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THE QUASI-BIENNIAL OSCILLATION (QBO): SOME POINTS ABOUT THE TERRESTRIAL QBO AND THE POSSIBILITY OF RELATED PHENOMENA IN THE SOLAR INTERIOR.

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Introduction

As Professor Holton has reminded us (*q.v.*, this Proceedings), the terrestrial quasi-biennial oscillation or QBO is a remarkably persistent phenomenon. It has been clearly seen in tropical meteorological radiosonde data since the early 1950s, and there is indirect but persuasive evidence that it has been present in varying degrees since at least the 1880s (*e.g.* Hamilton 1983; Hamilton and Garcia 1984,6; Hagan *et al.* 1993; Teitelbaum *et al.* 1994). I suggest we postpone the interesting question of the possible modulation of solar-terrestrial effects by the terrestrial QBO until after the presentation by Professor Labitzke. I thought, however, that it might be interesting to consider one or two very fundamental points about the fluid dynamics of the terrestrial QBO and its robustnesses and sensitivities, and then to consider briefly the possibility that some aspects of solar variability — some of the many aspects that we have been hearing about — might be related to partially similar mechanisms in the Sun's interior.

There is a wider motivation as well. It seems overwhelmingly likely that certain facts now established about the fluid dynamics of the Earth's stratosphere will, in one way or another, turn out to have direct relevance to understanding the fluid dynamics of the Sun's interior, regardless of whether QBO-like phenomena exist there. The Earth's stratosphere and the Sun's interior are dynamically similar systems to the extent that both are rapidly rotating, with strong Coriolis effects, and both are stably stratified with large values of the fundamental dynamical aspect ratio, Prandtl's ratio $N/2\Omega$. Here Ω is a typical angular velocity, and N a typical buoyancy frequency of the stable stratification, or Schwarzschild–Milch–Hesselberg–Brunt–Väisälä frequency. We have

$$N/2\Omega \sim 10^2 \ , \tag{1}$$

numerically about 1.5×10^2 in the Earth's stratosphere and about twice that value in the outer parts of the present-day Sun's radiative interior, the buoyancy periods $2\pi/N$ being about five minutes and one hour respectively.

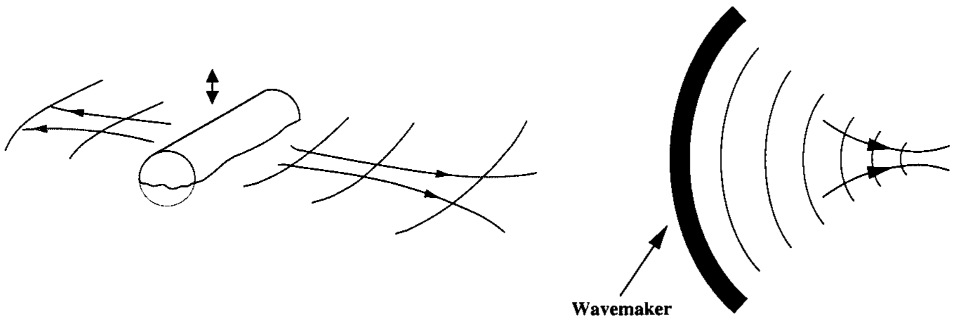
There is promise here of new keys to understanding primeval spindown and lithium depletion in the Sun and other stars. For instance, observation and numerical modelling of the dynamics and chemistry of the Earth's extratropical stratosphere have confirmed the likely effects of so-called 'geostrophic' or layerwise-two-dimensional turbulence. In particular, such turbulence has no tendency to produce shellular solid rotation. I shall explain why, then argue that if the outer parts of the Sun's radiative interior away from the poles are actually nearer solid rotation than is the overlying convection zone — a state of things indicated by current helioseismic results (*e.g.* Goode *et al.* 1991) — then there has to be a sufficiently strong poloidal magnetic field $\mathbf{B}_{\mathrm{p}}$ in the interior. This is consistent with the observed north–south asymmetry at the Maunder Minimum (*e.g.* Nesme–Ribes *et al.* 1994). The suggestion of such an interior field is hardly new in itself; and reasonable estimates of magnetic torsional periods range all the way down to tens of years or less (*e.g.* Gough 1990 & refs.). Layerwise-two-dimensional turbulence could still be present, affecting the transport of atomic species including lithium (Spiegel and Zahn 1992, Zahn 1992, & refs.) and possibly having weak dynamo action as well.

Again, observation and numerical modelling of the Earth's stratosphere have repeatedly confirmed the importance of Rossby waves. In dynamical systems of the kind we are dealing with, Rossby waves tend to occur whenever layerwise-two-dimensional turbulence occurs; and Rossby waves can be powerful transporters of angular momentum, having a ratchet-like, robustly one-signed effect. This has long been recognised as important in the Earth's extratropical winter stratosphere, and there has been a suspicion that Rossby waves of extratropical origin also play a part in the QBO. That suspicion is now being confirmed by, among other things, the recent recognition of a new form of Rossby-wave breaking that is always important in some latitude band surrounding the equator. The same phenomenon can occur in the Sun also, and could have formed an internal equatorial channel for the vertical leakage of lithium and other atomic species. Professor Gough and I are looking into this. It is worth noting that Rossby waves originating near the polar tachocline, from nonlinear baroclinic or other local shear instabilities (*e.g.* Thorncroft *et al.* 1993 & refs.), would have a robust tendency to propagate not only downward but also equatorward for the same reasons that they propagate upward and equatorward in the Earth's winter stratosphere. The one-signedness is in the right sense to help explain, also, the puzzling helioseismic result about the sign of the vertical differential rotation below the equatorial convection zone.

An important modelling failure

What is our most secure piece of knowledge about the mechanism of the terrestrial QBO? There is a clear answer: the terrestrial QBO is a wave-driven mean flow. The alternating deficits and excesses of angular momentum observed in the tropical stratosphere, reversing sign at roughly 14-month intervals — the full cycle time being variable between about 24 and 36 months — owe their existence and timing to the irreversible transport of angular momentum associated with the generation, propagation and dissipation of internal waves, of one sort or another, in the Earth's atmosphere.

There is little room for doubt about this. It is not only that many suitable types of internal waves are available: the atmosphere is observed to be full of internal gravity waves, inertia-gravity waves, Rossby waves and various other hybrid, buoyancy–Coriolis types including equatorially-trapped Rossby–gravity and Kelvin waves (*e.g.* Gill 1982). It is not only that transport of momentum and angular momentum is a robust and almost inevitable accompaniment to wave propagation in fluid media, as is well known and as was illustrated by the simple experimental demonstration at the Workshop (Fig. 1)



(a) *(b)*

Fig. 1. Simple experiments with 5 Hz water waves, illustrating the irreversible wave-induced momentum transport that accompanies the generation of waves in one place and their dissipation in another. The strong mean flows thereby generated can be made conspicuous by sprinkling a little powder such as chalk dust on the surface of the water. Configuration (*a*) works well with a cylindrical wavemaker whose diameter is of the order of 2 to 4 cm, and can be demonstrated in a transparent dish on an overhead projector. Configuration (*b*) works well with a larger wavemaker whose radius of curvature is of the order of half a metre. From McIntyre and Norton (1990) and McIntyre (1992,3); *q.v.* for further discussion.

