

Editorial comment for paper

acp-2020-89

Self-excited Oscillations in the Atmosphere (0 – 110 km) at Long Periods

by D. Offermann et al.

I thank the authors for their replies and (small) changes to the manuscript. I am sorry to say that I am still confused about two essential aspects of this study, which I mentioned in the last round of revisions and which are still not properly addressed. I urge the authors to spend more time to improve these issues. In its current version, I cannot accept this paper for publication.

Major comments:

1) **Origin of oscillations.** In your last reply document, you write “This is a misunderstanding: We did not claim an atmospheric origin of the oscillations, but we said that the oscillations are atmospheric properties. We do not know yet the origin of the oscillations, as was stated several times in the paper.” To me this is completely inconsistent with the paper. The title of the paper is “Self-excited oscillations in the atmosphere”, which I cannot read differently than these are oscillations that are generated / excited by atmospheric processes! So how can you now say that you don’t claim an atmospheric origin, I am very confused. If your reply reflects what you would like the reader to get from your study, then you must change the title, the abstract and many parts of the paper. Please also note that the first sentence of your conclusions reads “The structures analyzed in this paper are believed to be oscillations that are self-generated in the atmosphere.” This is radically different from your last reply. This essential aspect must be made consistent from the title to the last line of the paper.

2) **The potential role of the land surface model.** There is still not enough specific information in the paper (nor in the reply document) for the reader to understand the setup of the land surface model used in your simulations. This point is important because in case of an interactive coupling of the atmosphere with the land surface model, this might be a potential cause for the oscillations. You write that “The land component of the models has been kept constant” but this is not clear enough. Most likely your land model has a seasonal cycle of vegetation and of soil moisture? Soil moisture is an important parameter here; is this variable interactively coupled to the atmosphere (as in normal GCMs) and therefore can there be interannual variations in soil moisture and land-atmosphere interactions? Did you maybe even use a dynamic vegetation model? These aspects are very important to clarify. I fully acknowledge that your expertise is not with the land model, but for this particular study more detailed information about this aspect of the model setup is essential.

A minor remark: In the short summary you write “However, a GCM can be changed arbitrarily!” Hopefully not. I think I understand what you like to say, but “arbitrary changes” sounds like unphysical model modifications.