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CONVECTION WITHOUT EDDY VISCOSITY: AN ATTEMPT TO MODEL THE INTERIORS OF GIANT PLANETS

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Meteorologists are lucky. In the theory of hydrostatic quasi-geostrophic flow in the earth's atmosphere the principal results do not depend on the eddy viscosity. This contrasts with published theories of convection in deep rotating fluid spheres, where the wavelength of the fastest growing disturbance varies as $E^{1/3}$, where E the Ekman number is proportional to the eddy viscosity. A new theory [an extension of work by A. P. Ingersoll and D. Pollard (1982, Icarus *52, 62-80)] of quasi-columnar motions in stably stratified fluid spheres attempts to capture the luck of the meteorologists. The theory allows one to investigate the stability of barotropic and baroclinic zonal flows that extend into the planetary interior. It is hypothesized that the internal heat of Jupiter and Saturn comes out not radially but on sloping surfaces defined by the internal entropy distribution. To test the hypothesis one searches for basic states in which the wavelength of the fastest-growing disturbance remains finite as E tends to zero, and in which the heat flux vector is radially outward and poleward. A status report on this search will be presented.*

You've seen what happens to a nice guy like myself who tries to observe something and measure something, so for this talk I'm going to be a theoretician. I'd like to test a hypothesis. I don't necessarily believe the hypothesis, but I want to develop theoretical models that will provide us with some tests, especially if we have some data. The hypothesis is that the zonal flows and other large scale structures that we see are due to large scale motions in the interior driven by convection of internal heat (see Fig. 1). A number of collaborators have helped with this work. (D. Pollard, 1982; R. L. Miller, 1986; S. Schilpf, in progress.)

Eddy viscosity is sort of a distasteful subject, since you never know it in advance or even its sign or magnitude or the exponent of its magnitude and I don't like it. But it's a useful way to classify the two kinds of convection that I want to talk about. One is viscous convection, starting with Rayleigh and Benard plane parallel convection, but including configurations of rotating spheres. The general feature of this kind of convection is that the entropy gradient is decreasing as you go outward, so it's unstable stratification. There's nothing wrong with this kind of convection, except that if you're going to invoke it to explain the zonal flows on Jupiter, you get into trouble. Let me explain that. If you pick the eddy viscosity to be a small number, you find that the scale of the convection, which is the diameter of the little columns shown in Fig. 1, goes to zero as the eddy viscosity that you choose goes to zero. Eventually you get into scales that are just too small to account for the zonal jets. So if you're going to pursue the hypothesis that the zonal jets are related to the cylinders and those are related to the columns, then you've got trouble if the eddy viscosity is small. On the other hand, if you choose a large enough eddy viscosity to get large scales, you've got another problem if you're going to invoke this kind of convection on Jupiter, and that is that you've increased the dissipation of the mechanical energy. According to the hypothesis we've got shear flows of the same order that we see in the atmosphere extending way into the fluid, and with the large viscosity down there you're going to get more energy being dissipated than the planet has available.

I tend to reject viscous convection, or if not reject it at least look elsewhere for the kind of convection that I'm going to invoke for Jupiter. There is another place to look and that is the Earth's atmosphere which is stably stratified, with entropy increasing as you go up. On the Earth convection takes place on sloping surfaces of potential temperature and this is of course the theory of baroclinic instability which is quite successful. It's robust, the scale size does not depend on some undetermined eddy viscosity. It depends on fairly easy to measure quantities. Its radius of deformation involves the entropy gradient, and it kind of explains our weather at mid-latitudes. Also, you don't need a lot of fancy computation to get nice models. Of course this has been formulated for thin atmospheres with solid boundaries.