— noting also that, as hinted above and discussed in two recent reviews (McIntyre 1992,3), wave-induced angular momentum transport is now recognised to be of primary importance for understanding almost all global-scale aspects of the Earth's atmospheric circulation including those aspects that depend on what used to be called 'negative eddy viscosity'. It is not only that the internal waves usually thought to be responsible for the QBO, which are hybrid upward-propagating equatorially-trapped waves, have been observed to exist in the real atmosphere and in some cases to have amplitudes of the right order of magnitude. This is often true, at least, of the equatorial Kelvin waves (e.g. Wallace and Kousky 1968, Kousky and Wallace 1971, Ziemke and Stanford 1994 & refs.). It is also, above all, the clear failure of serious, highly skilled and intelligent attempts to model the QBO *without* invoking wave-induced angular momentum transport that compels us to deduce that the QBO is a wave-driven mean flow.

This modelling failure is an example of an important negative result in science. I am thinking especially of the work of Wallace and Holton (1968), which Professor Holton, with typical modesty, has not emphasised. There, an attempt to model the QBO in a dynamically self-consistent manner — consistent also with the known orders of magnitude of relevant and well-understood physical processes such as radiative heat transfer, and consistent with available observational estimates of eddy fluxes of angular momentum — showed that the observed angular momentum changes could not be explained without assuming the existence of a mysterious additional eddy flux of angular momentum. This had to be quite unlike the fluxes suggested by the observational estimates. It also had to be quite unlike any flux by quasi-diffusive processes such as the eddy viscosity often assumed, rightly or wrongly, to be associated with unresolved fluctuating motions.

The convergence of this additional eddy flux of angular momentum had to correspond to a time-dependent, axisymmetric mean torque, acting over a certain fairly narrow range of altitudes, a few kilometres, that systematically descended as time progressed. The sign of the torque had to be such that it systematically tended to reinforce, not reduce, the local angular momentum anomaly. If one tried to describe it in terms of eddy viscosity one would be compelled to admit nonsensical negative and infinite values. The descent had to begin at altitudes around 30km and end around 15 km, the same pattern being repeated with opposite sign and some temporal overlap and the whole cycle taking, of course, about 24 to 36 months. The time-altitude contour plot of the torque had to look a bit like half a sunspot butterfly pattern, but with thinner 'wings'.

Despite being unable to say what physical process or processes might produce such a time-dependent mean torque, Wallace and Holton reported their negative result, concluding that the torque must exist despite its strange character and despite the lack of any direct observational confirmation — but also bearing in mind that the available observational estimates were based on sparse radiosonde data and therefore tentative. It was only subsequently recognised (Lindzen and Holton 1968, Holton and Lindzen 1972; see also *e.g.* Plumb 1977, Saravanan 1990) that exactly this qualitative kind of mean torque, with exactly this qualitative kind of time-dependence, can be expected, on theoretical grounds, to result from wave-induced angular momentum transport. This is because of the refractive effect of the differential rotation associated with the QBO upon internal wave propagation, giving rise to a crucial feedback.

The differential rotation selectively refracts and Doppler-shifts the internal waves in much the same way as an ordinary bathplug vortex selectively refracts and Doppler-shifts randomly incident surface waves (tending to deceive the eye as to the direction of rotation). In the tropical stratosphere, under plausible assumptions about the types of internal waves and how they might be dissipated, the net effect turns out to be a time-dependent mean torque whose spacetime pattern is qualitatively the same as the required thin-half-butterfly pattern, with the correct phase and sign.

Mean upwelling in the Earth's tropical stratosphere

With hindsight, one can add that if Wallace and Holton had known in 1968 what we know today about upward mean motion in the tropical stratosphere, then their conclusion would have been further strengthened. For reasons connected with the stratosphere's response to *extratropical* wave-induced angular momentum transport, which latter is dominated by Rossby waves and related eddy motions, there is a persistent global-scale upwelling or upward mean motion in the tropical stratosphere. It is of the order of a fraction of a millimetre per second, or 10 km per year, in the altitude range of interest. This can be taken as a fact well established both directly from many observations — including global-scale, space-based spectroscopic observations of long-lived chemical tracers — and indirectly from strong lines of theoretical argument and numerical modelling as discussed, for instance, in the two recent reviews I have mentioned and in the many publications cited therein. See especially the remarks about the consequences of the Earth's rotation and the one-signedness of Rossby-wave dynamics. The extratropical stratosphere acts as a gentle but persistent one-way, global-scale pump, extracting air from the tropical stratosphere and feeding it down into the extratropical troposphere. This pump acts persistently all year round, despite some seasonal variability (Yulaeva *et al.* 1994). To satisfy mass continuity, the circulation has to be completed by mean upwelling in the tropical stratosphere. The resulting adiabatic temperature tendencies are balanced by infrared radiative heat transport at low to moderate opacity. The net effect is qualitatively like Newtonian cooling to space; in this respect the stratosphere is more like the Sun's photosphere than the Sun's interior.

One consequence of the mean upwelling in the Earth's tropical stratosphere is that the torque required to account for the QBO must still conform to the thin-half-butterfly spacetime pattern but, in the presence of the upwelling, must have a greater magnitude than would otherwise be required. The torque now has to overcome the upward advection of angular momentum, represented by the $\bar{w}\,\partial/\partial r$ term in the Eulerian angular momentum equation where $\bar{w}$ is the mean upwelling velocity. The numerical magnitudes imply a torque greater by nearly a factor 2 than would be required in the absence of upwelling. From a Lagrangian viewpoint, the persistent upward motion of the air through the (descending) range of altitudes where the torque is significant shortens the time available to accelerate a given ring of air. It shortens the time by nearly a factor 2 because, by chance, the upwelling velocity has nearly the same magnitude as the typical descent velocity of the thin-half-butterfly pattern.

Model studies of the effects of mean upwelling on the QBO can be found for instance in the papers by Saravanan (1990) and Dunkerton (1991). Saravanan points out that the much stronger mean upwelling in the tropical troposphere can explain why the QBO is observed to be confined to the stratosphere.

