Equatorial Superrotation in Earth-like Atmospheric Models

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We have come to expect that our climate models will produce circulations that closely resemble the observed circulation. Yet, there are occasional references in the literature to Earth-like atmospheric models that produce a circulation of a very distinctive kind – one with strong equatorial superrotation in the tropical upper troposphere. I have encountered this mode of circulation several times, often when least expected.

Superrotation refers to zonal mean winds with angular momentum, M, that is greater than the angular momentum of the surface at the equator, $M_0 = \Omega a^2$, where Ω is the angular velocity and a is the radius of the Earth. If U is the zonal mean zonal wind in a frame rotating with the angular velocity of the surface, then $M = acos(\theta)(\Omega acos(\theta) + U)$, and $M > M_0$ requires $U > U_m$, where

$$U_m \equiv \Omega sin^2(\theta)/cos(\theta) \quad (1)$$

Figure 1 contains a plot of U_m along with a schematic plot of the observed upper level zonal winds in the atmosphere. Also pictured is the kind of superrotating flow that is the subject of this lecture.

A zonally symmetric circulation in the meridional-vertical plane conserves M following the flow, in the absence of torques. In the presence of torques generated by eddy angular momentum fluxes, it is easy to see that a local interior maximum in M cannot be maintained as long as these torques are all directed down the mean angular momentum gradient. In this case the maximum in M must occur on the surface, which implies that the maximum must be at most M_0. As Hide (1969) pointed out (see also Held and Hou, 1980), countergradient eddy angular momentum fluxes are required to maintain equatorial superrotation. And if we are talking about superrotation, we might as well talk about *equatorial* superrotation. If the flow where superrotating off the equator, but not on the equator, it would be inertially unstable and could not be maintained without extraordinarily strong forcing. Angular momentum is required to decrease with increasing latitude on an isentropic surface if the flow is to be inertially stable.

Of course, the Earth's stratosphere superrotates in the westerly phase of the QBO, due to the countergradient vertical angular momentum transport by Kelvin and eastward propagating gravity waves. And Jupiter and Saturn are famous for their equatorial superrotation (the "surface" rotation for a gas planet is the deep interior rotation rate as inferred from the rotation of the magnetic field). Even more dramatically, the "4-day rotation" of the Venusian atmosphere seems to bear no relation at all to the surface rotation. Even our Sun superrotates at its equator, as compared to its deep interior rotation rate inferred from helioseismology. So perhaps countergradient eddy angular momentum fluxes are the rule rather than the exception. Yet in the Earth's troposphere, the inequality $M < M_0$, or, equivalently, $U < U_m$, is well-satisfied. This is to be expected as long as the dominant eddy fluxes in the troposphere, the small-scale vertical stresses near the surface and the large-scale quasi-horizontal angular momentum fluxes concentrated near the tropopause, are all down-gradient (an exception being the small equatorward horizontal fluxes in subpolar latitudes).

We sometimes do speak of the large-scale horizontal eddy angular momentum fluxes as being "countergradient", but in the context of energy transfer from the eddies to the mean flow, which is determined by the gradient of *angular velocity*, $\propto U/cos(\theta)$, not the gradient of *angular momentum*. The dominant feature of the eddy angular momentum fluxes, the poleward flux in the subtropical upper troposphere, is directed up the angular velocity gradient but down the angular momentum gradient. The smaller equatorward fluxes in subpolar latitudes happen to be directed up the angular momentum gradient but can play no role in generating superrotation since the angular momentum involved is so small. The latter do serve to remind us, however, that there is no fundamental reason why these horizontal fluxes, generated by midlatitude baroclinic eddy production, need be directed down the angular momentum gradient.