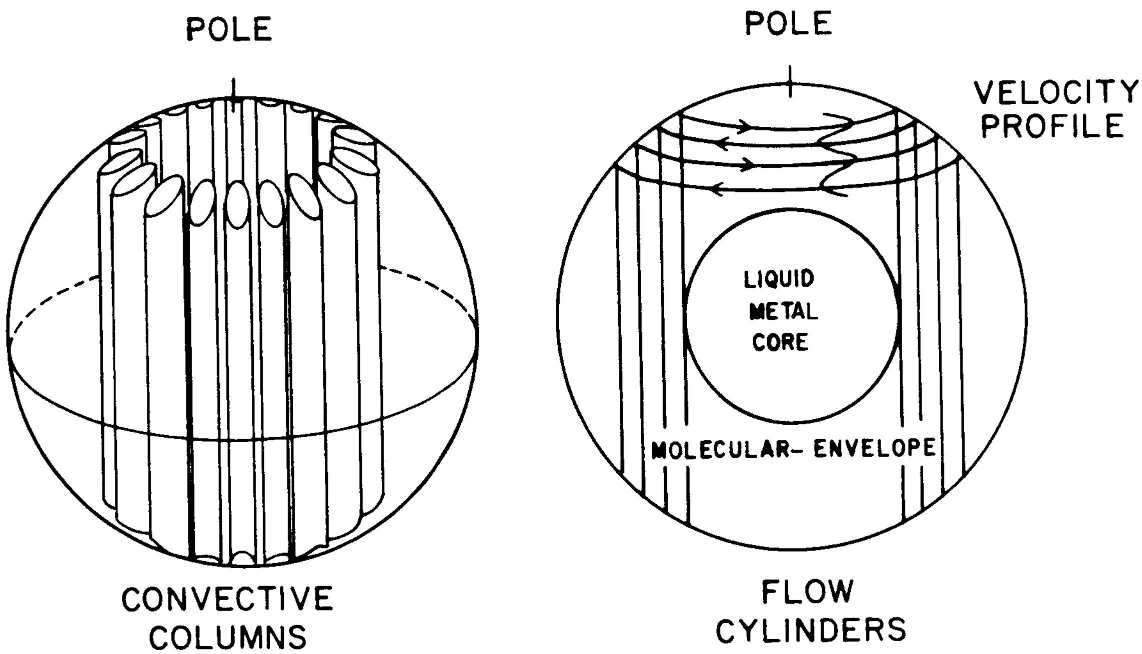
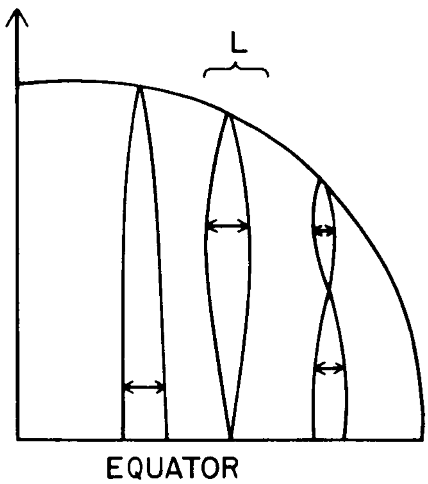


Figure 1. Schematic geometry for models of deep interior convection and zonal flow in the Jovian planets. The scale of the convection is indicated by the diameter of the columns in the illustration on the left. According to the hypothesis under study the columnar convection supports the observed zonal flow as cylinders of motion extending through the fluid interior as shown in the illustration on the right.

Well, the question is, can we somehow apply this idea of convection in stably stratified media to a full sphere of density, varying from some internal value down to zero at the surface, as in the interior of Jupiter and Saturn? So the goals of this research are ambitious. I'd like to redo all of text-book meteorology. I'd like to take Holton--page by page--and solve the normal mode oscillation problem, the problem of barotropic instabilities, the problems of baroclinic instabilities, all of these textbook problems. I want to throw in non-linear calculations, do it all again, having abandoned the thin layer approximation but kept the quasi-geostrophic approximation. It turns out that you can do that if you are dealing with quasi-columnar motions of the sort depicted in Fig. 2 showing the first three modes under study. I want to proceed with the hypothesis that the fluid is stably stratified, which means that entropy increases as you go out along its spherical radius, but on the other hand, is not spherically symmetric either so that there are horizontal entropy gradients; i.e., there are horizontal temperature gradients that can drive some sloping convection.

So that's what I'd like to do, and I'm only half way through it. Of course you don't know where the end is so I'm probably only 10% of the way through it. I have a few achievements. There is a nice quasi-geostrophic system of equations. It's a little different but it somewhat resembles the old system. It's got some new terms cropping up here and there. It's lots of fun, and I've been solving a few simple problems. The first one was the normal mode oscillations of the otherwise uniformly rotating planet. I get solutions that look like Rossby waves but they go to the east, not to the west, and that's because the effective beta parameter for the system has the opposite sign (cf. Ingersoll and Miller, 1986).