Wave dissipation, Doppler shifting and the 'critical-layer effect'

It seems likely that the internal waves that drive the QBO in the tropical stratosphere dissipate mainly by infrared radiative damping, or wave breaking, or both. The notion of 'wave breaking' needs to be understood in a suitably generalised way, relevant to its part in making the wave-induced momentum transport irreversible (McIntyre and Palmer 1984,5). The waves of interest could include almost any non-acoustic internal waves. Many such waves propagate slowly enough to be Doppler shifted significantly by the differential rotation, whose characteristic relative velocities are of the order of tens of metres per second. Such waves become increasingly vulnerable to breaking and other forms of dissipation whenever the differential rotation Doppler-shifts them toward small intrinsic frequencies and group velocities. This is the relevant aspect of what is sometimes, for historical reasons, called the 'critical-layer effect', but which could more plainly be called wave dissipation enhanced by Doppler shifting, more specifically Doppler downshifting. Call it what we may, it is an important part of the feedback that gives rise to the QBO.

(There is a critical-layer *myth*, still occasionally found in the literature, saying or tacitly assuming that there is a separate critical-layer effect independently producing convergence of the wave-induced angular momentum flux, additional to the convergence produced by the various forms of wave dissipation. However, this comes from too literal an interpretation of linearised steady-state wave theory. For real, finite-amplitude waves the real 'critical-layer effect', wave dissipation enhanced by Doppler shifting, represents merely one subset — albeit an important subset — of the circumstances in which the waves must dissipate.)

If the Doppler shift goes far enough toward small group velocities while dissipation remains negligible, then the waves will inevitably break, however small their initial amplitude may be. Linearised, nondissipative wave theory predicts its own failure, by predicting a pileup of wave activity. In this respect, a real critical layer behaves like the surf zone of an ocean beach. The edge of the beach corresponds in the simplest examples to the place where the group velocity of the internal waves is Doppler shifted to zero. In the absence of other dissipation mechanisms, waves of non-infinitesimal amplitude must break *before* reaching this notional edge. How long before reaching it depends on the amplitude. These points were well explained in a useful review by Fritts (1984, Fig. 9), for the case of ordinary internal gravity waves in the usual, large-Richardson-number parameter régime, to which these ideas apply in their simplest form. For Rossby waves, the surf-zone analogy still applies, with some qualifications. There can be more backscattering — Rossby-wave surf zones tend to be more reflective than ocean beaches — and there is no simple characterisation of edge position, which is more sensitive to wave amplitude and to details of the differential rotation than to group-velocity values.

Uncertainties about the terrestrial QBO

Even though we can be practically certain that the QBO is wave-driven, there is great uncertainty about exactly which types of waves are most important. It was suggested by Dickinson (1968), and strongly argued by Andrews and McIntyre (1976) on the basis of a radiosonde data study by Lindzen and Tsay (1975) that, in addition to the upward-propagating equatorial Kelvin and Rossby–gravity waves traditionally considered, there could also be a significant contribution from planetary-scale Rossby waves, coming in obliquely or nearly horizontally from the extratropical winter hemisphere.

The possibility of driving a realistic-looking QBO entirely by such Rossby waves together with an equatorial Kelvin wave, and with no other waves at all, was demonstrated in an idealised QBO model simulation by Dunkerton (1983). The Rossby waves were assumed to provide a retrograde torque near the equator, and the Kelvin wave a prograde torque. This also proved to be one way of producing space-time patterns of torque and differential rotation more realistic than traditional simulations using Kelvin and Rossby–gravity waves alone, albeit not the only way (*e.g.* Plumb and Bell 1982, Saravanan 1990). However, while the Kelvin wave is confined near the equator and has long been known to be able to dissipate there in an appropriate way, both by infrared radiative damping and by wave breaking (Kousky and Wallace 1971), it was less clear how the Rossby waves might dissipate in suitable locations near the equator.

It therefore seems significant that recent work on 'asymmetric inertial instabilities' (Ciesielski *et al.* 1989, O'Sullivan and Hitchman 1992, Dunkerton 1993, Clark and Haynes 1994) has produced theoretical and observational evidence for the existence of a previously unrecognised form of Rossby-wave breaking that depends on smallness of the Coriolis parameter and is confined to some neighbourhood of the equator. The latitudinal width of that neighbourhood is of the same order as the latitudinal particle displacement amplitude of the Rossby waves. Although quantitative estimates have yet to be made, a significant contribution to the retrograde torque is indicated by order-of-magnitude considerations, applied to Rossby-wave theory together with wave–mean interaction theory expressed in terms of the Rossby–Ertel potential vorticity (see eq. (8)ff. below, also McIntyre and Norton 1990 & refs., McIntyre 1993 & refs., Bühler *et al.* 1994). A good qualitative understanding is available from these theories, and it is clear in particular that there is a robust tendency for real, finite-amplitude Rossby waves to produce a retrograde torque when they dissipate — an aspect of the 'one-signedness' already alluded to.

In any case, the work of Lindzen and Tsay (*op. cit.*), and a large range of observational and modelling studies since then (*e.g.* Takahashi and Boville 1992 & refs.), have been suggesting more and more clearly that some such extra contribution is needed to account for the required torque during the phases of the QBO when the torque has to be retrograde. The observed equatorially-trapped waves to which the retrograde torque is usually attributed, the Rossby–gravity waves, typically seem to be too weak and too sporadic to provide all the torque, even before allowance is made for the effect of mean upwelling. Besides this, the newly-recognised inertial-instability-related form of wave breaking near the equator is practically certain to affect Rossby–gravity waves, tending to limit their amplitudes regardless of source strength. This is because, like Rossby waves, Rossby–gravity waves involve latitudinal particle displacements. Unlike the breaking of Kelvin waves, which resembles the breaking of ordinary internal gravity waves and has a fairly definite finite-amplitude threshold — at least for idealised theoretical models of Kelvin waves with zero latitudinal particle displacements — the inertial-instability-related breaking of Rossby and Rossby–gravity waves can be expected to take place at all wave amplitudes and at all altitudes. It should not switch off below a threshold amplitude, but should merely be confined nearer the equator. This may tend to prevent large-amplitude Rossby–gravity waves, and indeed other equatorially trapped waves apart from the Kelvin wave, from propagating very far in the vertical, while not, of course, preventing Rossby waves of extratropical origin from reaching the neighbourhood of the equator by propagating obliquely or horizontally.

There is also great uncertainty about the contribution from vertically-propagating high-frequency gravity waves, although vertical soundings by radars and lidars in the tropics might, in the coming few years, tell us more about this contribution, by observing the waves directly, and similarly for the equatorially-trapped waves. For all these wave types there is extreme uncertainty, in addition, about wave generation mechanisms and wave source strengths. The sources all involve nonlinear fluid dynamics, among other things the dauntingly complex, multi-scale phenomena associated with large ensembles of tropospheric cumulonimbus convection cells. To quote briefly from my 1993 review — and this applies to all the relevant wave types — "we have even less knowledge, either observational or theoretical, of possible wave generation mechanisms. The real QBO seems to involve highly complicated, chaotic, nonlinear wave generation processes (mainly in the troposphere, both tropical and extratropical) to which there is a robustly non-chaotic response in the tropical stratosphere."