As a graduate student in the 1970's, I had the good fortune to share an office with Max Suarez, and we both leared a great deal by collaborating on the construction of an atmospheric general circulation model. It was an idealized moist primitive equation model on the sphere, two levels, finite differenced in the meridional direction and spectral in the zonal direction, and then severely truncated to retain only a few zonal wavenumbers (typically 0-3-6 or 0-3-6-9). We managed to generate some Earth-like circulations, which allowed us to write a few papers, but we also found some solutions that superrotated in the upper level of the model. In Held and Suarez (1978), in a section of the paper entitled a "note of caution", we wrote

> *The model used in this study exhibits a very peculiar behavior in certain cases ... after several hundred days of integration, the eddy kinetic energy very slowly begins to decay, temperatures drift towards radiative equilibrium, surface winds decrease in strength, and the subtropical jets drift slowly equatorward. Various experiments were performed with different subgrid scale mixing and*

time finite-differencing in trying to understand this behavior. We found, surprisingly, that if we changed the procedure for convective adjustment by relaxing the fields to their adjusted values rather than adjusting instantaneously, these models quickly return to statistically steady states resembling (observations)

The slow drift of the subtropical jets eventually resulted in an upper level flow that more resembled solid body rotation. We were clearly confused, and did not focus at that time on the eddy angular momentum fluxes. We naturally assumed that we were doing something fairly stupid. In any case, how interesting could it be that generation of this unusual circulation was dependent on an obscure detail of the convective adjustment algorithm? In fact, we both eventually came to feel that there *was* something very interesting in this behavior.

Max returned to this problem with Dean Duffy and in a key paper (Suarez and Duffy, 1992; hereafter SD) helped explain the transition to a superrotating state by utilizing a simpler dry two-level model, forced by a specified zonal wavenumber 2 heating field in the tropics. This model has no seasonal cycle and no land-sea contrast. The transition to superrotation occurs in the model as the strength of the asymmetric component of the tropical heating is increased. A figure from SD is reproduced here in Figure 2. The zonally averaged zonal wind at the equator in the upper layer of the model is shown as a function of time and of the strength of the asymmetric part of the tropical heating.

To understand this result and its relationship to the behavior we obtained accidentally as graduate students with a moist model, we need to be aware of two fundamental properties of Rossby waves:

1) *When Rossby waves are forced at one latitude and propagate to some other latitude where they are dissipated, they decelerate the zonal flow (*$\partial U/\partial t < 0$*) in the dissipation region, and they accelerate the flow (*$\partial U/\partial t > 0$*) in the source region.*

As an example, suppose that Rossby waves are preferentially excited in midlatitudes and then propagate to some extent into lower and higher latitudes before being dissipated. The result will be a zonal acceleration, or a convergence of eddy angular momentum fluxes, in midlatudes, and deceleration to the north and south, just as observed in our atmosphere. On the other hand, if there is a Rossby wave source near the equator, and this wave propagates into midlatitudes before being dissipated, then countergradient angular momentum fluxes will be generated so as to accelerate the equatorial zonal winds.

This is not the place to try to discuss this property of large-scale flows in all of its generality. For starters, we can just think about the simplest steady Rossby wave on a β-plane. One can show quite easily and directly that the meridional eddy momentum flux due to the wave is opposite in direction to the meridional group velocity of the wave. Therefore, the wave's momentum flux will be directed from the sink to the source, resulting in westerly acceleration in the source region, and easterly acceleration near the sink.

Rather than think about group velocities, one can also start with Stokes' Theorem and vorticity conservation. Since the zonal mean zonal flow is proportional to eh circulation around a latitude circle, it is also proportional to the vorticity integrated over a polar cap bounded by this latitude circle. Consider a Rossby wave packet that propagates from its source into a previously quiescent region. Focusing on a latitude circle within the latter region, the entrance of the wave will cause some of the high vorticity north of this latitude to move southward, and some of the low vorticity to move north. This reduces the vorticity within the polar cap bounded by this latitude circle, and, therefore, must result in easterly acceleration ($\partial U/\partial t < 0$). By a similar argument, there is westerly acceleration in the region vacated by the wave.

Note that it is not the dissipation of the wave in the sink region that produces the easterly acceleration. The easterly acceleration is produced as the wave enters. The role of dissipation is to prevent the equal and opposite westerly acceleration that would occur if the wave is allowed to leave. Note also that while we continue to use the term "wave" in this circulation argument, in fact the argument makes no reference to the disturbance being of small amplitude. The generalization of this picture to baroclinic flows, motivated originally by stratospheric issues, is discussed, for example, in Edmon, et. al. (1980).

