportional to the inertiae of the modes, indicating that the predominant structural variations are confined to the near-surface layers of the sun. But there might be another component of the variation, an oscillatory component barely discernible to the eye, which could be indicative of a localized temporally varying acoustic glitch. However, it may not be real, and indeed Antia and Basu have declared it to be insignificant (e.g. Gough 2002). Goldreich *et al.* (1991) suggested that it might be due to a thin sheet of horizontal magnetic field buried somewhere in the sun, as had been discussed by Gough and Thompson (1988, 1990) and Vorontsov (1988). The oscillatory feature would therefore be expected to be greatest at sunspot maximum. Subsequently I measured the frequency of the oscillation (Gough, 1994), and found it to to be about 700s, corresponding roughly to the depth of the HeII ionization zone and therefore locating the glitch in the convection zone. It seems quite unlikely that the integrity of a magnetic sheet could be maintained against the disruptive influence of the turbulent convection; and indeed numerical simulations by Tobias and his collaborators (2001) have supported that view. Instead, it is more plausible that a magnetic field in the convection zone would be more evenly distributed, on a vertical length scale greater than the helium glitch. Therefore an increase in the intensity of the field, which might be expected at solar maximum, would actually dilute the glitch and thereby reduce the amplitude, Γ, of the oscillatory feature, not augment it. (A tangled field would act similarly.)

Whether the evidence for a variation in Γ is significant or not, an upper bound can be set on its magnitude from Libbrecht and Woodard's observations: $\Delta\ln\Gamma \lesssim 0.025$. That corresponds to a variation in the horizontal magnetic field given by $\sqrt{\left(\Delta B^2\right)} \lesssim 2.5\mathrm{T}$. Were that bound to be achieved, the associated magnetic energy variation would exceed the local energy density in the convective motion by nearly a factor 10, a result which, as Jüri and his collaborators (e.g. these proceedings) have demonstrated, is not dynamically impossible. The strength of a tangled field would be yet greater.

The variation in cycle 23 was rather different from its predecessor. Basu and Mandel (2004) studied fourth differences (with respect to order n) of seismic frequencies, from which they claimed to have found the first evidence for structural changes with solar activity. In keeping with earlier discussions, they wrote that they believed the changes to be caused by a magnetic field, although they made no attempt to estimate its magnitude. Soon afterwards, Verner, Chaplin and Elsworth (2006) obtained a qualitatively similar result from raw frequencies of low-degree modes. The magnitude of the frequency variations imply $\sqrt{(\Delta B^2)} \simeq 10$T, assuming the field to be predominantly horizontal. There has been much said and much written about the anomalies of the last solar cycle – or at least about the long delay between its decline and the onset of the new cycle. Here is another difference, although, because we have no pertinent seismic data prior to cycle 22, we do not know whether it indicates an anomaly in cycle 23 or one in cycle 22.

An important consequence of these investigations is that the quite substantial temporal variations of the oscillatory component of the seismic frequencies must be taken into account when trying to infer helium abundance. That was not done in the model calibrations by Houdek and myself discussed in §2, nor in the original calibrations of the HeII hump (e.g. Däppen *et al.*, 1988). It appears, therefore, that Y has been underestimated (and with it Z), implying that the sun is closer to typical standard models than we have recently surmised.

