UC Santa Cruz

2010 International Summer Institute for Modeling in Astrophysics

Title
Geostrophic turbulence with a magnetic field

Permalink
https://escholarship.org/uc/item/73q3t00z

Authors
Mak, Julian
Diamond, Patrick

Publication Date
2010-09-01

GEOSTROPHIC TURBULENCE IN THE PRESENCE OF A WEAK MAGNETIC FIELD

JULIAN MAK[†]

ABSTRACT. This document provides an overview of the project I carried out under the mentorship of Pat Diamond (UCSD), David Hughes (Leeds) and Nic Brummell (UCSC), for the first International Summer Institute for Modeling in Astrophysics (ISIMA) program, Jul-Aug 2010. The project is an extension of the work on β-plane magnetohydrodynamic (MHD) turbulence and its consequences on momentum transport. A somewhat detailed overview is given, with the physical mechanisms explained. The quasi-geostrophic equations, so well known in the Geophysical Fluid Dynamics (GFD) community, is derived with the Lorentz force present. The two-layer model is proposed as a simplified model for our studies. Progress with magnetically influenced barotropic and baroclinic instabilities are given, and some proposed future work concludes the document.

1. CONFINEMENT PROBLEM OF THE SOLAR TACHOCLINE

One fundamental and open problem in Solar physics concerns the confinement problem of the *tachocline*. The tachocline is the region straddling the convection zone and radiative zone, located at around 70% Solar radius, believed to be of no more than 4% Solar radius thick. Through helioseisomology, it has been observed that the convection zone rotates differentially, faster at the equator and slower at the poles, whilst the radiative zone rotates uniformly, thus the tachocline is the transition region between differential and uniform rotation. It is believed that the upper layer (overshoot) is turbulent, with strong toroidal magnetic fields, whilst the lower layer is stably and strongly stratified, but less known about the field structure in this region. For a more comprehensive review of the tachocline, please see the book by Hughes *et al.* (2007).

In the first ever study on the tachocline, Spiegel & Zahn (1992) observed that, in a purely hydrodynamic model of the Sun, deviation from thermal wind balance together with angular momentum conservation would drive meridional flows that transports angular momentum from the equator to the pole, burrow down, and necessarily spread the tachocline region into the radiative zone. This is however not observed, so clearly there needs to be some mechanism that prevents this spreading of the tachocline. Meridional flows are probably not strong enough to transport the

momentum back, from pole to equator, whilst radiative mixing will be inefficient due to the strong stratification. So what Spiegel & Zahn (1992) invoke is that, since the region is so strongly stratified and probably turbulent, we must have a strongly anisotropic turbulent viscosity. This will effectively diffuse momentum horizontally, thus maintaining the uniform rotation as observed. The spreading of the tachocline in the hydro case has been studied numerically (e.g. Garaud 2003, see also references in Miesch 2007).

However, Gough & McIntyre (1998) argue that this would imply a situation like that of the stratosphere on Earth, and thus should be in the *geostrophic turbulence* regime. The mixing of potential vorticity will necessarily drive zonal flows (see later). The presence of zonal flows will not give the kind of momentum diffusion as required by Spiegel & Zahn (1992). From this, they conclude that the mechanism proposed by Spiegel & Zahn (1992) does not work, and proposed that there must be an interior poloidal field (possibly primordial) locking everything in uniform rotation, via Ferraro's law of isorotation. There is debate as to whether the tachocline is in fact turbulent, and if so, what causes the turbulence, which has an effect on the precise outcome; see the article by Miesch (2007). Either way, it seems plausible that there is an interior field which locks the radiative zone in uniform rotation. Some of the intricate details of the Gough & McIntyre model has been investigated by Pascale Garaud, and also in Toby Wood's thesis (Wood, 2010).

From a turbulence theory point of view, the argument Michael McIntyre uses is that, in such circumstances, wave turbulence is dominant, but because waves allow the possibility of long range transport, one cannot just invoke the usual mixing length theory argument and say that the turbulent viscosity here is frictional, in the sense that it takes energy away from the mean flow. It is instead potential vorticity which is mixed, but because potential vorticity is not a passive scalar, this mixing has a tendency to drive flows. Geostrophic turbulence thus tend to be *anti*-frictional, where waves transfer energy to the mean flow, and end up driving the large-scale flows.

So all is well, except that the tachocline is believe to be magnetised, via plumes raining down from the convection zone, dragging magnetic flux from the convection zone which *is* observed to be magnetised (Tobias *et al.*, 1998), or magnetic field leaking into the tachocline from the interior (should there be an interior magnetic field). So what happens if we consider MHD turbulence in the geostrophic turbulence regime, even if the field is small? Diamond *et al.* (2007) predicted, and Tobias *et al.* (2007) showed that, when a weak magnetic field is present, potential vorticity mixing is inhibited, and zonal flows no longer form once a critical threshold of magnetic field strength / resistivity is reached (and this threshold is achievable in the tachocline). What in fact happens is that Maxwell stresses has a tendency to cancel with Reynolds stresses. When they exactly cancel with each other, there are no stresses driving the flow. Further more, no total stress means no turbulent viscosity. This result supports neither Spiegel & Zahn (1992) or Gough & McIntyre (1998), although this result is worse for the former than the latter: The interior

field will still do the job, and allow tachocline confinement, but the lack of turbulent viscosity is bad news for the Spiegel & Zahn model.

Diamond *et al.* (2007) and Tobias *et al.* (2007) did their work in the 2D β-plane regime, and it is my intention to try and generalise this to more 'realistic' regimes. The first generalisation is to introduce the missing spatial co-ordinate back in, but in such a way that most of the theory done before will still hold. For this, I will consider the so called *quasi-geostrophic* (QG) equations, well known in GFD, which was used before the days of high power computing for numerical weather prediction (Charney, 1948, 1949).

2. Overview of geostrophic turbulence: HD vs. MHD

To provide some continuity, I give here an interpretation of the articles by Diamond *et al.* (2007) and Tobias *et al.* (2007), as well interpretations of the chapters in the books by Vallis (2006) and Salmon (1998). Those aware of the article or the area of conventional geostrophic turbulence can probably skip to the next subsection for a brief overview to MHD geostrophic turbulence.

2.1. **Geostrophic turbulence with no field.** *Geostrophic turbulence* is turbulence in a system where strong stratification and rotation keeps the flow essentially 2D - strong stratification inhibits vertical motion, whereas strong rotation suggests Taylor-Proudman effect should occur, both contributing to the '2D-ness' of the problem. Although the Taylor-Proudman constraint implies that eddies should be like vertical cylinders, strong stratification implies that, in fact, eddies are more like pancakes in geostrophic turbulence. Turbulence in this regime has been well studied by the GFD community.