How is this property of Rossby waves related to the behavior that Max and I observed in our two-layer model? Our current understanding is that the convection scheme in that model is the source of a lot of noise in the tropics, perhaps due to the severe spectral truncation in conjunction with the two-layer approximation and the particular convection scheme employed. This noise in turn serves as a source of Rossby waves that propagate out of the tropics. Some of these waves dissipate in midlatitudes, resulting in deceleration of the mean flow there and acceleration in the tropics. The underlying dynamics appears to be very similar to that in the model of Suarez and Duffy. Rather than the source of the Rossby waves being poorly characterized noise, SD explicitly force a stationary Rossby wave from the tropics by specifying zonal asymmetries in the tropical heating.

2) *Rossby waves will preferentially break and dissipate in those regions where $U - c$ is small, where c is the phase speed of the wave.*

The simplest way of justifying this claim is to consider the case of a steady wave. Putting ourselves in a reference frame moving with the wave, the mean flow will be $U - c$. If the eddy zonal velocity u' is larger than $U - c$, the streamlines of the flow will overturn in the horizontal plane. Since the flow is steady in this frame, streamlines are also trajectories and vorticity contours. In a realistic situation, the wave will not actually be steady, and the overturning vorticity contours will likely evolve into turbulent vorticity mixing. If the wave is infinitesimal, this picture implies that the wave will break in the immediate vicinity of its critical latitude, where $U = c$. Waves of finite amplitude will break before they reach their critical latitude.

Fig 3 is taken from Randel and Held (1991). Here the observed space-time spectrum of the eddy momentum flux at 200mb has been organized into a phase speed spectrum at each latitude

$$\overline{u'v'}(y) = \int M(y, c)\, dc \tag{2}$$

The plot shows $M(y, c)$ for DJFM on the left and JJAS on the right, and also shows the climatological zonal mean zonal winds at this level. One sees, first of all, that all of the momentum flux is carried by Rossby wavelike disturbances that propagate westward with respect to the upper level mean flow. The sign of the momentum flux signifies an equatorward group velocity on the equatorward sides of the two midlatitude baroclinic zones, and weaker poleward propagation poleward of these zones, the latter more clearly seen in the Southern hemisphere. Very little of the wave activity responsible for the momentum flux crosses the equator. Also, disturbances with smaller phase speeds propagate further into the tropics before being dissipated, as anticipated from the simple wave breaking arguments. Most of the momentum flux is carried by disturbances with phase speeds of 5-10 m/s.

Why is there an abrupt transition in the SD model from the normal state of the circulation (N) with weak winds in the tropical upper troposphere, to the superrotating state (S)? In the N state, the large downgradient eddy momentum flux from the tropics to midlatitudes provides a strong drag on the zonal flow in low latitudes. This momentum flux is produces by eddies generated in midlatitudes that break, or are sheared out by the winds, in lower latitudes. Now suppose that some process introduces a westerly perturbation to the zonal mean flow in the tropical upper troposphere. The characteristic phase speeds of the eddies responsible for the momentum transport are more or less unchanged, as these are set by the generation process in midlatitudes. Therefore, some fraction of the Rossby wave activity, that part with relatively small phase speeds, will now be less likely to break in the tropics, since $U-c$ will be larger for these waves. The drag associated with this wave breaking will be lost as well – i.e., these waves will propagate more easily into the opposite hemisphere where a substantial fraction of their wave activity will be dissipated. *An increase in U in the tropics makes the tropics more transparent to Rossby waves generated in midlatitudes, resulting in a reduction in the eddy momentum flux divergence.* In this way, the eddies provide a positive feedback to the original zonal wind perturbation in the tropics.

Positive feedback in itself need not create a sharp bifurcation to a new mode of circulation. But the strength of the feedback increases as U at the equator approaches the dominant phase speeds of the eddies. As it does so the feedback becomes strong enough to cause a runaway loss of wave drag and a runaway equatorial acceleration.

