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Geoscientific Model Development Discussions

Interactive comment on "The upper-atmosphere extension of the ICON general circulation model" *by* Sebastian Borchert et al.

Anonymous Referee #1

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General comments

The manuscript describes modifications of the dynamical core and physical parameterizations in the ICON general circulation model, which allows to extend the model domain to the lower thermosphere up to 150 km. In the first part the authors describe the changes in the dynamical core and the results of two idealized tests with great details. The climatological tests aimed at the evaluation of the overall model performance and the influence of two major modifications are described in the second part with much less details. The manuscript is in the scope of GMD and will be useful for climate community, because the vertical coupling of the atmosphere from the ground to the middle thermosphere is widely discussed in the recent publications. The number of appropriate models for this kind of studies is very limited and the appearance

of new model is very welcome. However, the structure and some disproportion in the manuscript make it rather difficult to read. The first part is interesting mainly to the developers of dynamical cores because the used experimental set up is very idealized and cannot be easily applied to the modeling of the real atmospheric processes. The second part is more interesting for the climate community, but in the present for it is too sketchy and does not convince readers that model is ready for operational use. The authors are constantly mentioning substantial biases in the zonal wind and temperature distributions and even state that they know how to improve the model in the future. It suggests that the model is not mature enough to be recommended as a tool for community. Of course, this aspect will make the manuscript less important and readable. If it is ok with authors I will not strongly insist on any major changes. However, I would advise to keep in the manuscript only the first part and the analysis of the climatological runs ICON, ICON-UA and ICON(UA), which makes possible to show the influence of introduced changes in dynamical core and physical parameterizations. Maybe it is even better to concentrate on dynamical core (with proper changes of the title). The model evaluation should be better postponed because more careful model tuning and large set of considered variables are necessary. This should be done before the publication of the manuscript, otherwise this important contribution will not be fully appreciated.

Major issues

1. The manuscript is too long and rather difficult to read. I can propose some changes (see in general comments), but I do not strongly insist on their implementation.

2. Introduction: Some review of the existing models is a must. The new model development should be considered in the context of the existing tools.

3. The atmospheric state is not just temperature and zonal wind. There are much more parameters to evaluate for the complete understanding how the model represents atmospheric processes. The most important processes were defined during several

model intercomparison campaigns. In the case when the upper atmosphere is involved this list can be extended by additional parameters (e.g., tidal waves).

Minor issues:

1. page 1, line 10: what is satisfactory and good agreement? I would not characterize major bias in the stratospheric zonal wind as a good agreement.

2. page 4, line 6: electronically? Electrically sounds better.

3. page 5, line 1: H is not constant. Is it considered in the described example?

4. page 5, first paragraph: I do not completely understand the message.

5. page 13, first paragraph: Why not to use HAMMONIA output for the initialization?

6. page 15, table 1: Any reason not to use HAMMONIA approach (Solomon and Qian, 2005).

7. page 16, line 1: I am not sure I understand the situation with GWD. Is it off in the standard ICON, but on in ICON-UA? Then the comparison between them is difficult because there are differences between models such as GWD and presence of sponge layers.

8. page 16, line 6: It is not correct. Lyman-alpha and SRB can contribute down to 60 or even 50 km.

9. page 14, line 16: why 0.23?

10. page 29, line 21-22: Unidentified bug? What is the reason to assume that this limitation is not important?

11. page 30, line 1: Would it be possible to discuss the role of damping when you compare different model versions. It can be rather dramatic.

12. page 30, line 27: Which minimum is discussed? It is not visible.

13. page 31, line 1: It would be interesting evaluate the influence of dynamical and physical processes separately.

14. page 34, line 24: Clarify good agreement. In some cases, the agreement is not so good.

15. page 35, lines 1-2: It would be interesting comment on the influence of dynamical and physical processes separately.

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