7. Deep meridionial circulation and magnetic field

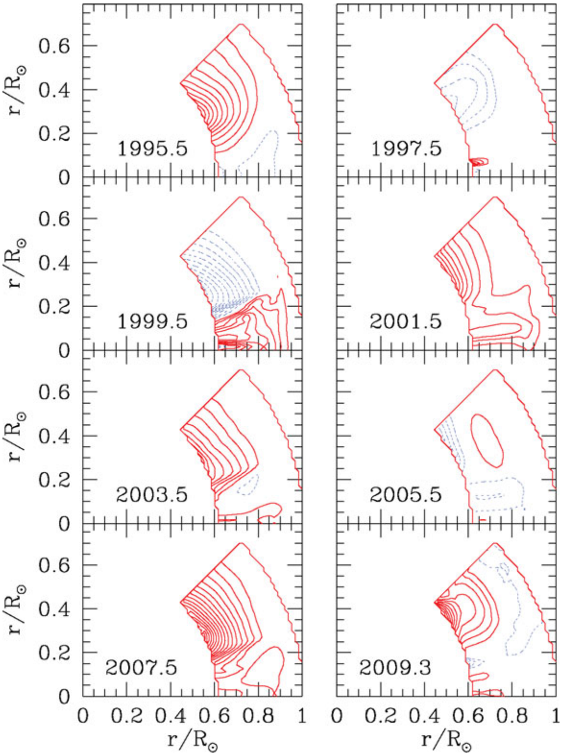

Figure 1. Lines of meridional magnetic field in a quadrant of the sun at different epochs as inferred by Antia, Chitre and Gough (unpublished) from GONG and SOI/MDI helioseismic data ignoring advection by the meridional flow. Continuous curves indicate anticlockwise field loops, dashed curves clockwise.

Direct helioseismological measurement of deep meridional flow is difficult because the effects on global seismic modes of north-south advection – or indeed the effects of any radial flow or zonal flow with zero longitudinal average – do not perturb the frequencies to leading order in the local Doppler shift. Flow in the outermost layers of the sun can be detected by localized Doppler measurements of high-degree waves that are damped within a circumundulation time and do not cohere to form standing waves; but deeply penetrating modes live longer, and cannot be analysed in that way. One must adopt a procedure for measuring the distortion of the eigenfunctions, either from leakage under projection onto putative undistorted wave forms (Schou *et al.*, 2009), variations in temporal phase (Gough and Hindman, 2010), or directly by some technique such as telechronoseismology (e.g. Duvall and Hanasoge, 2009); no valid seismic detection has yet been reported.

The interior magnetic field is also difficult to measure with confidence, because corresponding to any (at least axisymmetric) magnetic configuration is an isospectral density and sound-speed configuration (Zweibel and Gough, 1995). It is necessary to augment the seismic data with nonseismic information, or assumption, to draw any inference. For example, it is hardly possible for the solar-cycle HeII-glitch variation discussed in the previous section to have been produced by a thermal anomaly, and certainly not by a change in the chemical composition or the equation of state.

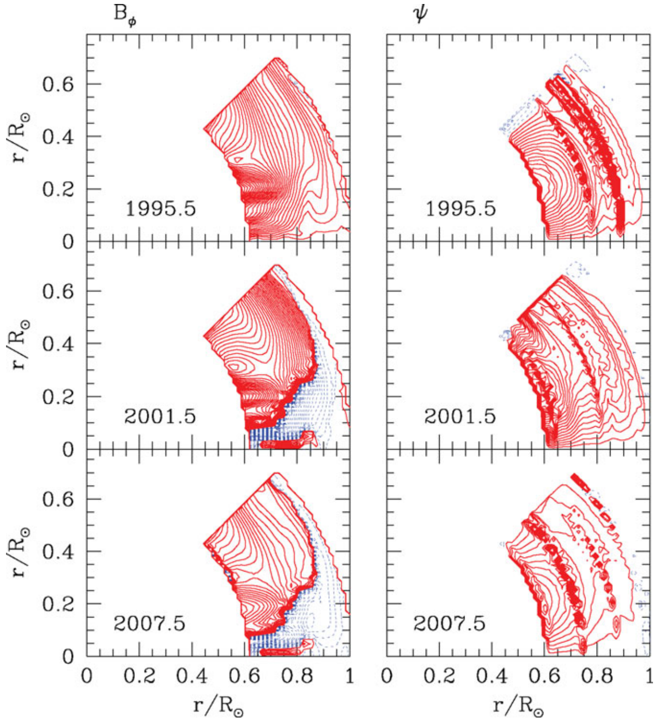

Figure 2. The right panels depict first iterates to determine the streamlines of meridional flow associated with a selection of the magnetic-field configurations illustrated in Fig. 1; continuous curves indicate anticlockwise flow, dashed curves clockwise. Panels on the left depict contours of constant B_ϕ, separated by 0.02T, such that continuous curves circle the axis of rotation positively, dashed curves negatively.