An additional factor is that the Rossby wave forced from the tropics can escape the tropics more easily once westerlies develop. Thus, the equatorial torque exerted by the rectified forced wave is itself expected to increases as the westerlies increase.

The SD model, although highly idealized in being dry and having only two levels, has the complex feature that its climate is not zonally symmetric. The asymmetric tropical heating results in the equatorial westerlies developing preferentially in certain regions (those in which the tropical heating $Q(x)$ is such that $\partial Q/\partial x < 0$). Indeed, these upper tropospheric westerly ducts occur in the observed atmospheric circulation, where they clearly do allow wave activity to propagate across the equator (Webster and Holton, 1982). But this zonal structure conplicates any attempt at analyzing these models and testing our understanding of the eddy feedbacks.

R. Saravanan (1993) studied essentially the same model, but in his thesis (Saravanan, 1990) he also considered a case in which, rather than forcing a Rossby wave with asymmetric tropical heating, he retained symmetric heating and simply accelerated the angular momentum in the tropics with an imposed torque. By varying the strength of this torque, a similar bifurcation to superrotation is generated which one can study in this simpler framework of a model with a zonally symmetric climate. This study also shows that the feedback between the equatorial westerlies and the forced wave, as opposed to the transient eddies forced in midlatitudes, is not an essential ingredient in the bifurcation.

The picture obtained by Saravanan is broadly consistent with that painted above, although it must be admitted that even in this simpler framework the reaction of the eddy dynamics to changes in the tropical winds is not fully understood. Also remaining somewhat mysterious are the balances that control the final S state and the extent of hysteresis. Having made the transition to the S state, can one decrease the strength of the tropical asymmetric heating, or the strength of the specified zonal torque in the simpler problem, and still remain on the S branch, rather than drop back down to the N branch? The answer is yes in these two-layer models, but the extent of this hysteresis is sensitive to horizontal resolution and subgrid scale diffusion in the modest resolution models analyzed to date.

Is this susceptibility to the superrotating state a property of a particular class of dry, two-layer models, or is it of more general relevance in models of earth-like atmospheres? At about the same time as Saravanan's work, Peter Phillipps and I decided to construct a "realistic" two-layer model (moist, with a seasonal cycle and realistic continental geometry) in order to address some issues related to the ice age problem. The choice of a two-level model was determined by the desire for efficiency; we wanted to generate a large number of climates. To our surprise, a version of this model that seemed at first glance to produce a normal climate, made a transition, when integrated long enough, into the superrotating state.

One reason that we were surprised by this result is that this moist model has a much stronger Hadley cell than the dry models discussed above. Because of the way that these dry models are forced, the dry static stability of the tropics is large – comparable to that in midlatitudes; in a moist model with about the same dry stability, the circulation must be much stronger in order to transport the same amount of energy polewards, since latent heat is being transported equatorwards. One could create a dry model with a strong Hadley cell, by forcing the model so that the dry stability of the tropics is small, but this is generally not done to avoid having to think about the deep dry convection that would result in the ITCZ.

In the presence of a stronger Hadley cell, our intuition is that it should be more difficult to destabilize the N state with westerly torques in the tropics. The upward motion in the Hadley cell, superficially at least, provides an additional drag on the upper tropospheric flow by bringing low angular momentum air up from the surface. Recent work by Karen Shell (personal communication) suggests, in fact, that the weakening of the Hadley circulation as the equatorial westerlies strengthen, and the consequent weakening of the drag on the upper troposphere associated with vertical advection, can itself cause a bifurcation to strong superrotation, in the absence of any feedback from altered eddy fluxes. The weakening of the Hadley cell as the equatorial westerlies increase can, in turn, be understood by modifying the Hadley cell model of Held and Hou (1980) so as to include non-zero equatorial winds.

And yet, the model that Peter and I intended for an ice-age study, with its strong Hadley cell, still made a transition to strong superrotation. The transition was spontaneous, requiring no additional Rossby wave source or zonal torques besides those generated by the model itself. Neither the seasonal cycle nor the realistic land-ocean geometry seemed to matter. The changes in the hydrologic cycle as the model went through this transition were profound.

