

Letters to referees #1 and #2.

Letter to Referee #1

The technical corrections suggested by Referee #1 are answered in the following:

- *I would still recommend to add this figure because it illustrates a key conclusion of the article: using the total energy norm as a target is superior to other choices because it accounts for forecast deviations in all levels.*

If the figure is included the physical units should be given in the legend and a linear scaling instead of the logarithmic scale in the vertical may be considered in order to reflect the relative contributions to the integral of the norm. (see also the remark below).

Figure and text added. **Figure 9 added. Text added, Chapter 4, p. 11 lines 20-23 and line 25.**

- *"dp" is the differential used in the vertical integration of the energy norm. If the integral is approximated on a spatial grid (as done here) the integral should be the sum of the integrand at each grid-point times "dp", with "dp" being the thickness of the model layer (as stated in the first part of the above sentence). However if dp=1 each model layer would enter with the same weight, regardless of its thickness. This is contradictory and should be clarified. If the latter is the case (dp=constant) it would be essential to give information of the model layer thickness in the paper because that would determine the weight of the target norm at different levels.*

The choice of dp=1 indicates here that all model layers have the same weight. dp values in ECHAM5 with 31 vertical model levels vary between 10 - 50 hPa. Thus treating dp as constant with value of 1 emphasizes the surface pressure term since the dp-summation term becomes smaller than with correct dp values. This treatment also gives slightly less (relative) weight to model levels at 800 - 500 hPa where the ECHAM5 model levels are furthest apart. **Text added and clarified, Chapter 3.1, p. 6 lines 10-13.**

- *Also "w" in equation (3) should have a physical unit (J should be dimensionless).*

Unit added. **Text added, Chapter 3.1, p. 7 line 2.**

Letter to Referee #2

The questions posed by Referee #2 are answered and relevant corrections presented in the following:

- *The authors should discuss the impact of other terms, particularly moisture, to the optimization. Would this be a large or a small change - and would it improve or not the overall results?*

Moist total energy norm is discussed in Chapter 5. The difference to the dry total energy norm is the addition of a moisture component into the first sum term. We speculate that changing the cost function to include this moisture component would give more weight to correct presentation of tropical moisture fields, and would thus likely lead to different parameter posteriors. Without running a new experimentation we cannot tell how big a change this would impose on the optimization procedure nor if this cost function would lead to a model which is better than the one found here. **Added a reference to the moist energy norm formula, Chapter 5, p. 13 line 12.**

- *How should other modelers do their parameter tuning based on the lessons learned here?*

Parameter tuning can naturally be done by focusing on various aspects of the model. Targeting the optimization on direct effects of the parameterizations only (which in this case would have been cloudiness and precipitation) leads likely to improved model in the direct effect sense. The model changes achieved this way might however lead to a model which is deteriorated with respect to other model fields. If the goal of the optimization is a univocal model improvement it is more practical to focus the tuning efforts on model wide changes of critical model fields. With this in mind, the total energy norm offers a potential target for optimization procedures since it focuses on key features of the model, and takes into account the model changes in the all model layers. **Text added, Chapter 5, p. 13 lines 23-28, p. 14 lines 1-2.**