The latitudinal scale of the terrestrial QBO

The angular momentum changes associated with the terrestrial QBO are observed to take place mainly within a band extending to about 10 to 15 degrees of latitude to either side of the equator. Let us say that the width of this band is something like π times the latitudinal length *scale*, so that the scale itself is of the order of 10^3km. This localisation of the angular momentum changes, together with our ignorance, until recently, of the abovementioned inertial-instability-related form of wave breaking, may have been part of the reason why many researchers interested in the QBO seem to have paid little attention to wave types other than the equatorially-trapped Kelvin and Rossby–gravity waves, despite the increasingly conspicuous problems with Rossby–gravity wave amplitudes.

The equatorially-trapped waves have latitudinal scales that depend on the Doppler shift but, fairly typically, are such that the waveguide structure fits within the same 10-to-15-degree latitudinal band. When one looks more carefully at the effects of Doppler shifting toward small intrinsic frequencies, and considers what is now known about wave breaking, one gets somewhat narrower latitudinal scales on both counts; but then one can reasonably call on processes like sideways or layerwise-two-dimensional shear instability to broaden the scale, giving something like that observed. So it may still be tempting to assume that dominance by equatorially-trapped waves is part of the reason why the angular momentum changes occur mainly in the 10-to-15-degree latitude band around the equator, and not in a band of broader latitudinal scale.

However, in an important recent investigation Haynes (1994) has thrown new light on the problem by demonstrating that the terrestrial QBO could not, in any case, occur in a band of much broader latitudinal scale, no matter how broad the latitudinal distribution of wave-induced torque. The localisation of the angular momentum changes to a 10-to-15-degree latitude band around the equator can be accounted for purely in terms of the dynamical and thermal nature of the axisymmetric response to an axisymmetric wave-induced mean torque, in a spherical stratified rotating system having large Prandtl's ratio (1) and large Richardson number. This removes any possible basis for confining attention to equatorially-trapped waves alone. Such waves may still of course contribute; and the importance of equatorially-trapped Kelvin waves, in particular, is not in question.

Haynes' results show in detail how the different characters of the tropical and extra-tropical responses to wave-induced angular momentum transport fit together. Consider the axisymmetric response to a given time-dependent torque, of shallow vertical scale D, say, and broad latitudinal scale comparable to the radial distance R to the centre of the Earth or the Sun. Thus the torque might be proportional, for example, to something like the cosine squared of the latitude. Its time variation is assumed to be characterised by the timescale τ_{torque}, let us say quasi-periodic with period of order $2\pi\tau_{\text{torque}}$, so that for the terrestrial QBO τ_{torque} is several months. The temperature and buoyancy perturbations associated with the evolving differential rotation, taken to be in hydrostatic and cyclostrophic balance, are subject to a thermal relaxation process characterised by timescale $\tau_{\rm thermal}$. In the relevant altitude range in the Earth's tropical stratosphere, $\tau_{\rm thermal}$ can be taken to be of the order of weeks. Because of the stratosphere's low infrared opacity, already mentioned, $\tau_{\rm thermal}$ is less strongly dependent on vertical scales of temperature variation than the diffusive thermal relaxation found in the Sun's interior, going more like $D^{1/2}$ than D^2 for vertical scales of order D (Fels 1984, Haynes and Ward 1993). For the terrestrial QBO, then,

$$\tau_{\rm thermal}/\tau_{\rm torque} \sim 10^{-1}\ . \tag{2}$$

Define

$$\widehat{D}^2 \ = \ \min\left(D^2,\ HD\right)\ , \tag{3}$$

where H is the e-folding pressure scale height, $\sim$ 7km for the Earth's stratosphere and $\sim 10^5$km for the outer parts of the Sun's interior. Then the unsteady changes in differential rotation are found to be localised to a band around the equator with latitudinal scale

$$L_{\rm lat} \ \sim \ \left(\frac{N\widehat{D}R}{2\Omega}\right)^{1/2} \left(\frac{\tau_{\rm thermal}}{\tau_{\rm torque}}\right)^{1/4} \ . \tag{4}$$

For the terrestrial QBO, the second factor in this expression is about 0.6, from (2). The quarter power makes it insensitive to the exact timescales assumed. From (1)ff. and with $\widehat{D} \sim D \sim$ 4km and $R \sim$ 6000km we have, consistent with what is observed, $L_{\rm lat} \sim$ 1000km.

Further poleward, the response to the torque consists mainly of the extratropical 'pumping' action already mentioned. There the essential feature, which depends on the Earth's rapid rotation, is a poleward or equatorward flow whose Coriolis force comes into balance with the applied torque on a timescale less than $\tau_{\rm torque}$. For a retrograde torque, such as that due to dissipating Rossby waves, the flow is poleward; this is what occurs in the real extratropical stratosphere, as already mentioned, although there we should take $2\pi\tau_{\rm torque} = 1$ year. The two regimes merge smoothly into one another near latitudes of order some number of order unity times $\pm L_{\rm lat}/R$ radian. All these results can be made plausible via scaling considerations related to those given in Professor Holton's paper in this Proceedings. Haynes' very thorough investigation includes, also, precise quantitative modelling using both analytical and numerical techniques.

Could there be a QBO-like phenomenon in the Sun's interior?

Since, as (1) reminds us, the Sun's radiative interior is fluid-dynamically very like the Earth's stratosphere, the question that inevitably arises is whether any of the observed unsteadinesses in solar properties are connected, directly or remotely, with QBO-like phenomena in the Sun's interior. To be sure, dynamos can exhibit unsteadinesses by themselves, but is that the whole story?

In the relatively opaque solar interior, τ_{thermal} becomes simply a thermal diffusion time,

$$\tau_{\text{thermal}} = D^2/\kappa \ , \tag{5}$$

where a typical thermal diffusivity would be $\kappa \sim 10^7 \text{cm}^2\text{s}^{-1}$. Thus, with $\widehat{D} \sim D \lesssim H$, (4) is replaced by

$$\frac{L_{\text{lat}}}{D} \sim \left(\frac{NR}{2\Omega}\right)^{1/2} (\kappa\tau_{\text{torque}})^{-1/4} \ . \tag{6}$$

With $R \sim 5 \times 10^5$km we have $NR/2\Omega \sim 1.5 \times 10^8$km. If we take $2\pi\tau_{\text{torque}} = 22$ years, then $\tau_{\text{torque}} \sim 10^8$s, so that $(\kappa\tau_{\text{torque}})^{1/2} \sim (10^{15}\text{cm}^2)^{1/2} \sim 3 \times 10^7\text{cm} \sim 3 \times 10^2$km, whence $L_{\text{lat}}/D \sim 7 \times 10^2$. If we take $2\pi\tau_{\text{torque}} \sim 2000$ years, then L_{lat}/D is only a factor 3 smaller, that is, $L_{\text{lat}}/D \sim 2 \times 10^2$.

