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Geoscientific Model Development Discussions

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Interactive comment on "Extending the Modular Earth Submodel System (MESSy v2.55) model hierarchy: The ECHAM/MESSy idealized (EMIL) model set-up" *by* Hella Garny et al.

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The authors document a new idealized model configuration within the ECHAM/MESSy modeling framework, and demonstrate how it can be used to investigate open questions in the climate sciences, namely chemistry-transport interactions and the monsoonal circulation. I believe that this work is timely and important, and would be of interest to GMD readers. I therefore recommend publication pending consideration of the comments/suggestions below. As my identify might be obvious given my familiarity with the system, I'm signing this review. Ed Gerber

General comments

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1) The authors compare the performance of EMIL against a number of benchmark cases that are in the literature. It would be ideal, however, if we could move beyond the "picture norm" for these comparisons – at least in the future. Could you publish the data for these results (or incorporate it within the ECHAM-MESSy distribution), so that in the future, other groups could check their models against yours? The best standard would be to determine whether your integrations are consistent/inconsistent with other benchmark integrations, within the sampling uncertainty. I believe that data can be archived through Zenodo.org, or other structures. You could just provide the zonal mean time mean data needed for the figures.

Another option would be to include the key benchmarks as test cases within ECHAM/MESSy, something that could easily be reproduce by another group. Could you provide a citable link to the model and the required parameter scripts? (That is, a frozen version of the model, as was used to produce this paper, ideally with the same run scripts that you used.) I appreciate that the supplement provides all the parameters, but it would still involve a lot of work (and hence many chances to make a mistake) to reproduce this exactly.

2) I appreciate that the authors have striven to find a balance between detailing a new model set-up for others to use, and presenting new results. I felt that the test cases that were shown at the end in section 5 were very interesting, but could have been more developed. To provide more space, perhaps the earlier sections could be condensed? (The reader might also be a bit exhausted by the time they reach these really interesting results!)

For example, there are a lot of equations and parameters defined in this study, many which are specified in other papers (but also many of which are new). I think some of this detail could best be put in an appendix (e.g., in sections 2.1.1, 2.1.2 and 2.1.3), allowing you to move more quickly to the results.

3) It would help the reader to have a table that defines all the parameters in one place.

It would also help you catch any parameters that are multiply defined. One example is k_max, which appears in the equations (8) and (9) with distinct values. k_damp is also defined inconsistently between these two equations (though any reasonable reader would understand what is meant). The parameter \delta\phi also appears in multiple equations, e.g., (5) and (16).

Finally, I noticed that \sigma is sometimes used to refer to a vertical coordinate (p/ps), and at other times used width (where I appreciate the motivation is to connect it to the variance of a Gaussian). It might be good to adopt a consistent notation, where \delta is always used for width parameters – but again watching out to make sure all parameters are uniquely defined. (This said, I know that these parameters came from multiple papers in the literature, where the other authors were not consistent with each other!)

4) The paragraph spanning from page 3 line 28 to page 4 line 2 is very interesting, but seems out of place in the introduction. I would consider pushing this to final section, where you could present it as the next step in your research program.

5) Finally, the topic of regimes comes up quite prominently in section 4. I think this is a very interesting (albeit sometimes frustrating) result that could be mentioned in the abstract and introduction. I think these regimes have simmering in idealized models for sometime: as detailed by Gerber and Polvani (2009), the original PK02 result is so dramatic precisely because of a regime switch between their \gamma 2 and 4 integrations. Chan and Plumb (2009, DOI: 10.1175/2009JAS2937.1) and Wang et al. (2012) discuss this in more detail.

The presence of regimes is interesting: if such a thing existed in our atmosphere, we could be in for surprises with global warming (or perhaps when the planet enters an ice age). If it is an artifact of these idealized models, however, it's something that the dynamics community should be wary of. It could lead to unphysical parameter sensitivity or results that are qualitatively disconnected from the real atmosphere, breaking the link we'd hope to establish through model hierarchies.

Specific comments (largely typographical) by page:line number

1:1 Consider "As models of the Earth system grow in complexity, a need emerges to connect them with simplified systems through model hierarchies in order to improve process understanding."

1:3 consider cutting "with the aim"

1:6 Would you consider ECHAM/MESSy a "model", or rather a "framework" which allows you to build many different models.

1:10 Consider "Test similations with EMIL reproduce benchmarks provided by earlier dry dynamical core studies."