We wrote no papers on this work. The model was relatively complicated and not particularly clean. We were not confident in our treatment of moist convection. Since we were aware of no reports of this kind of behavior from comprehensive atmospheric GCMs, we thought that perhaps our two-layer model was distinctive and unphysical in this regard. Is there something about the two-level approximation itself which causes the upper troposphere to become too transparent to Rossby waves as the zonal winds in the tropics are nudged towards stronger westerlies? Or was the tropics simply too noisy in this model once again?

When a Rossby wave propagates from midlatitudes into the tropics, we tend to focus on the winds in the tropical upper troposphere as most relevant for the wave structure, but the wave also encounters easterlies in the tropical *lower* troposphere. Shouldn't we expect a wave to break and be dissipated in the lower troposphere before reaching the deep tropics, since $U - c$ will vanish at low levels in the subtropics? Lee Panetta, Ray Pierrehumbert, and I looked at this problem in the context of a quasi-geostrophic two-layer model (Panetta et al, 1987). Specifically, we looked at the case of an external

Rossby wave propagating from a region where $U - c$ is large and positive in both layers into a region where $U - c$ crosses zero in the lower layer but remains positive in the upper layer. If the flow is slowly varying, one finds that the result depends on the strength of the vertical shear at the lower level critical latitude. If this shear is smaller than a critical value, then the wave does exhibit critical layer dynamics at this point, that is, it is expected to break and dissipate. On the other hand, above a particular value of the vertical shear, there is no wave absorption in the lower layer at all, and the Rossby wave propagates through the upper layer westerlies unscathed. This critical shear is precisely the same as that in Phillips' famous baroclinic instability criterion. In the supercritical case, the wave amplitude vanishes as the lower layer critical latitude is approached. Could it be that this perfect transparency in the supercritical case is a peculiarity of the two-level model, which results in a circulation that is too susceptible to this superrotation bifurcation?

The comparable analysis in a continuously stratified atmosphere is more complex. In an unpublished work, Fred Parham and Ray Pierrehumbert (personal communication, 1988) found that linear, dissipative, critical layer theory for a continuously stratified atmosphere always predicts some absorption in the scattering problem described above, no matter what the vertical shear. Small vertical scales, unresolved in the two-layer model, are involved in this process. When nonlinearities are allowed to develop, however, the absorption eventually disappears, at least in the case of a steady wavetrain. Thus, there are theoretical hints that the two-level model might indeed be too transparent to Rossby waves when the zonal winds in the upper but not the lower level are westerly with respect to the phase speed of the wave. The relevance of this work is still murky; in particular, it is not obvious that the scattering problem for external Rossby waves is the best starting point.

More recently, Dan Kosciecny, an undergraduate at Princeton, returned to this issue by looking for superrotating states in a model with relatively high vertical resolution. Max Suarez and I had earlier described a simple problem of a dry atmosphere with a zonally symmetric climate, forced by linear radiative relaxation and near-surface friction, that could be used to compare the "dynamical cores" of general circulation models. We tried to design the forcing and dissipation to create a circulation that looked realistic, but it must be kept in mind that this resemblance is rather superficial (for example, Max found that the error growth in this model is far slower than is that in more realistic models.) Following Saravanan, Dan added a prescribed zonally symmetric torque to the tropical upper troposphere of this model. I have followed this up more recently by using asymmetric tropical heating instead of an imposed torque (with T42 horizontal resolution and 20 levels in the vertical). Superrotating states are obtained, but we have found no clear example of abrupt bifurcation and hysteresis. In addition, the transition to superrotation has a somewhat different flavor. In the initial stage, an equatorial westerly jet is generated in the upper troposphere. This jet then merges with the subtropical jets to create the superrotating state. A two-stage evolution, the creation of an equatorial jet followed by jet merger, was much less clear in the two-layer models. Evidently, work remains to sort out possible distinctions between the behavior of two-layer and higher vertical resolution models in this context. Alan Plumb and Juno Hsu (personal communication, 1999) have also enountered strongly superrotating states in an idealized multi-layer model designed to study monsoonal circulations.