If we take this at face value, there are two possibilities. The first is that the phenomenon is very shallow, of order 10^3km or less, in which case, if it is also very close to the Sun's centre or to the base of the convection zone, then N might be an order of magnitude smaller hence L_{lat}/D several times smaller, which, however, does not change the picture very much. The second possibility is that the phenomenon is significantly deeper, in which case the whole of the Sun's interior is, for present purposes, dynamically like the Earth's tropics. QBO-like phenomena would be possible on a broad latitudinal scale if suitable waves were present. More precisely, if the vertical scale D is significantly greater than the scale implied by substituting R for L_{lat} in (6), then there is little high-latitude pumping action like that operating in the Earth's extratropical stratosphere. Such pumping is then largely choked off by the stable stratification in combination with the far higher opacity to heat transport implied by typical values of κ. In other words thermal diffusion, though possibly crucial to wave dissipation (Gough 1977, Press 1981) can be altogether neglected in the dynamics of the differential-rotation changes. The latter can then be modelled simply as a frictionless, time-varying adiabatic response to the time-varying torque.

The next question is whether suitable waves are excited at sufficient amplitude. In fact there are two questions, first what amplitudes might be needed to drive some kind of QBO-like phenomenon, and second whether sufficiently strong wave sources might actually exist. I now argue that the answers to both questions turn critically upon the interpretation of recent helioseismic results, which give a new twist to the story.

Some implications of recent helioseismic results

As hinted earlier, the helioseismic evidence (*e.g.* Goode *et al.* 1991, Dziembowski and Goode 1993) suggests that the actual differential rotation of the present-day Sun is remarkably different from what might have been expected from purely fluid-dynamical considerations. Specifically, the evidence suggests that

(a) the angular velocity Ω decreases with decreasing R below the base of the Sun's equatorial convection zone, and

(b) the outer parts of the Sun's radiative interior away from the poles are nearer shellular solid rotation than the convection zone; and the entire convection zone has approximately the same latitude-dependent Ω as the photosphere.

By shellular solid rotation I mean constancy of Ω on the nearly-spherical stratification surfaces $\mathcal{S}$, say, of the radiative interior. If the helioseismic inversions are correct to the extent that statements (a) and (b) are true, then there are far-reaching implications for the problem under consideration. I shall provisionally assume that (a) and (b) are both true and consider the implications.

Statements (a) and (b) are both, at first sight, very surprising indeed. They contradict expectations from any fluid-dynamically-credible stratified, rotating primeval spindown model. In particular, statement (b) cannot be explained as an effect of layerwise-two-dimensional turbulence, for the very good reason that layerwise-two-dimensional turbulence is potential-vorticity-transporting.

For present purposes, this puts it at an opposite extreme from the momentum-transporting turbulent viscosities usually postulated in models of the Sun's differential rotation (*e.g.* Mestel *et al.* 1988, Zahn 1992, & many refs.). The potential-vorticity-transporting character of real layerwise-two-dimensional turbulence is clear from many observational, theoretical and numerical-modelling studies of the Earth's stratosphere and similar fluid-dynamical systems. It represents another of our most secure pieces of knowledge about such systems (see for instance §§8–9 & pp.362–5 of my 1992 review);

and on top of this the whole picture is now being strongly confirmed by a wealth of new observational data, obtained as a result of concerns about stratospheric ozone-layer chemistry (*e.g.* Lahoz *et al.* 1993, Waugh *et al.* 1994, Manney *et al.* 1994, & many refs.).

The potential-vorticity-transporting character of layerwise-two-dimensional turbulence means that, if such turbulence has a significant effect at all, then the effect is to reduce large-scale gradients not of momentum, but of the scalar quantity Q known as the potential vorticity — more precisely the Rossby–Ertel potential vorticity (Rossby 1936, 1940; Ertel 1942) — on each stratification surface $\mathcal{S}$. Q values tend to be carried advectively along each surface $\mathcal{S}$ and hence to undergo two-dimensional chaotic advection and mixing. This is just what we routinely see happening in the Earth's stratosphere and in similar systems. Q may be defined by

$$Q = b^{-1}\boldsymbol{\omega}\cdot\mathbf{n} \ , \tag{7}$$

where $\boldsymbol{\omega}$ is the absolute vorticity vector, or curl of the fluid-dynamical velocity field in an inertial reference frame, and $\mathbf{n} = \nabla s/|\nabla s|$, the local unit normal to the surface $\mathcal{S}$, where s is any thermodynamic variable, such as specific entropy, that labels the stratification surfaces $\mathcal{S}$. These surfaces, by definition, are material surfaces when the motion is adiabatic. The scalar field b^{-1} is defined as $|\nabla s|$ divided by the mass density. It is a strictly positive quantity for a stably stratified system, as is its reciprocal b, which latter can be regarded as a sort of stratification-modified mass density. More precisely, $b\,ds$ is the mass per unit area between neighbouring stratification surfaces, so that if dA is the surface area element then $b\,dA\,ds$ is the mass element.

The notation has been chosen to make conspicuous the fact that the surface area integral of bQ over each closed, quasi-spherical surface $\mathcal{S}$ must vanish, by Stokes' theorem:

$$\iint_{\mathcal{S}} bQ\,dA \ = \ 0 \ . \tag{8}$$

The implication is that there is only one possible effect of strong layerwise-two-dimensional turbulence, if by strong we mean effective in eliminating the isentropic gradient of Q, *i.e.* the gradient of Q on the surface $\mathcal{S}$. The only possible such effect allowed by (8) is to make Q *zero* everywhere on $\mathcal{S}$, since b is strictly positive. The resulting state might be called 'shellular stagnation': it is a state having zero absolute angular velocity Ω everywhere on $\mathcal{S}$, apart from any small oscillations due to acoustic and other wave motions. It is the only state of shellular solid rotation that can be produced by layerwise-two-dimensional turbulence. It is a fantastically improbable state, for the real Sun's interior and the real Earth's stratosphere, if only because its realization would require implausibly large, or persistent, retrograde torques to have been applied to $\mathcal{S}$ by whatever was exciting the turbulence.