It is well known in two-dimensions that the energy and enstrophy (square of the vorticity) is conserved in the absence of viscosity and forcing:

$$E = \int \frac{|\nabla \psi|^2}{2} \, dA, \quad Z = \int \frac{|\nabla^2 \psi|^2}{2} \, dA, \tag{1}$$

where we have used the streamfunction. Considering the Fourier transform and defining the energy spectrum to be

$$\hat{E} = (2\pi)^2 \int \frac{k^2 |\psi|^2}{2} \, dk \equiv \int \mathcal{E}(k) \, dk, \tag{2}$$

enstrophy is then

$$\hat{Z} = \int k^2 \mathcal{E}(k) \, dk. \tag{3}$$

Thus energy and enstrophy are the zeroth and second moments respectively. I will drop the hats in subsequent discussions as we will be staying in Fourier space.

Two other terms that one may encounter is *palinstrophy* ("curl or the curl", the fourth moment), and something which is relatively new known as *zonostrophy*, but we don't need them here. They come into play when one considers more specialised problems.

Now, the conservation of enstrophy as well as its tendency to forward 'cascade' to small scales necessarily imply an inverse cascade to large scales. In general, we

could show that

$$\frac{d}{dt}\left[\frac{\int kE(k)\ dk}{\int E(k)\ dk}\right] < 0, \qquad \frac{d}{dt}\left[\frac{\int k^2 Z(k)\ dk}{\int Z(k)\ dk}\right] > 0, \tag{4}$$

which confirms the above proposition. By the assumptions of inertial range theory, the addition of dissipation and forcing only acts to remove and inject energy at the relevant scales, so does not affect the conclusion.

So, assuming we have an inertial range, denoting the rate of enstrophy transfer to small scales by η, rate of energy transfer of energy at large scales (via some means) by ϵ, and suppose we inject some energy into the system via forcing at some k_f (assume sufficient scale separation), we can show, via a Kolmogorov type argument, that, at least asymptotically,

$$E(k) = \begin{cases} \eta^{2/3}k^{-3}, & k \gg k_f, \\ \epsilon^{2/3}k^{-5/3}, & k \ll k_f. \end{cases} \tag{5}$$

The $-5/3$ spectrum is well observed in simulations, but the downscale spectrum may be steeper than -3.

By using the quasi-geostrophic approximation (more later), an extra parameter comes in and is known as the *Rossby deformation radius* L_d (or $k_d \sim L_d^{-1}$). When we are dealing with length scales comparable or smaller than the Rossby deformation radius, our variations in stratification is small compared to the mean stratification. Starting with the infinite Rossby deformation radius case ($k_d = 0$), the (inviscid) β-plane equation we are dealing with is

$$\frac{Dq}{Dt} = 0, \qquad q = f + \nabla^2\psi, \tag{6}$$

where q is known as the *potential vorticity* (PV). It is also known as *vortensity* in some astrophysics literature, but we will insist on using the term PV as this is a more established idea.

The equation may be written in the form

$$\frac{\partial}{\partial t}\nabla^2\psi + \boldsymbol{v}\cdot\nabla\nabla^2\psi + \beta\frac{\partial\psi}{\partial x} = 0. \tag{7}$$

How time scales depends on what terms dominate, and this is determined by the spatial scale. The boundary between rotation dominated region (wave turbulence) and nonlinearity dominated region (quasi-2D turbulence) is then described by

$$L_R \sim \sqrt{\frac{U'}{\beta}}, \qquad k_R \sim L_R^{-1}, \tag{8}$$

commonly known in the literature as the *Rhines scale* (see Rhines 1975, 1979; Rhines & Holland 1979). The Rhines scale may also be defined via a comparison between Rossby wave frequency and typical decorrelation rate of the eddies, in the sense that

$$\omega_R = \frac{-k\beta}{k^2 + l^2}, \quad \omega_E = kU', \tag{9}$$
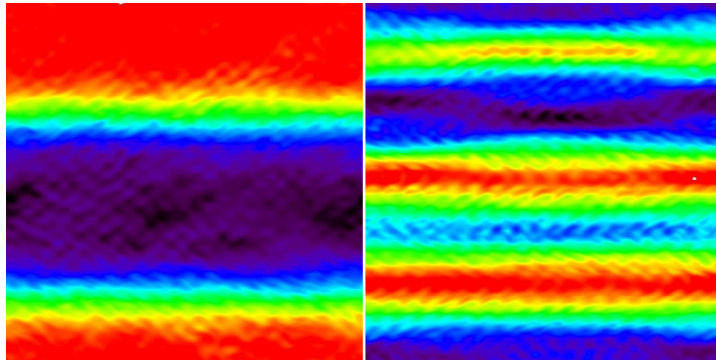
$$\Rightarrow K_R^2 \sim \frac{U'}{\beta}. \tag{10}$$

FIGURE 1. Simulation of the 2D β-plane in geostrophic turbulence regime with no field. β is 5 on the left and 50 on the right. It is clear that the jet widths are thinner when β is higher, as suggested by the Rhines scale occurring at smaller length scales due to the larger β. (Image courtesy of Steve Tobias).

The $U^{'}$ term should be seen as a RMS eddy velocity. Vallis & Maltrud (1993) argued that this should be redefined using of eddy strain rates, but it has been shown that this does not make a huge difference, so we shall adopt Rhines' argument.

One argument is that, because we have an inverse cascade, energy at some forcing will always end up reaching the Rhines scale. Once we reach the Rhines scale, we are in the wave turbulence regime, where we need to satisfy both the wave vector resonance condition as well as the frequency matching condition:

$$\boldsymbol{k_1} + \boldsymbol{k_2} + \boldsymbol{k_3} = 0, \quad \omega_1 + \omega_2 + \omega_3 = 0. \tag{11}$$

We consider triads because they are easier to satisfy than, for example, quartets (note gravity waves require quartets due to their strongly dispersive nature). Due to the anisotropic nature of Rossby waves, the resonance conditions are extremely difficult to satisfy. Note, however, if one of the k is zero, i.e. wave + wave + zonal flow, then the frequency matching condition is easily satisfied. Thus, statistically, we expect a substantial portion of the energy to be transfered to the $k = 0$ mode, making zonal flows the most prominent feature under the geostrophic turbulence regime. An alternative argument by Vallis & Maltrud (1993) is that energy seeks the gravest mode, and due to the anisotropy and mismatch in frequencies, the energy piles up onto the $k = 0$ mode, so giving rise to zonal flows. The argument for this is that $k = 0$ is not, in some sense, a wave. Both arguments yield the same conclusion.