Given all of these experiences, I have found it surprising that other reports of abrupt transitions to strongly superrotating states in Earth-like circulation models have not appeared in the literature.

If this superrotation bifurcation does rear its head in comprehensive GCMs, it is safe to predict that its physical plausibility will hinge on questions of momentum mixing by moist convection. In several of the idealized cases that I have examined, one can always get rid of this behavior by adding enough vertical mixing of momentum in the tropics. Momentum mixing by moist convection remains a poorly understood process One can argue that its importance for the present climate is modest because convection tends to be localized in regions of small vertical shear, where modifying the vertical mixing of momentum does not change the mean flow dramatically. However, when we consider the possibility of equatorial superrotation and the large shears associated with it, issues of momentum mixing by convection will inevitably rise to the foreground.

To illustrate some of the subtleties involved, consider the idea, discussed earlier, that a stronger Hadley cell would discourage superrotation due to the vertical transport by the "mean" flow of low angular momentum from the surface to the upper troposphere. Upward "mean" flow in the tropics is but the statistical residue of the much larger motions in convective cores. As there is no well-defined "large-scale" upward motion, there is no compelling reason to think that a term such as $\overline{w}\partial\overline{u}/\partial z$ captures a well-defined part of the momentum transport. Even if one's model contains no other parameterization of convective momentum transpot, this large-scale term alone must be considered as a (rather arbitrary) parameterization, which could, potentially, be exaggerating the momentum coupling between upper and lower troposphere.

Could equatorial superrotation be of relevance in the context of global warming? After all, one could argue that a warmer climate results in stronger latent heating in the tropics, and that gathering this increased heating into a localized region would result in an increased tropical Rossby wave source. Some increase in tropical upper tropospheric westerlies is probably to be expected on this basis. Indeed, Huang, et. al., (2000) show weak westerlies at the equator developing in a GCM simulation of global warming. I do not feel that it is entirely inconceivable that strong positive eddy feedbacks could make this increase in westerlies surprisingly abrupt at some point, perhaps in the distant future. Max and I discussed this possibility when the SD model results were first obtained. It is one of the reasons that my interest in this topic has been sustained over the years. The absence of any support from existing climate models is balanced by scepticism in my own mind that the vertical momentum transport by convection is treated adequately in these models. It is a centrally important problem in geophysical fluid dynamics to determine how easy or difficult it is to force fundamental changes in atmospheric circulation. On the other hand, I would strongly discourage drawing attention to this possibility in a global warming forum until a comprehensive climate model were found to undergo a transition to strong superrotation for plausible changes in forcing.