It can therefore be safely assumed that any layerwise-two-dimensional turbulence actually present cannot be strong in anything like the foregoing sense. It cannot greatly reduce the overall, pole-to-pole contrast in Q values. There are then only two likely situations. The first is that the turbulence is so weak or sporadic as to have a negligible effect on large-scale isentropic gradients of Q, hence a negligible effect on the distribution of Ω. The second is that the turbulence is stronger but inhomogeneous, effective in reducing isentropic gradients of Q but over limited regions only. The Earth's stratosphere, and numerical models of it, routinely and repeatedly provide examples of both situations. At solstice, between altitudes $\sim$ 30–40km, for instance, we see the first situation in the summer hemisphere, to good approximation, and we see the second situation in the winter hemisphere. The typical character of the fluid motion in the second situation is well illustrated in a recent numerical model study by Norton (1994).

It might well be asked where the implied torques come from, in this second situation. Though far smaller than the fantastically large torques that, as just shown, are part of the necessary conditions for exciting strong layerwise-two-dimensional turbulence, nevertheless the torques cannot generally be zero when layerwise-two-dimensional turbulence is effective. The answer has already been hinted at: the torques come from other altitudes or latitudes — which could be far away or nearby — via Rossby-wave-induced angular momentum transport or, if you prefer, Rossby-wave radiation stress, associated with propagating or diffracting Rossby waves. In particular, the surfaces $\mathcal{S}$ undulate gently but significantly, allowing pressure fluctuations to exert torques from other altitudes. Some of the consequences have already been mentioned in the case of the Earth, where for instance most of the retrograde torque on the stratosphere is exerted from the troposphere below in just this way.

The implied relation between layerwise-two-dimensional turbulence and Rossby-wave radiation stress is a fundamental reason why, in fluid-dynamical systems of the type we are dealing with, layerwise-two-dimensional turbulence tends to occur together with Rossby waves of one sort or another, as was suggested in the introduction. Indeed, it often makes sense to regard the layerwise-two-dimensional turbulence as having been excited by the breaking of Rossby waves, provided that 'breaking' is understood in a relevant and suitably generalised way, as already mentioned. The reader interested in the mathematical underpinning may consult my reviews and references therein to a large literature on 'vortex forces', 'potential-vorticity inversion', 'wave–mean interaction' and Rossby-wave critical layers, also Bühler *et al.* (1994). The resulting body of theory shows how the ratchet-like, robustly one-signed character of Rossby-wave-induced angular momentum transport and all its consequences are related to the likelihood that dissipative processes, including layerwise-two-dimensional turbulence, will locally reduce rather than increase the large-scale isentropic gradients of Q. This produces retrograde rather than prograde torques on the regions where the Rossby waves are being dissipated.

The expectation that any layerwise-two-dimensional turbulence that is effective will also be inhomogeneous, mixing Q on some regions of $\mathcal{S}$ but not on others, is reinforced by considering the nature of 'Rossby-wave elasticity'. This is the peculiar fluid-dynamical quasi-elasticity to which Rossby-wave propagation owes its existence, and against which work must be done if we want to excite Rossby waves or to initiate layerwise-two-dimensional turbulence. This quasi-elasticity depends on the magnitude of the isentropic gradient of Q, to which Rossby-wave intrinsic frequencies are proportional. If isentropic gradients of Q are reduced over some region of a stratification surface $\mathcal{S}$, then the gradients will tend to be increased in adjacent regions of the same $\mathcal{S}$. This is a positive feedback since the reduced gradients have weaker Rossby elasticity and facilitate further layerwise-two-dimensional mixing, while the increased gradients have stronger Rossby elasticity and inhibit such mixing. The Rossby elasticity is only part of the picture because it turns out that horizontal shear also has an important role; but the end result is exactly what the argument about positive feedback suggests, namely an extreme inhomogeneity with some regions dominated by undular, quasi-elastic behaviour adjacent to other regions dominated by layerwise-two-dimensional turbulence. The effects of this highly inhomogeneous 'wave-turbulence jigsaw puzzle' are seen, for example, in the various phenomena associated with the Antarctic ozone hole. There, the chemistry depends on the inhibition of layerwise-two-dimensional mixing by an undular, Rossby-elastic 'potential-vorticity barrier' marking the edge of the winter stratospheric polar vortex.

An immediate corollary is that homogeneous or weakly-inhomogeneous turbulence theory is inapplicable, indeed profoundly misleading for present purposes. The same conclusion can be reached by a careful consideration of the way in which homogeneous turbulence theory predicts *its* own breakdown, *e.g.* via Fjørtoft's theorem on 'upscale energy cascading', equivalently the well known tendency of nearby vortices to merge inelastically, seen in a whole range of theoretical models (*e.g.* Legras and Dritschel 1993, Dritschel 1993 & many refs.). The tendency toward inhomogeneity is very powerful, invalidating among other things the scale-separation, or weak-inhomogeneneity, assumption on which the usual notion of 'turbulent viscosity' is based.

What then is the main point? It is that layerwise-two-dimensional turbulence drives the system away from, not toward, shellular solid rotation. How then are we to make sense of statement (*b*) above? All laminar spindown models give latitude-dependent Ω in the Sun's interior, contradicting statement (*b*); and layerwise-two-dimensional turbulence will, if anything, make Ω still more strongly latitude-dependent, as in fact it does in the winter stratosphere. The clear implication is that some further physical effect must be pulling the outer parts of the Sun's radiative interior toward shellular solid rotation, except perhaps near the poles where helioseismology gives no information. There is one and only one obvious, and well known, candidate: a sufficiently strong poloidal magnetic field $\mathbf{B}_\mathrm{p}$.

The question of the origin of interior magnetic fields is outside the scope of this paper (see, *e.g.*, Mestel and Weiss 1987, & refs.). Suffice it to mention first that the usual argument against an interior $\mathbf{B}_\mathrm{p}$ from north-south symmetry of the sunspot butterfly pattern has been vitiated by the north-south *asymmetry* found at Maunder Minimum (*e.g.* Nesme–Ribes *et al.* 1994, this Proceedings), second that magnetic diffusivities are small enough to allow persistence of a primeval $\mathbf{B}_\mathrm{p}$ in a non-turbulent interior, as is well known, and third that if, on the other hand, layerwise-two-dimensional turbulence is significant in the interior, then helicity and dynamo action are possible through the enhanced magnetic buoyancy of field lines locally wrapped up by the turbulent eddies (E. A. Spiegel, personal communication). What can, with practical certainty, be said is that if statement (*b*) stands scrutiny, then the fluid-dynamical facts will compel us to accept the existence of the poloidal field $\mathbf{B}_\mathrm{p}$, whatever its origin.

The implications, which evidently include major implications for helioseismic inversion strategies as well as for our understanding of primeval solar spindown, will be discussed more adequately elsewhere (paper with Goode and Gough in preparation); but let me close with a very brief sketch. It will include the beginnings of an answer, of an unexpected kind, to the original question about the possibility of QBO-like phenomena.