Another thing we might expect is that we expect the zonal flows formed to have a characteristic width proportional to the Rhines scale. Indeed, this is suggested by simulations (see figure 1 for the 2D β-plane example), but the details are still being debated.

It should be noted that the word *cascade* may not necessarily be appropriate, as it is not known whether the process of energy transfer is really a multi-stage process like that described by L. F. Richardson and A. N. Kolmogorov. Also, physically, the

separation between the relevant length scales are not necessarily large enough to be described by an inertial range theory, so care should be taken when applying this to physical examples. Numerically of course we can artificially cook up situations where this applies.

Another way of thinking of this is to consider the equation for the Reynolds stresses: Doing the usual mean-field expansion, we see that, ignoring forcing and diffusion

$$\frac{\partial \langle u \rangle}{\partial t} = -\frac{\partial}{\partial y} \langle u^{'} v^{'} \rangle. \tag{12}$$

The usual mixing length theory implies the commonly encountered turbulent viscosity:

$$\frac{\partial \langle u \rangle}{\partial t} = -\nu_t \frac{\partial}{\partial y} \langle u^{'} \rangle. \tag{13}$$

Inspired by G. I. Taylor, we can see that, in the β-plane case (see, for example, Rhines & Holland 1979),

$$\frac{\partial \langle u \rangle}{\partial t} = \kappa_{yy} \frac{\partial}{\partial y} \langle Q \rangle, \tag{14}$$

where κ_{ij} is seen as a Lagrangian diffusivity. Thus we see that the formation of mean-flow is directly linked to the mixing of PV. This is because PV is a dynamical tracer. It has been argued it is in fact the mixing of PV which is fundamental in driving zonal flows, rather than the presence of an inverse cascade (e.g. Wood & McIntyre 2010).

2.2. **Geostrophic turbulence in MHD regime.** So what happens when we put a field in? The equations for ideal 2D β-plane MHD are then, denoting A as the usual magnetic potential function,

$$\frac{Dq}{Dt} = \partial(A, \nabla^2 A), \tag{15}$$

$$\frac{DA}{Dt} = 0. \tag{16}$$

Here, $\partial(\cdot, \cdot)$ is the Jacobian operator in 2D. Then it is clear the PV is no longer conserved, and indeed we could imagine the magnetic field inducing 'memory' (i.e. increased correlation time) into the turbulence and inhibiting the mixing of PV. As we have seen above, no PV mixing, no zonal flow formation. Another reason we do not expect zonal flows to form is because in 2D MHD turbulence, enstrophy is no longer conserved, so there is no energy inverse cascade (see Pouquet 1978 and references within).

So why should there be no zonal flows? It is well known that in 2D we have *Zel'dovich's theorem*:

$$\frac{\langle B^{'} \rangle}{\langle B \rangle} = \frac{\eta_t}{\eta} \equiv Nu_m, \tag{17}$$

which effectively says that a weak large scale magnetic field has a tendency to be chopped up into strong small scale field by the turbulence. Here, η is the magnetic diffusivity, and the subscript t denote the turbulent quantity via the usual mixing length theory. We note that the magnetic Nusselt number Nu_m usually scales with the magnetic Reynolds number Rm, so when Rm is large, small scale fluctuations can be extremely strong.
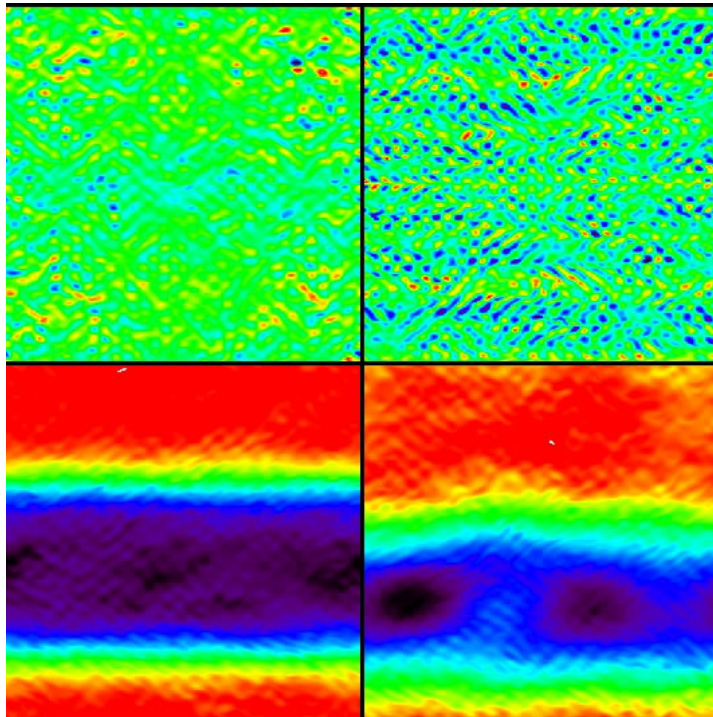
FIGURE 2. Simulation of the 2D β-plane in geostrophic turbulence regime with field. The bottom-left panel is the case with no field. Field strength increases anti-clockwise, starting from bottom-left corner. As field strength is increased, large scale coherent structures refuses to form. (Image courtesy of Steve Tobias).

The presence of a magnetic field has a tendency to *Alfvénise* the system: When $\eta \ll 1$ (or $Rm \gg 1$), field lines are frozen into the fluid, and breaking of field lines is not allowed except at extremely small scales. The turbulent shuffling of the eddies has a tendency to stretch the field lines at small scales, thus strengthening the small scale fluctuating magnetic field. The tendency is for equipartition of energy to occur (Pouquet, 1978):

$$|\boldsymbol{u}_k^{'}|^2 \approx |\boldsymbol{b}_k^{'}|^2. \tag{18}$$

Going back to the equation for stresses, we see that

$$\frac{\partial \langle u \rangle}{\partial t} = -\frac{\partial}{\partial y} \left[\langle u^{'} v^{'} \rangle - \langle b_x^{'} b_y^{'} \rangle \right]. \tag{19}$$

When equipartition is reached, the Maxwell stress will in general cancel out with the Reynolds stress, hence there are no turbulent stresses driving the flows. This lack of zonal flow formation may be seen in Figure 2.

3. THE QUASI-GEOSTROPHIC EQUATIONS IN MHD

Here I give a derivation of the QG equation with the relevant forcing and dissipation which. For those interested, there is a derivation using an asymptotic

expansion given in Gilman (1967a,b), similar to the procedure given in Pedlosky (1987) or Vallis (2006).

