Reply

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1. Introduction

Surface quasigeostrophic (SQG) turbulence (Held et al. 1995) is not an exotic geophysical effect but rather a generic description of quasigeostrophic (QG) turbulence at vertical surfaces of sharp change in the mean environment (vertical boundaries are an extreme example). Tulloch and Smith (2006, 2009) argued that surface quasigeostrophic effects are consistent with many perplexing features of the observed atmospheric energy spectrum (Nastrom and Gage 1985). In the first paper, Tulloch and Smith (2006) considered a ''toy'' model consisting of SQG flow overlying a finite-depth region of zero potential vorticity (PV), thereby including ''interior'' dynamics in the barest way possible. The energy spectrum that results from forcing this system at large scales nevertheless exhibits a spectral break from a -3 slope to a $-5/3$ slope and a forward cascade of energy at the upper surface, both qualitatively consistent with observations at the tropopause. However, the model is far too simple to be taken as quantitatively descriptive, and its flaws are amply described the paper. Tulloch and Smith (2009) developed a more complete but still very idealized model that includes active surfaces coupled to two interior modes. The model allows a mean baroclinic flow and thus baroclinic instability but assumes constant buoyancy frequency N, rigid boundaries above and below, horizontal homogeneity, and other simplifications, not the least of which is that it is still strictly quasigeostrophic. The goal of the papers is to suggest how synoptic-scale baroclinic instability, combined with the sharp jump in N at the tropopause, can lead to SQG effects that are consistent with observations, not to provide quantitatively descriptive models, nor to suggest that SQG dynamics is solely responsible for the shallowing of the spectrum in the mesoscales.

Lindborg (2009) implicitly assumes that Tulloch and Smith (2006, 2009) proposed their models as complete descriptions of the observations. Based on this mistaken perception, he explicitly criticizes the idea that the models of Tulloch and Smith explain the atmospheric spectrum on two counts:

1) the Rossby number in the mesoscales is too large for any quasigeostrophic model to be applicable; and
2) the mesoscale $-5/3$ spectrum would be confined to a very thin layer near the tropopause—too thin to be consistent with observations.

The former is a criticism of any application of quasigeostrophic theory to atmospheric mesoscales, while the latter is directed at the specific vertical structure of the energy spectrum that arises in the simplest possible application of the models suggested in Tulloch and Smith (2006, 2009). Here we put aside, for the moment, the mistaken premise that these models are to be taken literally and attempt to refute these criticisms as stated.

2. The Rossby number and quasigeostrophy

The quasigeostrophic approximation applies to flows with length L, depth H, and velocity U that satisfy small Rossby numbers (Ro $= U/fL$, with f the Coriolis parameter) and $O(1)$ Burger numbers (Bu $= fL/NH$). In a turbulent cascade, energy will move to larger scales (the inverse cascade) and enstrophy to smaller scales (the forward cascade), but in an unbounded domain, they may each do so in a way that maintains the required parameter regime (i.e., if U/L and L/H remain constant). This is not possible in a vertically bounded domain: in the inverse cascade, the energy will violate the Burger number constraint because H will eventually be set by the vertical extent of the domain, while L may continue to increase; in the forward cascade near the

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boundaries (or a jump in N, as occurs at the tropopause), temperature is roughly conserved within filaments, but the vorticity increases as the filament is stretched (Held et al. 1995), leading to an increase in the Rossby number.

In the earth's atmosphere, midlatitude baroclinic eddies satisfy Bu $\sim O(1)$ because the ratio of their horizontal scale (set by friction, orography, and the size of the planet) to their vertical scale (set by the height of the tropopause) is not much larger than N/f. [Schneider (2004) suggests that this is not an accident.] Because baroclinic instability injects energy near the eddy scale, the inverse cascade is severely truncated; thus for eddy energy the quasigeostrophic approximation remains well posed. In the forward cascade, however, scales can decrease by orders of magnitude, and given the arguments above, there is nothing to restrict the flow near the tropopause to remain in the quasigeostrophic regime. This does not mean, of course, that the quasigeostrophic approximation is invalid everywhere. Rather, it suggests that ageostrophic motions may be generated by a cascade that started in the quasigeostrophic regime and that one must be careful in diagnosing whether motions at a given scale in the cascade are balanced.

Lindborg defines the Rossby number as

$$\mathrm{Ro}_\zeta = \frac{\zeta_{\mathrm{rms}}}{f},$$

where ζ is the vertical component of the vorticity and $\zeta_{\mathrm{rms}} = \sqrt{\langle \zeta^2 \rangle}$ is its root-mean-square (rms) value. Assuming a rotational energy spectrum $E_R(k)$, one obtains an expression for ζ_{rms} given by Eq. (3) of Lindborg's comment. Taken literally, ζ_{rms} is divergent for any energy spectrum with slope shallower than or equal to -3. In the presence of viscosity or any finite range of wavenumbers, ζ_{rms} will be dominated by the smallest scale in the flow.[1] Using the $-5/3$ mesoscale spectrum, this definition of the Rossby number is essentially the Rossby number of the smallest-scale motions. Not surprisingly, when Lindborg considers a minimum *wavelength* of 2 km ($k_{\min} = 0.0005\ \mathrm{km}^{-1}$), he finds $\mathrm{Ro}_\zeta = 9.1$. By choosing a sufficiently small scale at which to truncate the cascade, quasigeostrophy is invalidated for the entire forward cascade.