$\mathbf{B}_\mathrm{p}$ lines as 'reinforcing rods', and helioseismic inversions

The picture we have arrived at has the $\mathbf{B}_\mathrm{p}$ lines of a well-contained interior field acting as if they were rigid 'reinforcing rods', to good approximation frozen into the fluid and spanning a substantial range of latitudes in the outer parts of the interior, albeit not necessarily as far as the poles. The 'reinforcing rods' may be twisted toward the toroidal, in a quasi-static way, by torques due to waves and turbulence. They are nonetheless effectively rigid, to the extent that they stop differential rotation, if we assume that magnetic torsional oscillations have been damped out. This last assumption has a strong theoretical justification, to be mentioned below; and the picture is fully consistent with statement (*b*). It tells us, furthermore, that the differential rotation throughout most of the interior must conform not to simple shellular solid rotation but to what might be called toroidally-shellular or doughnut-shellular solid rotation, otherwise known as Ferraro's law of isorotation: Ω must be constant not on the surfaces $\mathcal{S}$, but on the toroidal shells of $\mathbf{B}_\mathrm{p}$. At present this cannot be distinguished helioseismically from shellular solid rotation, since present helioseismic results give us no more than a low-resolution view of the outer parts of the radiative interior away from the poles.

It therefore appears that there is an exceedingly strong case for assuming toroidally-shellular solid rotation, including the implied jump in Ω at the tachocline, when doing helioseismic inversions — unless and until further observational information forces us to do otherwise by, for instance, overturning statement (*b*). Occam's razor suggests that first attempts should use simple $\mathbf{B}_\mathrm{p}$ configurations somewhat like the gravest modes obtained from classic calculations of primeval magnetic diffusion, with a single neutral ring or limiting torus on the equatorial plane. The boundary conditions on $\mathbf{B}_\mathrm{p}$ at the tachocline will need reconsideration, if only because of the requirement for a well-contained field to give a reinforcing-rod effect consistent with statement (*b*). Any such procedure will usefully constrain the Ω structure not only near the tachocline but also in the deep interior, where the scope of helioseismic information is most severely limited.

The same picture also promises a natural and convincing explanation for statement (*a*), consistent with the idea that the convection zone is relatively vigorous dynamically and has a latitude-dependent differential rotation $\Omega = \Omega_\mathrm{CZ}$ for dynamical reasons of its own, and that the tachocline is the necessary transition layer between Ω_CZ and the interior, $\mathbf{B}_\mathrm{p}$-constrained Ω field. This predicts first of all that the tachocline must be as thin as nonlinear fluid dynamics or magnetohydrodynamics permit it to be. Specifically, we can expect the tachocline to be the seat of three-dimensional, shear-related instabilities one of whose effects, to a first approximation, is likely to be a local, quasi-frictional torque across the tachocline far larger than the net torque associated with magnetic solar-wind braking (Gough 1985, Brown *et al.* 1989, Dziembowski and Goode 1993).

An inevitable by-product of such local shear instabilities will be radiation, into the interior, of various kinds of waves — some of them with far larger amplitudes, and with radial wavelengths far less subject to drastic thermal dissipation, than the weak internal gravity waves generated directly by convection zone eddies (*e.g.* Gough 1977, Press 1981, García López and Spruit 1991, Schatzman 1993 & refs.). The timescales probably favour Rossby waves; and Rossby waves from the tachocline should be emitted mostly into the high-latitude interior, because it is at high latitudes that the interior has prograde Ω relative to the tachocline. The one-signed Rossby-wave dynamics and dispersion properties then allow long-range propagation over substantial depths and latitudes. Rossby-wave propagation will hardly feel $\mathbf{B}_\mathrm{p}$ and should be far less drastically limited by thermal dissipation than gravity-wave propagation (Gough 1977, Press 1981); and Rossby-wave dispersion properties favour not only downward, but also equatorward propagation, of waves originating in high latitudes as can easily be shown. It seems possible that the Rossby waves in question could affect all the toroidal shells of $\mathbf{B}_\mathrm{p}$, especially if at some stage of spindown the shells were all rotating at the same Ω as, or faster than, the outermost shells relevant to statement (b).

This puts the high-latitude tachocline into close analogy with the Earth's winter troposphere, which emits Rossby waves into the relatively prograde winter stratosphere. In just the same way, the relatively prograde motion, Ω larger, in the winter stratosphere favours long-range Rossby propagation; and, again, the Rossby-wave dynamics and dispersion properties favour not only upward but also equatorward propagation, so that some Rossby-wave activity affects the tropical as well as the extratropical winter stratosphere. As argued earlier, it probably contributes thereby to the terrestrial QBO.

In the case of the Sun's Rossby waves, any wave dissipation, whether by thermal damping or by various modes of wave breaking, will produce a retrograde torque on the toroidal shells of $\mathbf{B}_\mathrm{p}$. This will slow them down until their Ω values are nearly outside the range of Rossby-wave phase speeds being produced by the tachocline. This too is consistent with statement (a), and it suggests moreover that future helioseismic deductions of Ω values on the shells of $\mathbf{B}_\mathrm{p}$ will contain key information about Rossby-wave phase speeds. The whole picture makes sense provided that the timescale for the retrograde torque to act is shorter than primeval spindown timescales. In all likelihood it is very much shorter; I shall refer to this as the 'strong Rossby-wave hypothesis'.

If the Rossby waves reach the Sun's equatorial plane — and in some circumstances they might even do so more than once, after propagating to polar regions and back hence reaching greater depths — then there are two further consequences of interest. The first is the inertial-instability-related wave breaking that could provide the lithium channel near the equator. This, too, merits further investigation. The second is the possibility of a QBO-like phenomenon near the equator. Why near the equator, in view of what was said above about the scale $L_{\rm lat}$ being likely to be broad, a conclusion that might seem at first sight unlikely to be altered by the presence of a broad-scale $\mathbf{B}_{\rm p}$ field? To understand this, we must first return to the question of the damping of magnetic torsional oscillations.

The peculiarly high damping of magnetic torsional oscillations

The hypothesis of toroidally-shellular solid rotation requires that magnetic torsional oscillations be sufficiently damped. Now it appears that typical magnetic torsional oscillations are much more highly damped, even without help from shear or magnetohydrodynamic instabilities, than one might guess from the magnetic diffusion time $\tau_{\rm I}$ for the whole interior ($\tau_{\rm I} \lesssim 10^{10}$ years) and from the corresponding viscous diffusion time, which is several orders of magnitude greater still. Typical magnetic torsional damping times $\tau_{\rm mag}$ turn out to be considerably shorter even than the Kelvin–Helmholtz thermal time $\sim 3\times 10^7$ years.