First of all, it should be noted that, although we have argued that QG dynamics is similar to 2D dynamics, this analogy should be taken with great care. For example, there is no reason to expect that the inertial range spectrum (should an inertial range actually exist) to be $-5/3$ and -3 as suggested by 2D turbulence (Charney 1971, but see rebuttal in Tung & Welch 2001), although it appears to be observed numerically (Salmon 1978, 1980; Tung & Welch-Orlando 2003; Tulloch & Smith 2009b).

We start with the 3D Boussinesq equation with the β-plane (and invoking the traditional approximation),

$$\frac{\partial \boldsymbol{u}}{\partial t} + \boldsymbol{u} \cdot \nabla \boldsymbol{u} + \boldsymbol{f} \wedge \boldsymbol{u} + \nabla P + \alpha \boldsymbol{g} T = \boldsymbol{F}, \tag{20}$$

$$\frac{\partial T}{\partial t} + \boldsymbol{u} \cdot \nabla T = Q, \tag{21}$$

$$\nabla \cdot \boldsymbol{u} = 0, \tag{22}$$

where $\boldsymbol{f}$ is the rotation vector (we take axis of ration to be in the z- direction), α the coefficient of thermal expansion, and $\boldsymbol{g} = g\boldsymbol{e}_z$ is gravitational acceleration, which, along with the density, we will take to be constant. $\boldsymbol{F}$ is some sort of forcing term, and Q would be some sort of heating term. We will bear in mind that we want

$$\boldsymbol{F} = \boldsymbol{j} \wedge \boldsymbol{B} + \nu \nabla^2 \boldsymbol{u}, \quad Q = \kappa \nabla^2 T, \tag{23}$$

so the induction equation will make an appearance later on. Another note is that T should really be thought of as buoyancy, although I have labelled it as T for temperature.

Taking the curl of the momentum equation and gradient of the thermodynamic equation, contracting the equations with the appropriate terms, and noting that the baroclinic term $\nabla T \cdot (\nabla \wedge \boldsymbol{g} T)$ does not appear in this regime, we have

$$\frac{D}{Dt}[(\boldsymbol{\omega} + \boldsymbol{f}) \cdot \nabla T] = \nabla T \cdot (\nabla \wedge \boldsymbol{F}) + (\boldsymbol{\omega} + \boldsymbol{f}) \cdot \nabla Q. \tag{24}$$

In the absence of forcing, dissipation and heating, this is just the statement of potential vorticity (PV) conservation.

We now make our QG approximation: we assume that

(1) $Ro \ll 1$, so we have a Taylor-Proudman state at zeroth order in Rossby number. Should we do an asymptotic expansion, this will act as our small parameter.

(2) $|\beta| \ll |f_0|$, where we have used $f = f_0 + \beta y$. So we assume the deviation from the mean rotation rate is small (or, alternatively, there is only mild differential rotation).

(3) We also assume that our typical length scale is comparable to (but certainly not much larger than) the *Rossby deformation radius*. As a consequence, we have small variations in stratification relative to the mean stratification.

(4) We assume the magnitude of Q and $\boldsymbol{F}$ are *small*. More precisely, we would like them to be of $\mathcal{O}(Ro)$.

The last assumption is in contrast to what is termed *magnetostrophic* approximation (by Keith Moffatt), as a model for the geodynamo. The assumption there is that, to lowest order, the balance is between Coriolis force, pressure, and Lorentz force. We are going to insist, to lowest order, the balance is between Coriolis force and pressure, so that we have geostrophy, simplifying our problem substantially.

We now proceed with the reduction of the equation. To zeroth order we have geostrophy, and, by assumption 2), we have

$$f_0 \boldsymbol{e}_z \wedge \boldsymbol{u} = -\nabla P, \tag{25}$$

so we define our streamfunction to be

$$\psi = \frac{P}{f_0}. \tag{26}$$

(If we include the Lorentz force explicitly, we will of course take $P = u^2/2 - B^2/2$, where B is measured in units of Alfvén velocity). At zeroth order, we have $\nabla \cdot \boldsymbol{u}_g = \nabla \cdot (u^{(0)}, v^{(0)}) = 0$, where $\boldsymbol{u}_g$ is the geostrophic velocity. So, at zeroth order,

$$\nabla \cdot \boldsymbol{u} = \nabla \cdot \boldsymbol{u}_g + \frac{\partial w}{\partial z} = 0, \tag{27}$$

which, upon choosing appropriate boundary conditions, will give a constant term in the equations. We will choose rigid boundary conditions and take $w = 0$ here.

From (magneto)hydrostatic balance, we have

$$\frac{\partial P}{\partial z} + \alpha g T = 0, \tag{28}$$

so all our relevant dynamic quantities may be expressed in terms of the streamfunction as

$$u = -\frac{\partial \psi}{\partial y}, \quad v = \frac{\partial \psi}{\partial x}, \quad T = -\frac{\partial}{\partial z} \frac{f_0 \psi}{\alpha g}. \tag{29}$$

We expect the vertical velocity not to come in because of the strong stratification (large Richardson number or low Froude number).

Following on from this, we take $T = T_0(z) + \theta(x, y, z)$, the mean temperature (buoyancy) and the fluctuating part. Then strong stratification implies that

$$|\Gamma| \equiv \left| \frac{\partial T_0}{\partial z} \right| \gg \left| \frac{\partial \theta}{\partial z} \right|. \tag{30}$$

We start reducing the various quantities:

Lagrangian derivative -

$$\frac{D}{Dt} = \frac{\partial}{\partial t} + \boldsymbol{u}_g \cdot \nabla. \tag{31}$$

Potential vorticity -

$$(\boldsymbol{\omega} + \boldsymbol{f}) \cdot \nabla T = f_0 \Gamma + f_0 \boldsymbol{e}_z \cdot \nabla \theta + \Gamma \boldsymbol{e}_z \cdot (\boldsymbol{\omega} + \beta y \boldsymbol{e}_z) + \mathcal{O}(Ro^2), \tag{32}$$

but of course the first term is just a given constant by definition (although it is $\mathcal{O}(1)$) and plays no dynamical role, thus

$$PV = \boldsymbol{e}_z \cdot [f_0 \nabla \theta + \Gamma(\boldsymbol{\omega} + \beta y \boldsymbol{e}_z)]. \tag{33}$$

Forcing -

$$[\nabla T \cdot (\nabla \wedge \boldsymbol{F}) + (\boldsymbol{\omega} + \boldsymbol{f}) \cdot \nabla Q] = \Gamma \boldsymbol{e}_z \cdot (\nabla \wedge \boldsymbol{F}) + f_0 \boldsymbol{e}_z \cdot \nabla Q + \mathcal{O}(Ro^2). \tag{34}$$

(This is where our assumption that the forcing and dissipation terms are of $\mathcal{O}(Ro)$ comes in).