[1] Waite and Bartello (2006) take this into account in their study of rotating stratified turbulence, denoting the parameter Ro_ζ the "microscale Rossby number," favoring instead for their analysis a "macroscale Rossby number" determined by the energy. Only for energy spectra with slopes steeper than -3 will the macroscale and microscale Rossby numbers be similar.

A more rational approach is to consider the Rossby number at each scale separately and ask at what scale the Rossby number ceases to be small. More crucially to the present problem, what is the Rossby number at the transition scale where the atmospheric energy spectrum flattens from a -3 slope to a $-5/3$ slope? The transition occurs at a wavelength of about 600 km. Taking $k_{\min} = 1/700\ \mathrm{km}^{-1}$ and $k_{\max} = 1/500\ \mathrm{km}^{-1}$ yields $\mathrm{Ro}_\zeta = 0.3$. This is not small, but not large either, and is certainly not far from the typical atmospheric generic minimum of about 0.1. Moreover, Hamilton et al. (2008) shows that divergent energy is an order of magnitude smaller than rotational energy near the transition scale, consistent with the dominance of balanced flow at these scales.

Interestingly, Lindborg cites Klein et al. (2008) as using a definition of Rossby number similar to his own. This is true, but this paper is worthy of broader consideration with respect to the present problem. Klein et al. find that Ro_ζ reaches about 0.3 near the upper surface in their simulations, that the horizontal velocity is almost entirely slaved to the density field, and that the spectrum of kinetic energy at the surface is shallow. Klein et al. argue that the latter two characteristics are consistent with SQG dynamics occurring at the surface. Simultaneously, however, they also find that there are significant ageostrophic effects, namely strong vertical velocities occurring through frontogenesis. The major point of the paper is to show that these balanced and unbalanced effects coexist at oceanic submesoscales (equivalent in a scaling sense to the atmospheric mesoscales considered here). Therefore, the Rossby number in the transition region does not at all preclude the significance of SQG dynamics.

Lindborg (2006) proposes that the mesoscale $-5/3$ spectrum is due to a forward cascade in stratified turbulence. This may be an accurate description of the flow at sufficiently small scales, but one still needs to understand the transition to this flow regime, if it is relevant anywhere. Here we suggest that SQG dynamics may act as the missing link, providing a balanced mechanism by which such unbalanced motions could be generated.

3. The vertical structure of the energy spectrum

Tulloch and Smith (2006, 2009) consider the effects of a rigid boundary at the upper surface and take N to be constant below it. As stated earlier, these assumptions are unrealistic. Lindborg points out that when using the vertical structure of only the surface mode, the $-5/3$ portion of the energy spectrum is trapped in an increasingly narrow layer near the upper boundary as the horizontal wavenumber increases. This is an unrealistic side effect of our unrealistic model conditions, but we remind the reader of the main point of Tulloch and Smith (2009): the spectrum is the sum of interior and surface contributions, so that even in our unrealistic model, the rolloff from the tropopause spectrum is bounded by a -3 slope.

Moreover, when turbulence in the presence of a realistic $N(z)$ and mean shear $U(z)$ is considered, the transition in depth is likely blurred more. New simulations at high vertical resolution, using a profile of $N(z)$ that includes a jump from tropospheric to stratospheric values, and a realistic profile of mean velocity will be reported on in a future publication. A heuristic argument, however, demonstrates part of the expected result.

Consider a stratification profile that mimics the transition from tropospheric to stratospheric values of N^2, such as

$$N^2(z) = N_0^2 + N_d^2 \tanh\frac{z-H}{\delta}, \tag{1}$$

where δ is the depth over which the profile changes and $N_0^2 = (N_S^2 + N_T^2)/2$, $N_d^2 = (N_S^2 - N_T^2)/2$, with N_T^2 and N_S^2 representing typical tropospheric and stratospheric values, respectively. Now consider the effect on the QG PV, which can be expanded as

$$q = \nabla^2\psi + \underbrace{\frac{\partial}{\partial z}\left(\frac{f^2}{N^2}\right)\psi_z}_{\mathrm{I}} + \underbrace{\frac{f^2}{N^2}\psi_{zz}}_{\mathrm{II}}.$$

In the limit $\delta \to \infty$, $N(z) \to N_0$ (a constant) and term I vanishes. In this case, one can rescale z by N_0/f, so that the inversion from q to ψ is a 3D elliptic problem, leading to the predictions of Charney (1971), with a forward cascade of enstrophy and a -3 slope kinetic energy spectrum. In the opposite limit, $\delta \to 0$, the profile of $N(z)$ takes on a discontinuous jump at $z = H$, from N_T to N_S. In this case, by integrating over the discontinuity and assuming $q = 0$ above and below $z = H$ (equivalent to considering only the surface mode), one recovers exact SQG dynamics at $z = H$, as shown by Held et al. (1995, end of section 2). It is term I, which is proportional to the temperature, that controls the dynamics in this limit.