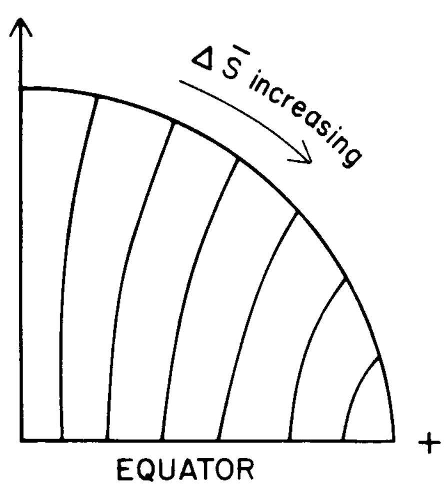


Figure 2. Schematic depiction of possible normal mode oscillations (left) and a possible configuration for the basic state distribution of entropy (right) for convection within the fluid interiors of rotating Jovian planets.

If one of the columns moves toward the axis of rotation, you stretch it and so the beta effect has the opposite sign as in the terrestrial problem. It is possible to match the wavelength of these features... *[Dr. Ingersoll holds up a copy of the conference notebook and refers to the schematic depiction of the equatorial plumes on the logo.]* ...according to these solutions and the 100 m s^{-1} wind speed if you assume the presence of a latitudinal duct at ± 9 deg latitude. Of course this is just forcing it to fit.

I've also solved the barotropic stability problem for a particularly simple case with no buoyancy forces at all and a totally adiabatic interior. You have differentially rotating cylinders and you want to know the criterion for shear instability. How much curvature can the differential rotation profile take before the instabilities will set it? It's very much like the barotropic problem in meteorology. Pollard and I, in our 1982 *Icarus* paper, investigated the deep barotropic instability in the long-wave limit for longitudinal disturbances. Now Miller and I have investigated this as a function of longitudinal wavenumber and have shown that the new criterion really does control the stability of the assumed flow. If you take the measured zonal velocity profile projected on the surface of the sphere and take its second derivative, the Voyager data show that every westward jet is unstable according to the simple barotropic stability criterion. Only the 23 degree jet is unstable according to the deep sphere criterion. And of course the sign of beta for the deep cylindrical case is opposite to that for the terrestrial case and the new criteria apply to the eastward jets, not to the westward jets. But it must be remembered that this has been derived for an adiabatic sphere.

I'm having problems with the baroclinic calculation, and this may be telling me something. I am objective enough that if it really tells me it's the wrong theory I will so state and so conclude. One problem is that there are too many possible basic state configurations for the entropy distribution. The real basic state should emerge from a fully non-linear calculation, but quasi-geostrophic theory can't give this. (There is the same problem in meteorology.) We've run a lot of cases with various entropy distributions and so far all the kinds of instabilities that emerge, all the growing oscillations, are small-scale even though the entropy gradient is increasing spherically outward. In other words the convection looks like what I call viscous convection. It wants to take place on small scales, and that really violates the scaling that I put into this whole theory. Maybe Busse is right. Maybe we just need a big eddy viscosity and there is no preference for large-scale motions. I'd like to be able to come to a conclusion at least and I'm not quite ready to do that nor have I put in buoyancy forces, i.e., baroclinicity, and at the same time shear. That lies ahead. So this is a progress report.

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DR. ROSSOW: Andy, could you just explain again the basic state for those of us who still think in terms of spheres? On a spherical surface do you end up with a gradient both radially and horizontally?

DR. INGERSOLL: Constant entropy lines are shown in my cartoon (Fig. 2). This is just one example of what you might try. The highest entropy is over on the right. This would be a planet heated by the sun, like Jupiter, but also radiating into space. If you go out along any spherical radius, entropy is increasing, so it's stably stratified. On the other hand, you do have horizontal entropy gradients so it's baroclinic. And the idea is the sloping convection would take place along these surfaces and would duct heat outwards and polewards just as it should to satisfy energy conservation. Ingersoll and Porco (1978), if I may cite myself.