I conclude my discussion by reporting briefly on an indirect procedure currently being carried out by Antia, Chitre and myself to use seismically determined angular-velocity variations to infer the axisymmetric component of a putative magnetic field and associated meridional flow. By necessity the procedure is fraught with assumption, but we believe that it points to a way forward. The idea is to find that magnetic field and meridional flow that are consistent with magnetic induction, assuming, for the moment, that the angular-velocity variations are produced entirely by Maxwell stresses. Thus the meridional components of the momentum equation and the thermal energy equation –

the difficult equations that are at the centre of convection and dynamo theory – are not addressed, but are instead replaced by seismic observations of the angular velocity $\Omega\,(r, \theta, t)$, and, to provide boundary conditions for the analysis, direct measurements of the flow and the line-of-sight magnetic field in the photosphere. I refrain from burdening you with the details of how we are doing it, except to say that we are progressing slowly from a procedure with many assumptions which we wish to shed one by one, or, failing that, whose influence on the robustness of the results we shall try to ascertain. To date we have ignored microscopic and turbulent diffusion of magnetic field and momentum.

Our first experiment was to ignore meridionial flow entirely, and determine merely the field that produces a Maxwell stress compatible with the angular-velocity variation. Results are illustrated in Fig. 1. They extend from the declining phase of cycle 22 essentially to the present. We have not yet succeeded in obtaining the field close to the poles.

In Fig. 2 we present a few examples of the meridional flow associated with that field, which we estimated as a linear perturbation, ignoring its distorting effect on the field. One might note with cautious interest that at some latitudes the flow reverses beneath the depth at which direct seismic probing has yet been possible. But we warn that even the solution of this simple idealization is not yet complete. One might note that the azimuthal component B_ϕ of the magnetic field reaches values of a few tenths Tessla, which is much lower than the values inferred from the seismic frequency variations reported by Basu and Mandel, and Verner, Chaplin and Elsworth. We are now trying to learn how to iterate to a fully consistent solution in which the field is properly advected by the flow. Maybe, if we ever get close enought to reality, we'll be able to address some aspects of the superb simulations that Jüri and his colleagues have produced.

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Discussion

THOMPSON: You talked about the 700s oscillatory signal in the $\nu_{\max} - \nu_{\min}$ solar-cycle variations, and said that a diffuse magnetic field in the HeII ionization zone would dilute the signal of the HeII glitch. So what is the phase of the oscillation?

GOUGH: Because magnetic field dilutes the acoustic glitch, the phase of the oscillatory variation from sunspot minimum to sunspot maximum deviates from the phase of the mean signal, as depicted by Verner, Chaplin and Elsworth, by π. It would deviate likewise from the fourth differences plotted by Basu and Mandel, were it not for the frequency variation of the amplitude of the signal which produces an additional small deviation. This phase is consistent with that of the variations reported by Basu and Mandel, and Verner, Chaplin and Elsworth.

HILL: Have you estimated the magnitude of the meridional flow as a function of r?

GOUGH: Not really. It would be dangerous to make inferences from an unconverged iteration (although it does appear that the velocity increases with depth immediately beneath the photosphere). I showed the picture of that iteration merely to whet my (and, I hope, others') appetites.

ZWEIBEL: Would the convection-zone magnetic field affect the stratification and thereby the baroclinic terms in the equation for Ω, producing evolution of Ω in addition to that produced by Maxwell stresses?

GOUGH: The magnetic field may have a significant influence on baroclinicity in the upper layers of the convection zone – perhaps the outer 15% by radius – where the density is relatively low, thereby adding to the complexity of the dynamics of the meridional flow which advects Ω. One should not forget that anisotropic Reynolds stresses are no doubt also important. As you know, our investigation is in a very early stage, and so far we have ignored those processes; but they are on our list of matters that we intend to investigate. Indeed, the purpose of our exercise is not merely to produce field and flow configurations that might plausibly reflect those in the sun, but primarily to understand the mechanisms that generate them.

TOOMRE: You side-stepped the heavy-element issue by saying that the low Z values deduced from surface observations appear to gradually increase with recent reanalyses. So possibly there is no real problem with the theoretical structure models.

GOUGH: At the moment it is difficult, even for the executors, to judge the accuracy of abundance 'determinations'. Yes, I did point out that reported values of Z have tended to increase with time since Martin Asplund's first announcement, but that was due mainly, although not entirely, to new independent investigators entering the fray. On the whole we believe that the precision of scientific measurements increases with the passing time, and we hope that the accuracy does too. However, the latter is not always the case; indeed, Martin Asplund's original work in this area exemplified that. I suspect that the value of the photospheric Z will settle down soon, and that it will end up being lower than the value of Z that seismic models elaborated with the tenets of standard stellar-evolution theory require of the radiative interior.