Anticipating we have the Lorentz force, we consider the induction equation. To first order, the z-component is

$$\frac{\partial b_z}{\partial t} + \boldsymbol{u}_g \cdot \nabla b_z = 0, \tag{35}$$

and so the vertical field is simply advected around by the geostrophic velocity. If it was initially zero (to first order), it would be zero for all subsequent time. Taking this as an assumption (which we expect to be a plausible approximation for a substantial portion of the tachocline), our horizontal magnetic is, to first order in Ro (forcing is $\mathcal{O}(Ro)$ by assumption), divergence free, so we can define the magnetic potential function A in the usual way. Vertical field may be produced by the equations, but there is no feedback at this order. They will come in once we consider equations at $\mathcal{O}(Ro^2)$.

Putting the approximated quantities (31), (33) and (34) together, using the definitions of the streamfunction given in (29), and back substitute into the original equations, we obtain the following set of equations:

$$\frac{D}{Dt} q = \partial(A, \nabla^2 A) + \nu \nabla^2 \left[\nabla_h^2 \psi + \frac{\kappa}{\nu} \frac{f_0^2}{N^2} \frac{\partial^2}{\partial^2} \psi \right], \tag{36}$$

$$q = \nabla_h^2 \psi + \beta y + \frac{f_0^2}{N^2} \frac{\partial^2}{\partial z^2} \psi, \tag{37}$$

$$\frac{D(\cdot)}{Dt} = \frac{\partial(\cdot)}{\partial t} + \partial(\psi, \cdot), \tag{38}$$

$$\frac{\partial A}{\partial t} + \partial(\psi, A) = \eta \nabla_h^2 A. \tag{39}$$

$\partial(\cdot, \cdot)$ is the Jacobian operator, and we have absorbed the relevant terms and called it the buoyancy frequency N^2 (constant in the Boussinesq regime).

Note that q is sometimes called the *pseudo*potential vorticity, conserved in the QG regime, in contrast to the *Ertel* potential vorticity, which is conserved in the 3D regime. The extra z-derivative term in q is a vortex stretching term.

To do the derivation more rigourously, we can derive the same equations using an asymptotic expansion with Ro as the small parameter (see Gilman 1967a). One should be careful that we will require a solvability condition (to avoid projection into the kernel), and this comes from taking the curl of the momentum equation.

We note that since the vertical field does not come into play here, we cannot get a dynamo. Indeed (assuming homogeneity for simplicity), multiplying (39) by A and integrating, we have

$$\frac{\partial}{\partial t} \frac{\langle A^2 \rangle}{2} = -\eta \left\langle (\nabla A)^2 \right\rangle = -\eta \left\langle B^2 \right\rangle, \tag{40}$$

$$\Rightarrow 0 \leq \int_0^\infty \left\langle B^2 \right\rangle \, dt = \frac{2}{\eta} [A^2(0) - A^2(t)] < \infty, \tag{41}$$

so the mean field squared must necessarily decay to zero as time progresses. To obtain the boundary conditions, we will need to go back to the definition.

The above equations, in the ideal case, conserves energy $(\nabla\psi)^2$, magnetic potential squared A^2, and cross helicity $\nabla A \cdot \nabla \psi$ (assuming appropriate boundary conditions, noting $\partial A/\partial z = 0$). Enstrophy q^2 is *not* conserved.

3.1. **Two-level approximation.** To derive the two-level approximation, one takes the central finite difference approximation of the z-derivative, which gives (for $i = 1$ the top level; see Pedlosky 1987, Vallis 2006 or Tulloch & Smith 2009a)

$$\frac{\partial q_i}{\partial t} + \partial(\psi_i, q_i) = \partial(A_i, \nabla^2 A_i) + \cdots, \tag{42}$$

$$q_i = \nabla_h^2 \psi_i + \beta y + \frac{k_d^2}{2}(\psi_j - \psi_i), \quad j = 3 - i \tag{43}$$

$$\frac{\partial A_i}{\partial t} + \partial(\psi_i, A_i) = \cdots, \tag{44}$$

and $k_d^2/2 = 4f_0^2/N^2H^2$, where H is the height of the domain (we took the levels to be of equal height). I have left out the dissipation terms for the time being. Note that the interface matching is given by the requirement that pressure is continuous either side of the interface. By having the Lorentz force, we note that we want the *total* pressure to be continuous either side of the interface. Within each level, the density is constant.

Alternatively, we could consider a mode projection of the continuous QG equations as

$$\psi = 1 \cdot \psi_0(x, y) + \cos(kz)\psi_1(x, y) + \cdots, \tag{45}$$

which is a projection onto the *barotropic* and *first baroclinic* mode, using the appropriate inner product and normalisation.

The thing to note is that the two levels are coupled via gravity (by the coupled set of PDEs), but not explicitly coupled magnetically. There is no obvious way to do magnetic coupling, or what the consequences would be if we did not have such a coupling. We could always just throw away the need for explicit coupling by considering the continuous QG equations, should we choose to just do a numerical simulation. The initial idea was to derive the two-level equations, force the upper layer and see how the lower layer responds. Physically, we expect plumes to be raining down onto the upper portion of the tachocline, and see what happens with the lower layer. We may well have interior field leaking into the lower tachocline, which we could model as some sort of magnetic flux coming in through the boundaries as a boundary condition. I aim to investigate this further in due course.

3.2. **Conserved quantities of the two-level model.** For $\kappa = \nu = \eta = 0$, the two-level model conserves

$$\mathcal{E} = \frac{(\nabla\psi_1)^2 + (\nabla A_1)^2}{2} + \frac{(\nabla\psi_2)^2 + (\nabla A_2)^2}{2} + k_d^2(\psi_2 - \psi_1)^2, \tag{46}$$

$$\mathcal{A} = A_1^2 + A_2^2, \tag{47}$$

which is the energy (kinetic and potential), and total squared magnetic-potential. Enstrophy is clearly not conserved layer-wise or within the whole system, so there is

no reason to expect energy to inverse cascade. The following quantity is conserved layer-wise:

$$\mathcal{H}_{x,i} = \nabla A_i \cdot \nabla \psi_i + \frac{k_d^2}{2}(\psi_i - \psi_j)A_i. \tag{48}$$

This is a quantity similar to cross-helicity (above without the second term), obtained by multiplying the momentum equation by A_i and the induction equation by q_i. Perhaps by demanding the traditional cross-helicity to be conserved, we may have a natural way of coupling the magnetic field in the two layers.