1:19 What do you mean by "the ability to simulate dynamical systems"? Dynamical systems in the broadest sense is a whole field in mathematics. Perhaps you mean "the ability to simulate qualitatively realistic dynamical variability of the circulation"

1:22 Consider something like "Earth system models continue to incorporate more processes to enable a more complete simulation of the climate system, and thus produce the best possible climate projections. In practice, this increases the complexity of model codes as new compartments are added to represent new processes."

I'm not sure if you need that second sentence; my thought was that the goal is to increase the range of processes that are simulated, and this is effected in practice by adding more compartments, modules, etc..

2:9 stray space: "hereafter) ."

2:13 I think the upper level drag is only in the PK02 set up, and not a part of the original HS94 configuation.

2:16 consider a paragraph break before "The functions..."

2:21 "to idealized heating that mimics the thermal response to CO2 increase" I think "climate change" is the response, not the forcing!

2:26 "motivates one to include"

2:29 Jucker and Gerber (2017) were not the first/only one to do this. Consider also referencing:

Merlis, T. M., T. Schneider, S. Bordoni, and I. Eisenman, 2013: Hadley circulation response to orbital precession. Part I: Aquaplanets. J. Climate, 26, 740–753, doi:10.1175/ JCLI-D-11-00716.1.

Tan, Z., T. A. Shaw, and O. Lachmy, 2019: The sensitivity of the jet stream response to climate change to radiative assumptions, J. Advan. Mod. Earth Sys., 10.1029/2018MS001492.

2:35 Here and throughout the text, the quotes seem to be reversed. Perhaps this is set by the journal, but I am used to “hello” as opposed to ”hello“

3:19 consider "allows the creation of model hierarchies"

3:20 consider "Earth-system model. Any developments..."

5:9 I found "idealzied localize contrained" to be awkward. Consider just "forced by a simple, localized heating that..."

eqn (1) In HS94 and other papers, it's usually just T_{eq}

5:30 This was a point where I feel you've lost the balance on providing enough technical advice without making the paper too long. Do you need to describe an option that "physicaly of little use"

eqns (204) To make the paper more concise, you could refer the reader to HS94. I appreciate that equation (2) is modified by the inclusion of the \epsilon sin(\phi) term; this was documented by equations A3 and A4 in PK02. A happy medium might be to reference past work in the paper, highlighting your modifications, and including equations in an appendix.

5:19 T_{US} isn't defined in the paper. The reference is: U.S. Standard Atmosphere, U.S. Government Printing Office, Washington, D.C., 1976. (Which I appreciate isn't so easy to find!)

eqn (6) Aditi Sheshadri did something like this in her 2015 paper, https://doi.org/10.1175/JAS-D-14-0191.1. There she lowered the start of the vortex to 200 hPa. That said, I appreciate the more thorough investigation of the transition height in this study!

Figure 3 and surrounding discussion. It is interesting that the jets shift equatorward when you move from the T63L19 to the T42L90 integrations. I suspect the vertical resolution plays a more important role her than you might suspect. This is consistent with the behavior of GFDL's spectral core, where the jets also shift equatorward when the vertical resolution is increased. See Fig. 4 of Gerber et al. (2008), https://doi.org/10.1175/2007MWR2211.1. This doesn't seem to happen in finite difference or finite volume based cores. [This said, I don't mean for you to add another citation; I think you've already been very generous in referencing my past work.]

11:13 consider a paragraph break after PK02.

11:14 (namely GFDL's spectral dynamica core)

Figure 5: the caption on this figure could be expanded to help a reader who's skimming the paper, for instance, defining the key parameters p_{Tw} and \gamma that are being used. I'll admit I had to remind myself what p_{Tw} represented.

13:16 Along the lines of my general comment on the "picture norm", it would be ideal to be more precise about what you meant by negligible. I think you mean that it is small relative to uncertainties in the cliamatology with resolution (i.e., T63L19 vs. T42L90), but you could also define it relative to sampling uncertainty (i.e., it would take inordi-

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nately long integrations for the difference to be significant above sampling noise.)

Figure 6 and discussion. I appreciated this portion of the paper, but a a quick question: is one month of austral hemisphere gravity wave drag enough to nail down the effective damping rate in models? I don't have a good sense how much this rate varies. I assume this includes both orographic and non-orographic drag? Would the effective rate be much different in the boreal hemisphere during winter?

I think it would help to expand the caption, to explain that GWD/u provides an effective damping time scale of the winds when using a full gravity wave drag scheme.

