

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

Anonymous Referee #2

Received and published: 1 January 2020

Review of ‘Baroclinic and Barotropic Instabilities...’ by Read et al.

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.

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.
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

C1

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.

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.
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.
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

C2

not, I'd suggest they moderate that statement.

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.

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.

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