3.3. **Barotropic-Baroclinic formulation.** Following Salmon (1978, 1980), we write the two-level equations in terms of the barotropic and (first) baroclinic mode

$$\psi = \frac{\psi_1 + \psi_2}{2}, \quad \tau = \frac{\psi_1 - \psi_2}{2}. \tag{49}$$

Then it can be shown that the resulting equations are

$$\frac{\partial}{\partial t}\nabla^2\psi + \partial(\psi, \nabla^2\psi) + \partial(\tau, \nabla^2\tau) + \beta\frac{\partial\psi}{\partial x}$$
$$= \partial(A_\psi, \nabla^2 A_\psi) + \partial(A_\tau, \nabla^2 A_\tau) + \cdots, \tag{50}$$

$$\frac{\partial}{\partial t}[\nabla^2 - k_d^2]\tau + \partial(\tau, \nabla^2\psi) + \partial(\psi, [\nabla^2 - k_d^2]\tau) + \beta\frac{\partial\tau}{\partial x}$$
$$= \partial(A_\tau, \nabla^2 A_\psi) + \partial(A_\psi, \nabla^2 A_\tau) + \cdots, \tag{51}$$

$$\frac{\partial A_\psi}{\partial t} + \partial(\psi, \nabla^2 A_\psi) + \partial(\tau, \nabla^2 A_\tau) = \cdots, \tag{52}$$

$$\frac{\partial A_\tau}{\partial t} + \partial(\tau, \nabla^2 A_\psi) + \partial(\phi, \nabla^2 A_\tau) = \cdots. \tag{53}$$

We see there are only three types of triad interactions:

$$(\psi, \psi) \to \psi, \quad (\tau, \tau) \to \psi, \quad (\psi, \tau) \to \tau. \tag{54}$$

In this case, we have no purely baroclinic interactions, because we insisted the two-levels have equal heights. The question we might ask ourselves is if we somehow have a magnetic coupling, would our operators change for the baroclinic magnetic fields? There is no obvious length scale that we expect to appear, since magnetic fields are not directly influenced by rotation, reflected by the presence of the Rossby deformation radius.

The above equations conserve the following quantities:

$$\mathcal{E} = \frac{(\nabla\psi)^2 + (\nabla A_\psi)^2}{2} + \frac{(\nabla\psi)^2 + k_d^2\tau^2 + (\nabla A_\tau)^2}{2}, \tag{55}$$

$$\mathcal{A} = A_\psi^2 + A_\tau^2. \tag{56}$$

Again, enstrophy is not conserved.

3.4. **Scales of motion.** Since we have a natural length scale k_d that arises in the problem, we could consider several limiting cases:

Small length scale compared to Rossby deformation radius, $k \gg k_d$. Going back to (43), we see that

$$q_i = \nabla^2 \psi_i + \frac{k_d^2}{2}(\psi_j - \psi_i) \approx \nabla^2 \psi_i, \tag{57}$$

and so the two layers decouple, and we go back to the two-dimensional equations. Of course, existence of magnetic coupling may change this. At small enough scales, the Rossby number is no longer small and we expect the QG approximation to break down, and we go into the 3D turbulence regime.

Large length scale compared to Rossby deformation radius, $k \ll k_d$. We only neglect ∇^2 when compared to a k_d^2 term. We see that the barotropic equation (50) is unaffected, but the baroclinic equation becomes

$$\frac{\partial \tau}{\partial t} + \partial(\psi, \tau) = \partial(A_\psi, \nabla^2 A_\tau) + \partial(A_\tau, \nabla^2 A_\psi). \tag{58}$$

Where there is no field, τ becomes a passive tracer. The variance (i.e. the energy) then tends to forward cascade. This is no longer true when the field is present.

Length scale comparable to Rossby deformation radius, $k \sim k_d$. When no field is present, energy has a tendency to seek the gravest mode, since we are in a quasi-2D regime, and energy mostly ends up on the barotropic mode. There is no inverse cascade when the field is present, we expect this not to happen, although I am not exactly sure what the consequences would be.

4. Barotropic and baroclinic instabilities

Much of the work has already been considered by Gilman (1967b, 1969). (I attempted the work before realising it was done already).

4.1. **Waves.** Doing the usual linearisation about $\Psi_1 = \Psi_2 = 0$, $A_1 = A_2 = -B_0 y$, and assuming Fourier modes of the form

$$\psi(x, y) = \hat\psi \exp[i(kx + ly - \omega t)], \tag{59}$$

we get a system of equations of the form

$$\begin{pmatrix} \omega(K^2 + k_d^2/2) + k\beta & -\omega k_d^2/2 & kK^2 B & 0 \\ -\omega k_d^2/2 & \omega(K^2 + k_d^2/2) + k\beta & 0 & kK^2 B \\ kB & 0 & \omega & 0 \\ 0 & kB & 0 & \omega \end{pmatrix} \begin{pmatrix} \psi_1 \\ \psi_2 \\ A_1 \\ A_2 \end{pmatrix} = 0, \tag{60}$$

with $K^2 = k^2 + l^2$. When there is no field, only the upper-left 2×2 block survives, and from solving this, we get the dispersion relation for the barotropic and baroclinic Rossby waves:

$$\omega_\psi = \frac{-k\beta}{K^2}, \quad \omega_\tau = \frac{-k\beta}{K^2 + k_d^2}. \tag{61}$$

We see again the baroclinic branch is affected by the Rossby deformation radius, and when we go back into the purely barotropic regime ($k_d = 0$ or $L_d = \infty$, the baroclinic mode becomes indistinguishable from the barotropic mode.

When we set $k_d = \psi_2 = A_2$, we go back into the 2D β-plane regime, with the dispersion relation

$$\omega^2 \pm \omega_{\mathrm{R}}\omega - \omega_{\mathrm{A}}^2 = 0, \tag{62}$$

so the Rossby branch is coupled with the Alfvén branch.

To find the waves in the two-level regime, we need to find the determinant of the matrix. The quartic is not easy to manipulate into a revealing form, and not having a computer program such as MAPLE at hand for checking made this a rather tedious task to do, and thus has been omitted at the time of writing. The obvious expectation is that we have the barotropic and baroclinic Rossby branch coupled to the Alfvén branch. The point of trying to do this analytically is that I am interested in how the baroclinic Rossby wave is affect by the magnetic field. We anticipate the barotropic case to be modified as before, but what happens to the baroclinic mode does not seem to have been extensively investigated. This is easy to do numerically and will be a future focus.

4.2. **Phillips problem.** There are certain problems that one could potentially solve at least semi-analytically. Three particularly simple examples for studying purely baroclinic instabilities in the QG (no field) regime are the Eady problem, Charney problem (for the continuous case), and the Phillips problem (for the two-level case). I had a quick look at the Phillips problem, and some notes are given below.