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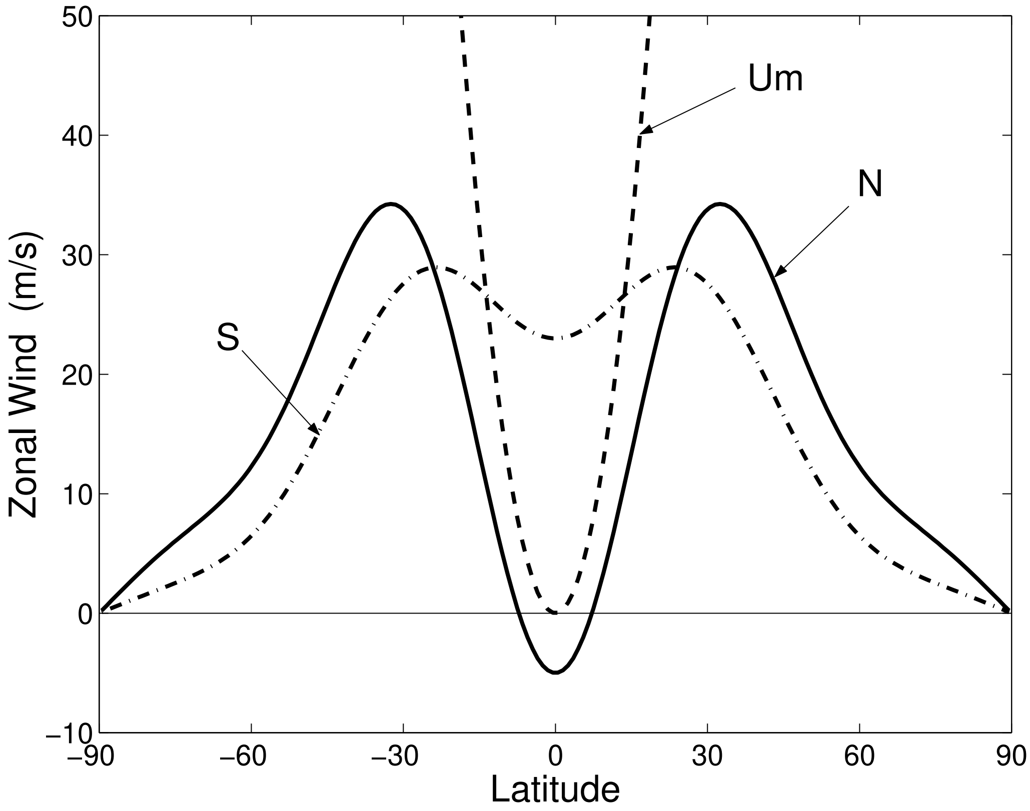
Figure Captions

Figure 1: A plot of the angular momentum conserving wind field Um, an upper tropopsheric wind field typical of the normal climatological state (N), and a superrotating state (S)

Figure 2: A space-time spectrum of the eddy momentum flux at 300mb in DJF. Also shown is the climatological zonal flow at that level. From Randel and Held (1991).

Figure 3: Zonal mean wind in the upper layer of a two layer model, as a function of time, for different strengths of the asymmetric tropical heating. From Suarez and Duffy (1992).



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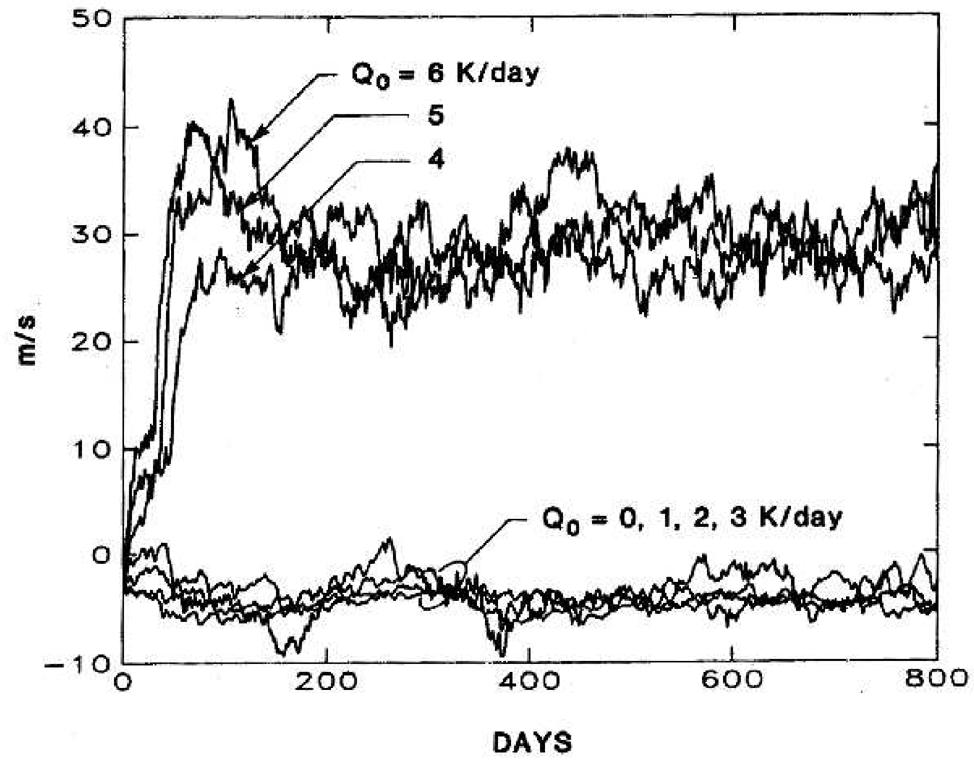