Given δ, at what horizontal scale will term I cease to affect the inversion from q to ψ? The surface mode has $\psi \sim e^{-NK|z-H|/f}$, which implies $\psi_{zz} \sim (NK/f)\psi_z$. Using this approximation, terms I and II will be on the same order when

$$K \sim K_c \equiv \frac{N_d^2 f}{\delta} N^{-3}(z)\,\mathrm{sech}^2\frac{z-H}{\delta}. \tag{2}$$

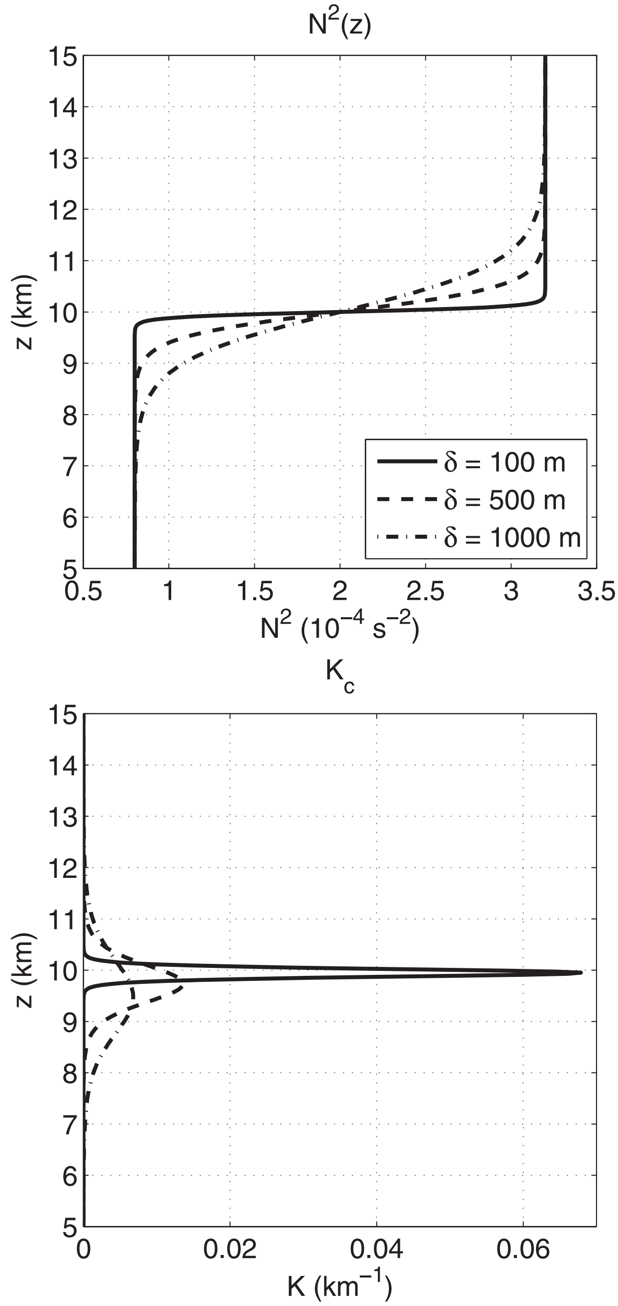

FIG. 1. (top) Theoretical $N^2(z)$ profiles from (1), with values of δ stated in the legend. (bottom) Critical wavenumber K_c from (2) for same values of δ listed in the legend of the top panel.

The profiles of $N(z)$ from (1) and the quantity K_c are plotted in Fig. 1, assuming $f = 10^{-4}\ \mathrm{s}^{-1}$, $N_T = 10^{-2}\ \mathrm{s}^{-1}$, and $N_S^2 = 4N_T^2$, and taking a range of values for δ (shown in the legend). The curve is peaked near or just below the "tropopause" $z = H$ and drops to zero over some distance above and below its peak. At some K sufficiently large compared to K_c, term I will cease to affect the QGPV inversion. Recall also that at sufficiently small K the dynamics are barotropic, as argued in Tulloch and Smith (2009); thus, at a given z, there is a range of K bounded above and below in which temperature dynamics will affect the inversion. More to the point, at a given K there is a range of z for which temperature dynamics is important. Consider, for example, the case with $\delta = 500$ m: at $K = 0.01\ \mathrm{km}^{-1}$ (a wavelength of about 600 km), one must move approximately 1 km from $z = H$ to suppress term I. At the same wavenumber, but with $\delta = 100$ m, the range of z for which term I is active is much smaller and approaches 0 as δ vanishes.

Again, the point here is not to supply a conclusive argument but rather to point out that a more realistic stratification profile will quantitatively alter the spectrum of kinetic energy. A more realistic mean velocity profile will further affect the results, as will the presence of other ''interior'' modes in the problem. Therefore, there is no reason to conclude that the inability of the surface mode structure proposed by Tulloch and Smith (2009) to exactly match observations should disprove the idea that SQG dynamics plays a role in the observed spectral transition.

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