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Interactive Comment

Interactive comment on "Total energy norm in NWP closure parameter optimization" *by* P. Ollinaho et al.

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- *The title does not make much sense to most readers. Something about EPPES, or related, should be there.*
  Title changed to "Optimization of NWP model closure parameters using total energy norm of forecast error as a target". **Title changed.**

- *Should have "dry" in front of total energy norm throughout the paper.*
  Total energy norm is now referred to as dry total energy norm. **Text amended.**

- *Discuss or even speculate how much the moisture part can influence the results and conclusions.*
  The moisture part of the total EN (calculated as in e.g. Barkmeijer, 2001) seems

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to be in the same order of magnitude as the temperature term when using w=1. When using the uniform weight the tropical lower troposhere is dominating the term.

Even using the dry total energy as the target for optimization improves the humidity profile when compared to the default model. We speculate that including the moisture term would have a slight effect on the final parameter distributions, with a (small) added influence on the model performance with respect to the humidity fields. But without constructing a weighting function for the moisture part we cannot accurately predict what the magnitude of the impact would actually be. **Text added, Chapter 5, p. 13.**

- *Also when kinetic energy is used, please explain why not use the dry total energy.* The division of the dry total energy norm into kinetic energy, and temperature and surface pressure terms is done to better understand the model response to the change of the closure parameters. We want to study the total norm itself, but also learn about the individual contributions. This has been emphasized now. **Text amended, Chapter 3.2, p. 7.**

- *As this is based on the previous work using other norms, it would be nice to show some comparison results, which can demonstrate the superiority of the energy norm.*
  We have added a scorecard comparison of tropical RMSEs of the energy norm and geopotential height target criterion experiments. **Text changed, Chapter 5, p. 13. Added Fig. 9.**

- *I guess the energy norm can also computed over a limited area and a selected vertical range. I know many people try to find a universal number for a model parameter over the whole globe, but I guess we may have to use different numbers for different areas. Some discussion may be useful, especially in connection with the regional degradations.*

Yes, the energy norm can be also computed non-globally. Also, the EPPES estimation can be applied to target improvements in limited area(s). Thus, if the aim is to find a model that performs at the best possible forecast skill in a certain area(s), EPPES could be used to optimize a closure parameter set with this in mind. However, we feel that extending the discussion into the technicalities of modeling the physical processes themselves is beyond the scope of this paper. **Text added to emphasize the limited area optimization possibilities, Chapter 5, p. 13.**

- *All readers need to read previous EPPES papers in order to read this paper. Is EPPES really well-known?*
  We have extended the description of the EPPES algorithm. **Text added, Chapter 2, p. 4-5.**

- *Eq (2). Should there be a delta_p or delta_sigma in the vertical summation to give proper weights to different model layers? At least some comments should be offered on why they can use the same weight for different layers for the total energy computation.*
  Yes, there should be. The term was omitted since we use dp=1 throughout the atmosphere. Since this was an initial experiment we wanted to also have a contribution to the cost function from the surface pressure term. Including proper dp weights would make the kinetic energy and temperature terms about 30 times larger. Also, the weights between the levels are quite uniform, thus we feel that having dp=1 does not produce substantial errors in the atmospheric weighting. Although we do realize that the upper atmosphere is a bit overweighted in our treatment. The small surface pressure term could also just be omitted (like in e.g. Orrell, 2001). **Equation 2 amended and text added, Chapter 3, p. 6.**

- *"The impact of initial state and parameter perturbations separately... (not shown)." Why not? It is quite interesting.*

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We have added a figure showing the individual spread contributions. **Text added, Chapter 3.2, p. 7. Added Fig. 2.**

- *Fig.1. What is the unit for energy norm?*
  $[v]^2 * [A] * [p] = ... = J/kg * m^2 * Pa$. **Unit added, Fig. 1.**

- *Fig.2. Where is the shading scale? May need to use colors. Units?*
  The importance weights are not the primary focus of the figure, but rather included as a curiosity factor. Thus, adding a shading legend would, in our mind, take the focus to the wrong place. The units are in fractions, i.e. black dot 51/51 (the parameter value dictated solely the distribution update), white dot 0/51 (the parameter value did not effect the distribution update). **No changes.**

- *Fig.3. Units?*
  As in Fig. 1. **Unit added, Fig. 3.**

- *Fig.4. Units? Are these large or small differences?*
  Units for RMSE is m, for ACC fraction (0,01 equals to 1%). The ECHAM5 average RMSE score for three day forecast of z500 over the sampling period is about 27 m. The change is thus about 2%. **RMSE unit added to caption of Fig. 4.**

- *Fig.5. Too small.*
  Agreed. **Figure enlarged.**

- *Fig.6. Why not dry total energy?*
  We felt it would be of more interest to show a quantity which is easier to relate to real phenomena (accuracy improvements in wind speeds). **No changes.**

- *Fig.7 Too small.*
  Agreed. **Figure enlarged.**

References

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Orrell, D., Smith, L., Barkmeijer, J., and Palmer, T. N.: Model error in weather forecasting, Nonlin. Processes Geophys., 8, 357-371, doi:10.5194/npg-8-357-2001, 2001.