Recent unpublished results independently obtained by Professor Gough and myself show that $\tau_{\rm mag}$ depends on the magnitude $|\nabla \ln \tau_{\rm A}|$ of the logarithmic gradient, across the toroidal shells of $\mathbf{B}_{\rm p}$, of the Alfvén time $\tau_{\rm A}$ around those shells in a meridional plane:

$$\tau_{\rm mag} \sim {\tau_{\rm A}}^{2/3}\,\{\eta(|\nabla \ln \tau_{\rm A}|)^2\}^{-1/3}\ , \tag{9}$$

where η ($\sim 10^5 {\rm cm}^2{\rm s}^{-1}$) is the magnetic diffusivity. The gradient causes an inexorable scale-shrinkage as t^{-1}, giving enormously enhanced dissipation rates. If the cross-shellular variation of the reciprocal Alfvén time ${\tau_{\rm A}}^{-1}$ is of the same order as ${\tau_{\rm A}}^{-1}$ itself, then very roughly $\tau_{\rm mag} \sim {\tau_{\rm A}}^{2/3}\,{\tau_{\rm I}}^{1/3}$. If for instance $\tau_{\rm A} \lesssim 10^4$ years, then in this simplest case $\tau_{\rm mag} \lesssim 10^6$ years. Magnetic-buoyancy or other instabilities could, at finite amplitude, shorten this still further (Gough, personal communication).

The theory is a nontrivial generalization of the theory of Kelvin sheared disturbances (Thompson 1887), with $\tau_{\rm A}$ taking the place of the single-wavelength advection time but with the unequal magnetic and viscous diffusivities complicating the analytical and behavioural details. When the diffusivities are equal, the theory becomes mathematically the same as Kelvin's. When they are unequal, as in the real Sun, it is the larger, magnetic, diffusivity that governs the damping.

The possibility of a 'watch-spring QBO' near the neutral ring

Should we expect there to be, or have been, a solar QBO-like phenomenon, specifically, a QBO-like fluctuation in the interior differential rotation field Ω? If statement (b) is true and $\mathbf{B}_{\mathrm{p}}$ exists, and if furthermore statement (a) and the strong Rossby-wave hypothesis are true, then at least some of the ingredients are present. Indeed the analogy between the tachocline and the Earth's troposphere, already discussed, and the indication from helioseismic data that typical tachocline shears are far stronger than required by primeval spindown dynamics alone, point toward Rossby-wave amplitudes that could easily be far above the minimum necessary. Some order-of-magnitude estimates are being attempted, to gain an idea of the retrograde torques that might result.

On the other hand, it is still a moot point whether any wave types capable of exerting prograde torques are available, with sufficient amplitudes. To the extent that the partial analogy between the Sun's tachocline and the Earth's troposphere is valid, the tachocline must be a far more efficient generator of Rossby waves than of gravity waves and equatorial hybrid types. This seems unlikely to be altered by Lorentz forces of plausible magnitude and timescale in the tachocline. The direct wave source from the convection zone seems weak, also, for this purpose, as already mentioned, with current estimates pointing to rates of change of Ω not much faster than primeval spindown (*e.g.* Schatzman 1993 & refs.).

So it is worth asking first whether we could get a QBO-like phenomenon with only one type of wave. The answer, albeit with certain caveats, is "perhaps yes" — in interesting contrast to the terrestrial QBO. Moreover, the requirement on wave amplitude is far less severe. The reason is the Alfvénic quasi-elasticity, or watch-spring mechanism, provided by $\mathbf{B}_{\mathrm{p}}$. The waves need only do work on the torsional oscillations once per cycle; and the work per cycle need only be a small fraction of the energy of the oscillation. The wave-induced angular momentum transport is required to be significant not on the timescale τ_{A} of a single oscillation, but only on the damping timescale τ_{mag}, which even though far shorter than τ_{I} may well be far longer than τ_{A}.

In order for the wave-induced angular momentum transport or radiation stress to do positive work on an incipient torsional oscillation, the waves need only satisfy two of the conditions already familiar from studies of the terrestrial QBO. First, the waves must have nonzero angular phase speeds relative to the mean Ω values of the toroidal magnetic shells in question. Second, their dissipation must be enhanced by Doppler downshifting. Then it is not too hard to see that the resulting torque, whether prograde or retrograde, from any waves that propagate past the relevant places, will always do positive work on the incipient torsional oscillation because such waves dissipate most strongly, exerting the greatest torques, when and where the fluctuation in Ω is in the same sense as, albeit of smaller magnitude than, the waves' intrinsic phase speeds — just as in the terrestrial QBO. So whether the magnetic torsional oscillations are excited or not is partly a question of the competition between their own damping rate $\tau_{\rm mag}^{-1}$ and the strength of the excitation by the waves, in turn dependent on radiation stress, approximately proportional to wave amplitude squared, and on the sensitivity of wave dissipation to Doppler downshifting.

The main caveat is that such an oscillation could not persist by itself, because if nothing else were happening there would inevitably be a time-mean retrograde torque that would gradually take Ω out of the effective range of available Rossby-wave phase speeds. This is the same 'strong Rossby wave' effect already invoked to account for statement (a). It would be necessary for some other agency, perhaps a weaker prograde wave, to stop Ω drifting completely out of range. An intriguing possibility, in no way contradicting statements (a) and (b), is that the field $\mathbf{B}_{\rm p}$ might have been expelled from a rapidly (and progradely) rotating core, or axial vortex, in which case an inner 'spindown tachocline' would exist and might provide enough prograde torque either in a local quasi-frictional sense or, for instance, through short-range gravity-wave propagation, on parts of the toroidal shells of $\mathbf{B}_{\rm p}$, keeping their Ω values in range.

Where should we first look for a solar QBO-like phenomenon? One reasonable answer is to look first where magnetic torsional oscillations are least damped. The estimate (9) tells us clearly where that is, even though the estimate itself obviously becomes invalid there: the place to look is near the neutral ring, or limiting torus, of $\mathbf{B}_{\rm p}$, where $\tau_{\rm A}$ is stationary and $|\nabla \ln \tau_{\rm A}|$ vanishes. Here we are again invoking the Occam's-razor assumption that $\mathbf{B}_{\rm p}$ is simple, with just one neutral ring on the equatorial plane.

Quantitative estimates are beyond the scope of this discussion, and indeed will demand much detailed work. But it might not be accidental that helioseismic results, in a recent interpretation reported by Goode and Dziembowski (1991), give a hint of possible time-dependence of Ω in a deep region below the solar equator, which could indeed be part of the neighbourhood of the neutral ring of a plausible $\mathbf{B}_{\mathrm{p}}$. If this putative time-dependence is real, then the timescale is comparable to that of the solar cycle and, as the authors point out, could only be a magnetic torsional oscillation on a $\mathbf{B}_{\mathrm{p}}$ of kilogauss strength.

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