Linearising about $\Psi_1 = -Uy$, $\Psi_2 = Uy$, $A_1 = A_2 = -By$, where U and B are both just constants, the equation we are dealing with is of the form (now letting $\omega = kc$)

$$\begin{pmatrix} (U-c)(K^2 + Uk_d^2/2) - (\beta + k_d^2) & -(U-c)k_d^2/2 & -K^2B & 0 \\ -(U+c)k_d^2/2 & (U+c)(K^2 + k_d^2/2) + (\beta - Uk_d^2) & 0 & K^2B \\ -B & 0 & (U-c) & 0 \\ 0 & B & 0 & (U+c) \end{pmatrix} \begin{pmatrix} \psi_1 \\ \psi_2 \\ A_1 \\ A_2 \end{pmatrix} = 0. \tag{63}$$

$B = 0$ gives us the Phillips problem and the dispersion relation is

$$c = \frac{-\beta(K^2 + k_d^2/2) \pm \sqrt{K^8U^2 - K^4U^2k_d^4 + \beta^2 k_d^4/4}}{K^2(K^2 + k_d^2)}. \tag{64}$$

There are explicit criteria for certain cases. For example, if we had no differential rotation ($\beta = 0$, but note Rhines scale does not exist then), we see that we have stability when

$$K \leq k_d, \tag{65}$$

which is usually known as a high wavenumber cutoff criterion, so only the long enough waves can become unstable. When β is not zero, we see that we need a minimum shear of

$$U > \frac{2}{k_d^2}\beta \tag{66}$$

for instability. So the presence of differential rotation or a large Rossby deformation radius (small k_d) causes the flow to be more stable baroclinically. In general, there is a high wavenumber cutoff which may be found numerically, so only the

sufficiently long waves can be unstable and grow. We expect in general the system to be unstable, because the gradient of PV take opposite signs in the two layers, in accordance with the Charney-Stern-Pedlosky criterion (see Pedlosky 1987; cf. Rayleigh's criterion in classical hydrodynamics).

In the magnetic case, the Charney-Stern-Pedlosky criterion does not hold, so the system is not necessarily unstable even if the gradient of PV changes sign over the domain. Although we expect a magnetic field aligned with the flow to stabilise a barotropic flow, this may not necessarily be true for the baroclinic case, as potential energy also plays a role. Again, with something like MAPLE, we should be able to do the problem at least semi-analytically. The key thing to investigate is to see how the energy budget works, since we now have potential energy in addition to kinetic and magnetic energy.

5. Future work

This project actually links up very well to my current PhD project, where the general idea is to extend and study well known GFD models with MHD effects. Thus (having convinced the advisor) I can potentially dedicate the rest of the PhD period to actually finish this work off. Having Steve Tobias (Leeds) and David Hughes (Leeds, my PhD advisor) there will certainly allow me to attempt to finish the work and extend it accordingly.

Having this in mind, the list below is much longer and extensive than it otherwise would be. I shall include the more definite tasks first, then move on to more open questions that I have thought about.

5.1. **Closure calculation.** Much as we would like to predict the dynamics and spectral transfer of the relevant quantities in a turbulent regime, we cannot do it, since that would involve solving the problem of turbulence! One problem is that of closure, as we cannot close the hierarchy in a self-consistent way. So there are two approaches one could try: approximate the equation but keep the hierarchy (e.g. Direct Interaction Approximation), or approximate the hierarchy but keep the equation (e.g. Quasi-Normal approximation). None of the methods in the current literature work, and when they do work, it is usually regime dependent. Some give better results than others (or some are less wrong than others, depending on how one looks at it). One consistently 'successful' method that has been regularly used in both GFD and MHD regimes is the so called Eddy Damped Quasi-Normal Markovian approximation (EDQNM), and it is my intention to learn something about closure theory in order to perform such calculations. (See Orszag 1970 for a brief review).

Doing such approximations allows one to reach parameter regimes which are probably impossible to reach using a direct numerical simulation (e.g. Pouquet 1978), but of course the calculations resulting from the approximation may just be completely and utterly wrong. Wrong we may be, but we have do at least try something! It may suggest things we should look out for in a direct numerical simulation.

5.2. **Numerical simulation.** The obvious thing to do alongside the analytical work is of course the simulation. Having absolutely no experience with programming or with programming languages (probably going to be Fortran or C), I expect it will take me around three or so months to get familiar with the language, then actually programming it and running the simulations should take a few more months. I expect there may well be some sort of numerical result by no later than this time next year. The nice thing about working in QG regimes and β-plane is that the domain is simple enough that spectral methods is easily implemented, but also the QG regime filters out fast unbalanced gravity waves, allowing a more sensible time-stepping to be used.

5.3. **Relaxing the assumptions.** By being in the QG regime, we essentially get a quasi- two dimensional problem. In particular, the tachocline is not necessarily well described by the QG approximation; Rossby number is probably around $\mathcal{O}(1 - 10^{-1})$, so dynamics are rotationally influenced but not rotationally dominant. In particular, we have seen that the QG equations do not allow a dynamo to function, which is probably not the case for the tachocline, where we expect it to play a role in the interface dynamo as part of the Ω-effect. Would the same conclusions hold if we relax the QG approximation?

Taking a bold guess, I expect that zonal flows will not form, and almost everything in the QG regime should carry over to the more general case. The point is that once we allow the third dimension back in, we do not have enstrophy conservation even in the hydrodynamic case, and no inverse cascade occurs. Also, the presence of the field should still Alfvénise the system, turbulence chopping the large-scale weak field into a strong field at the relevant scale, which will cause stress cancellation, thus reduce the effectiveness of the turbulent viscosity (which should still be anisotropic due to the stratification), and quench momentum diffusion; although Zel'dovich's theorem no longer holds in 3D, it is well observed numerically that small-scale fluctuations tend to be strong. Of course, the controlling parameter for zonal flow formation may then be different, but this is expected as there are less restrictions imposed. Presence of field inhibits PV mixing, also suggesting the lack of zonal flow formation. By going into the 3D regime we may end up getting a dynamo, but that should not affect the zonal flow formation question. An interesting question to ask is whether we get zonal magnetic bands from small scale turbulence (cf. dynamo problem).

I propose first to do similar calculations for the continuous QG case, then try and investigate the layered shallow-water cases, where rotation is not necessarily dominant. We anticipate an affirmative answer with the shallow-water equations, as there is already a simulation for the spherical case for the magnetic shallow-water equations (Staehling & Cho, unpublished; see James Cho's talk given at the Issac Newton Institute, for the *High Reynolds number turbulence* workshop). In due course I shall move onto the 3D Boussinesq case, doing similar calculations and simulations, as well as developing the theory on the way.