15:4 "cannot"

Figure 8 and following figures. You could possible color the dashed curves which show the equilibrium profiles, to make the comparison with their respective \gamma's easier. For Figure 8 specifically, please specify the location of this profile. Is it right at the pole?

16:7 consider a paragraph break after \gamma.

17:3 In Wang et al. (2012), I think we had to grapple with this same regime behavior. The model switch abruptly from a state with active stratospheric variability and a strong residual mean circulation (which allows the temperature to deviate substantially from T_eq) to a state with an very cold, stable vortex near "radiative" equilibrium. In Wang et al., this regime change was associated with a substantial change in the position of the tropospheric jet. Does that happen here?

17:28 "these two simulations"

Figure 10 Here you are showing results from integrations which exhibit multiple regimes. Based on past experience (e.g., Wang et al. 2012), regime transitions can introduce very long time scales, as the model switches between states. You can see this of this Figure 5 of your text, which corresponds to pTw=400, gamma 2 integration shown in the right panel (I think.) Therefore, you have to be very careful in establishing convergence. Earlier in the text you suggested that runs were done for 1825 days; it seems that you have longer runs (3000 days are shown in Fig. 5), but I'm not sure that would be sufficient. It would be good to check/comment on the sampling uncertainty in these climatologies.

Figure 12 and discussion. I suspect that the strength of the overturning (difference between T and T_eq) near the model top will be dominated by the drag layer. Hence, it's likely to be determined by \gamma: if you force a stronger vortex, you need a stronger drag. At lower layers, the strength of overturning is dominated by "wave pumping", and so the resolved circulation.

I worried about this a lot in preparing my 2012 paper, but convinced myself that in the mid-to-upper stratosphere, the differences in the residual circulation in response to changing gamma were still being dominated by the waves, and so not an artifact of the sponge layer. I'm not exactly sure how far down you need to go to be free of the sponge layer, but perhaps 10 hPa would be a better choice than 1 hPa? This would be supported by Figure 7, where you find that the spong layer has a negligible impact below 10 hPa. I'd also be curious to see if the nonlinearity in the vortex shown in Figure 11, bottom left, shows up in the overturning at 10 hPa in the model with heating.

22:18 consider a paragraph break after "high."

22:19 consider "high latitudes (north of 60N), driven by the strong wave dissipation that effected the SSW; see the red line in the top panel of Fig. 13. This transports ..."

22:22 consider "latidues, evident in Fig. 13 ... 15 ms-." (no paratheses). I'd also consider breaking the paragraph after this sentence.

22:29 Isolated from what? Consider cutting "in an isolated manner," or to be more specific, e.g., "independent of the annual cycle" or "isolated from all other chemical processes".

Figure 13 Consider reworking the caption, as you first refer to the middle panel.

It might also be nice to include a second axis on the top panel, or to make "w*[10ˆ-5

hPa]" in red

23:1 Consider a paragraph break after "is steep."

23:3 "downwelling is maxized at the"

23:4 same as above

24:10 consider a pargarph break after "state."

24:17 10ˆ20 J sounds like a lot, but could you provide some context? Say, what is the effective heating rate per square meter (W/mˆ2), which could be more easily compared to solar or precipitation forcing. With hope this number is in the ball park for what you'd expect from monsoon precipitation.

24:19 "produced in response to the additional heating"

24:23 consider a paragraph break after "respectively.)"

24:26 Perhaps the anticyclonic centers could be marked/labeled in the figure.

24:30 You could break the paragraph after "2016).

24:30 Consider. "An example of eastward eddy shedding was observed during the second period, as displayed on the right of Fig. 16. This phenomenon has been previously investigated..."

25:7 Your summary opens with a hard sentence to parse. Consider from line 8"... model system is documented. The set-up, denoted EMIL (explain the acronym), is shown to perform consistently with established dry dynamical core benchmarks, both earlier configurations of the ECHAM core, and those developed by other modeling centers."

25:26 "used setups. The polar"

26:1 This is an interesting result, as we see this coupling in observations (i.e., with the ozone hole, or following an SSW). It is my understanding that the tropospheric state of the Lingren et al. (2018) model is substantially different, and might explain why does not couple to the stratosphere. As you have shown in Figure 10 (right panel), for instance, easterlies are generated in the UTLS region of the winter hemisphere.

26:3 consider "we present, as a proof-of-concept, a"

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