DR. FLASAR: You mentioned that as the eddy viscosity goes down for the convective modes that the scales get shorter and that bothers you, but is that really so bad? Why can't the heat transporting convective modes drive larger scales? I mean, drive larger eddies which in turn drive the jets. Why does that bother you necessarily?

DR. INGERSOLL: It's only something that bothers me, it's not a proof of something. Numerical models of viscous convection are always done at moderate Reynolds numbers and moderate Rayleigh numbers, because you can't just shoot those numbers way up and have a convergent numerical scheme; and at those values, the scale of the eddies is about the same scale as the mean flow that they drive. No one really knows what those models are going to do at much higher scales and it may be that little tiny eddies will drive great big cells.

DR. LEOVY: I very much appreciate your desire to redo Holton and I got the same feeling just from reading your paper with Pollard, but there are two aspects of the barotropic situation that I'd like to ask about. Two aspects of the kind of barotropic analogy that you and Pollard describe intrigue me. One is that, for that barotropic vorticity equation one gets 2-D turbulence with upscale energy and downscale enstrophy cascades, and the second is this: there may be the possibility of Rossby wave solitons in this type of system. Have you investigated whether either of those are possible in the barotropic case?

DR. INGERSOLL: There's only one obviously conserved quantity, which is energy, in this system. I don't see any enstrophy sitting around...

DR. LEOVY: Do you have a vorticity?

DR. INGERSOLL: I'm just telling you what I know so far. There's a vorticity, but the vertical equation is totally different from anything you're used to. You can't combine it into a single potential vorticity equation. You just can't do that in this system the way you can in the other systems and so there's only one conserved thing and it's energy. You could say there are low frequency inertial oscillations too, so in a sense it's got more degrees of freedom. It has more than just a Rossby-type wave mode. The quasi-geostrophic system filters out inertia-gravity waves. This system doesn't filter them out.

DR. EMANUEL: In your reworking of Holton, did you get around to generalizing the Charney-Stern theorem? In other words, do we really know for a deep rotating fluid like this what the criterion for internal instability really is? Is there a generalization? Can we state with any confidence that an instability criterion is really satisfied because I think, as you pointed out earlier, just looking at the horizontal variation of U along spherical surfaces is insufficient, it's not really a critical look at the stability criterion.

DR. INGERSOLL: I haven't found any lovely theorems. What I call the Rayleigh criterion is certainly an integral constraint. I could only derive that assuming something about the perturbations, namely they have long wavelength. Otherwise I just had to root it out numerically. So I don't know the answer.

DR. EMANUEL: And that was a strictly two-dimensional analysis?

DR. INGERSOLL: If you assume long wavelengths you can turn it into two dimensions, one of which has perturbations proportional to e^{ikx}. The numerical three dimensional analysis is non-separable in the spatial coordinates. It's kind of a nasty thing.

DR. HATHAWAY: A comment about eddy viscosity. You suggest as you make that viscosity smaller and smaller you get a smaller scale of convection that is preferred, when what really happens is that you just open up the spectrum to those smaller scales. It may still be the large scale convection that dominates. If you in fact look at growth rates for convection in a purely inviscid fluid, the spectrum rises from zero and then flattens out completely off to infinity. The maximum is out at infinity but you get this broad spectrum that is included.

DR. INGERSOLL: Well I was citing the linear stability analyses of Busse and company when I said that. It's true that in a fully nonlinear calculation, the fastest growing disturbance is not necessarily the one that's preferred.

DR. HATHAWAY: Then a question. Can you get Jupiter's internal heat flux out for a model that has an entropy maximum at the surface and the equator?

DR. INGERSOLL: That is the kind of self-consistency test that must be applied. I will believe the model if I can and I won't believe it if I can't. But it's hard to prove you can't do something because you may not have tried it the right way. You can try it a million times and the million plus first time it works. That's kind of the problem I'm having right now.

DR. TAYLOR: At the beginning you explained a desire to have more data. Could you say what kind of data you'd like to have and in detail how you would use it to test different aspects of this?

DR. INGERSOLL: I'm going to spend tonight writing my conference review and I'll tell you tomorrow about what you can expect from Galileo, Voyager, and what I'd like if I had some other spacecraft...You think I'm just going to review what you people said.

[Laughter from conference participants.]