5.4. **Collection of further questions.** What is the parameter that controls whether zonal flows actually form? Tobias *et al.* (2007) observed that it should be B_0^2/η, where B_0 is the magnitude of the imposed weak field. Could we possibly do better and determine this from analytical work? Should the same parameter crop up if we go into different regimes?

One of the more obvious problems is that we have inverse cascade of magnetic potential squared, so do we expect zonal magnetic bands to form? If so, what are their effects on the whole system in general?

If we relax the QG approximation and go into 3D Boussinesq regime, do we still get the same results? Zel'dovich's theorem does not hold anymore, although it is well observed that small scale fields can get strong. There is also the added bonus of the dynamo which can come into play, regenerating fields which feed back onto the flow, complicating matters further.

For the Solar problem, if above mentioned processes occur, what are their roles in the larger picture, for example, in affecting the differential rotation profile, confinement of the tachocline, or in the dynamo problem?

There are many problems, but no simple answers. It is my hope that I will be able to at least answer a (small) portion of it within my PhD period.

- - - - Julian Mak, August 24, 2010.

REFERENCES

CHARNEY, J. G. 1948. On the scale of atmospheric motions. *Geophys. Publ. Oslo*, **17**, 1–17.

CHARNEY, J. G. 1949. On a physical basis for numerical prediction of large-scale motions in the atmosphere. *J. Meteor.*, **6**, 371–385.

CHARNEY, J. G. 1971. Geostrophic turbulence. *J. Atmos. Sci.*, **28**, 1087–1095.

DIAMOND, P. H., ITOH, S. I., ITOH, K., & SILVERS, L. J. 2007. β-plane MHD turbulence and dissipation in the solar tachocline. *In "The Solar Tachocline", Ch.9, CUP, eds. Hughes et al. (2007)*, 211–239.

GARAUD, P. 2003. Dynamics of the solar tachocline. *in 'Stellar Astrophysical Fluid Dynamics', editor M. J. Thompson, J. Christensen-Dalsgaard*, 131–143.

GILMAN, P. A. 1967a. Stability of baroclinic flows in a zonal magnetic field: Part I. *J. Atmos. Sci.*, **24**, 101–118.

GILMAN, P. A. 1967b. Stability of baroclinic flows in a zonal magnetic field: Part II. *J. Atmos. Sci.*, **24**, 119–129.

GILMAN, P. A. 1969. Baroclinic, Alfvén and Rossby waves in geostrophic flow. *J. Atmos. Sci.*, **26**, 1003–1009.

GOUGH, D. O., & MCINTYRE, M. E. 1998. Inevitability of a magnetic field in the Sun's radiative interior. *Nature*, **394**, 755–757.

HUGHES, D. W., ROSNER, R., & WEISS, N. O. 2007. *The Solar Tachocline.* CUP.

MIESCH, M. S. 2007. Turbulence in the tachocline. *in 'the Solar Tachocline', editor D. W. Hughes, R. Rosner, N. O. Weiss*, 109–128.

ORSZAG, S. A. 1970. Analytical theories of turbulence. *J. Fluid Mech.*, **41**, 363–385.

PEDLOSKY, J. 1987. *Geophysical Fluid Dynamics.* 2nd edn. Springer-Verlag.

POUQUET, A. 1978. On two-dimensional magnetohydrodynamic turbulence. *J. Fluid Mech.*, **88**, 1–16.

RHINES, P. B. 1975. Waves and turbulence on a β-plane. *J. Fluid Mech.*, **69**, 417–443.

RHINES, P. B. 1979. Geostrophic turbulence. *Ann. Rev. Fluid Mech.*, **11**, 401–441.

RHINES, P. B., & HOLLAND, W. R. 1979. A theoretical discussion of eddy-driven mean flows. *Dyn. Atmos. Oceans*, **3**, 289–325.

SALMON, R. 1978. Two-layer quasi-geostrophic turbulence in a simple special case. *Geophys. Astrophys. Fluid Dyn.*, **10**, 25–53.

SALMON, R. 1980. Baroclinic instability and geostrophic turbulence. *Geophys. Astrophys. Fluid Dyn.*, **15**, 167–211.

SALMON, R. 1998. *Lectures on Geophysical Fluid Dynamics.* OUP.

SPIEGEL, E. A., & ZAHN, J. P. 1992. The Solar tachocline. *Astron. Astrophys.*, **265**, 106–114.

TOBIAS, S. M., BRUMMEL, N. H., CLUNE, T. L., & TOOMRE, J. 1998. Pumping of magnetic fields by turbulent penetrative convection.

TOBIAS, S. M., DIAMOND, P. H., & HUGHES, D. W. 2007. β-plane

magnetohydrodynamic turbulence in the Solar tachocline.

TULLOCH, R., & SMITH, K. S. 2009a. A note on the numerical representation of surface dynamics in quasigeopstrophic turbulence: Application to the nonlinear Eady model. *J. Atmos. Sci.*, **66**, 1063–1068.

TULLOCH, R., & SMITH, K. S. 2009b. Quasigeostrophic turbulence with explicit surface dynamics: Application to the atmospheric energy spectrum. *J. Atmos. Sci.*, **66**, 450–467.

TUNG, K. K., & WELCH, W. T. 2001. Remarks on Charney's note on geostrophic turbulence. *J. Atmos. Sci.*, **58**, 2009–2012.

TUNG, K. K., & WELCH-ORLANDO, W. 2003. The k^{-3} and $k^{-5/3}$ energy spectrum of atmospheric turbulence: Quasigeostrophic two-level model simulation. *J. Atmos. Sci.*, **60**, 824–835.

VALLIS, G. K. 2006. *Atmospheric and Oceanic Fluid Dynamics*. CUP.

VALLIS, G. K., & MALTRUD, M. E. 1993. Generation of mean flows and jets on a beta plane and over topography. *J. Phys. Oceanogr.*, **23**, 1346–1362.

WOOD, R. B., & MCINTYRE, M. E. 2010. A general theorem on angular-momentum changes due to potential vorticity mixing and on potential-energy changes due to buoyancy mixing. *J. Atmos. Sci.*, **67**, 1261–1274.

WOOD, T. S. 2010. *The Solar Tachocline: A Self-Consistent Model of Magnetic Confinement. Cambridge, University Library, Superintendent of Manuscripts: PhD thesis.*

[†]DEPARTMENT OF APPLIED MATHEMATICS, UNIVERSITY OF LEEDS, LEEDS, UK. LS2 9JT