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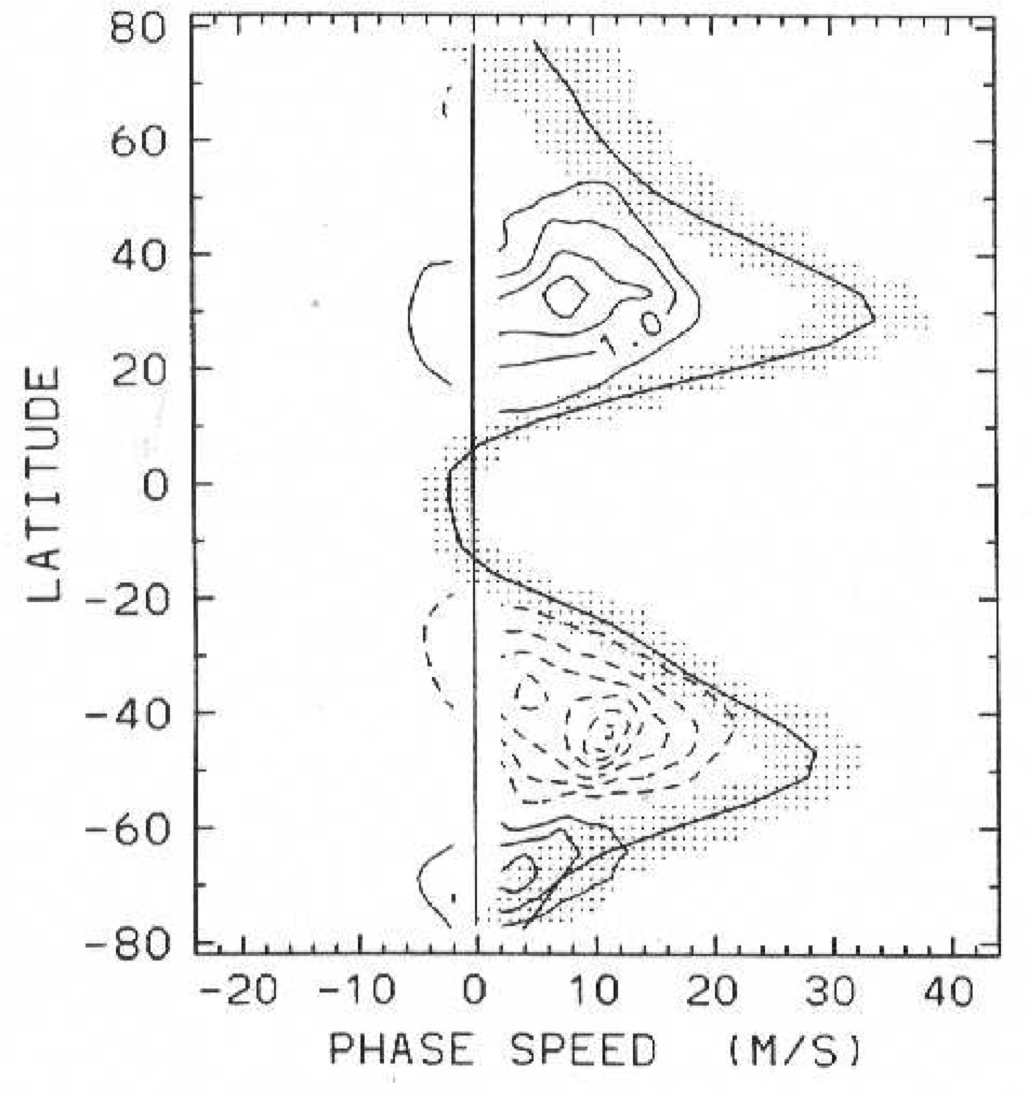

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