

Interactive comment on “Baroclinic and barotropic instabilities in planetary atmospheres - energetics, equilibration and adjustment” by Peter Read et al.

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Received and published: 14 January 2020

This is an interesting and generally well-written review of barotropic and baroclinic instabilities, and their equilibration, in planetary atmospheres. I have mostly fairly minor comments and clarifications for the authors to consider.

Thanks for this. We respond to these points below and will modify the text of the paper as indicated.

1. I think it was Margules who introduced the idea of a potential energy that is available for conversion to kinetic energy. Granted it was Lorenz who (as the authors describe) came up with the eponymous energy cycle, but in a review article such as this it might be appropriate to reference Margules.

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Noted. We will add a reference to Margules as suggested.

2. Regarding baroclinic equilibration and baroclinic adjustment. It is fairly apparent that Earth's ocean is in a supercritical state, not one of marginal criticality. The ocean may be beyond the scope of this article, but that observation prompted a somewhat more general discussion of why supercriticality does occur that may be found in Jansen and Ferrari in 2012/2013. Going back a little further, precursors may be found in Salmon (GAFD 1980) and Vallis (QJ 1988) who both found supercritical regimes. The authors may wish to consider a brief discussion of all this.

This is a good point. Although the ocean is probably beyond the intended scope of the article, similar super-critical regimes may occur in gas giant atmospheres. We will add some extra discussion of this in Section 5, including mention of some of the arguments of Jansen and Ferrari. A notable addition to this from the present work is to emphasise the non-negligible role of the zonally-symmetric meridional overturning in addition to eddy transports in maintaining a criticality close to 1 under some conditions. We will try to clarify this in our discussion.

3. Regarding Venus, it seems to me rather an 'ill-posed planet' as regards quasi-geostrophy, or at least its troposphere is. Both N and f are close to zero (compared to values on Earth) so the deformation radius, NH/f (or equivalently the square root of the Burger number) is poorly defined. At cloud level N is finite but f is still small, so the conventional deformation radius is very large, probably larger than the planet itself. The discussion toward the end of section 4.2 needs a little more clarification, since as defined by equation (13) the Burger number does not vary with the reference frame, unless f is being considered as changing, which is perhaps what the authors mean. But in any case this needs clarifying and a bit more discussion.

Noted. We will clarify in our discussion in Section 4.2 that the modified Burger number in the cloud layers assumes a change in f to a value representative of the mean angular velocity of the cloud layer itself - with a rotation period of around 4 Earth days. This is

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what has been assumed in other published work, such as that by Young et al. (1984) and (at least partly) by Sugimoto et al. (2014).

4. *It is not just realistically forced models that struggle to get the key features of Venus. Different models with Held-Suarez forcing can give very different results, as I think some recent model intercomparisons have shown (although I don't have a reference to hand). I suspect this is at least in part because of the ability of models to conserve angular momentum, and the fact that models are sensitive to the ratio of N/f , which might vary among models.*

Agreed. We will include mention of idealised models and reference the recent inter-comparison of such models by Lebonnois and others.

5. *The authors say, early in in the conclusions, that 'the distinction between baroclinic and barotropic instabilities is to a large extent an artificial one.' Is that really the case? Granted the CSP criterion covers both cases, but as the authors themselves say the difference is associated with whether the basic state has a store of potential energy that is converted to eddy kinetic energy, and this doesn't seem like a minor difference. And the Lorenz cycles for barotropic and baroclinic instabilities are quite different. If the authors are trying to make a profound statement that, in spite of these differences, the instabilities are really of the same type then I think they need to justify this more. If not, I'd suggest they moderate that statement.*

Point taken. The similarities are from a vorticity point of view only, but the distinction remains energetically as noted here. We will clarify this.

6. *At the beginning of the section on Jupiter the wording suggests that there are a number of gas giants in the Solar System, whereas in fact there are only two, if ice giants are regarded as separate. This is made clearer later on, but a slight rephrasing to clarify might help.*

OK though the dynamical distinction between gas and ice giants is perhaps rather a

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fine and subtle one. Section 4.3 does make a distinction between gas and ice giants in a few places, but we will emphasise the distinction in our discussion of Jupiter a little more than we do at present.

7. *Is it really the case that (line 581) 'the release of latent heat energy from condensation of water vapour plays an important role ...in the weather layer'? Do we know that or is that really just a conjecture? After all, the moist and dry adiabatic lapse rates are almost the same on Jupiter because of the hydrogen atmosphere, suggesting that moisture may have a limited effect. Granted there are arguments the other way (lightning is seen, there is a strong virtual temperature effect, some numerical simulations), but the importance otherwise of moisture seems to me an open question. If I am mistaken and it is a settled issue then the authors need to point to some definitive evidence and give references.*

Precisely what the role of latent heat release in moist convection is in the weather layers of Jupiter and Saturn is still an open one. But evidence for the occurrence of moist convection I think is fairly unequivocal (notably through observations of lightning and convective water clouds). Also, estimates of upward energy flux by moist convection (e.g. by Gierasch et al. (2000)) suggest that an $O(1)$ fraction of the interior heat on Jupiter (>50%) may be carried by moist convective processes. This led Ingersoll et al. (2000) to suggest that the dominant eddies driving the zonal jets might also be from moist convection, although the recent work of Young and Read (2017) suggests that baroclinic instabilities (on scales similar to the Rossby deformation radius) may be most important for this. We will add some discussion to clarify this.

Interactive comment on Nonlin. Processes Geophys. Discuss., <https://doi.org/10.5194/npg-2019-53>, 2019.

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