



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

Anonymous Referee #2

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General Comments

This paper presents the optimisation of parameters of a NWP or climate model by means of a augmented ensemble prediction system. It closely follows the procedure of (Ollinaho et al.,QJRMS 2013) but uses the total energy norm as a target criterium instead of 500 hPa geopotential height. The similarity to that paper is no shortcoming but a very valuable additional contribution because the choice of a suitable criterium for parameter optimisation is of great relevance.

Specific Comments

The estimation procedure itself is not rigorously explained within this paper. It is advisable to read (Ollinaho et al.,QJRMS 2013) first in order to understand the details. The

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relation to that paper should be made more clear from the beginning.

It could be mentioned that the total energy norm is not only used for seeking the fastest growing modes (as cited) but also for forecast sensitivity studies based on adjoints or forecast ensembles. (this kind of application is related even closer to this approach).

The paper presents the temporal evolution and final value of the standard deviation of the estimated parameters (Table 2, Figure 2). However the meaning of this uncertainty measure should be explained in more detail within this paper. Is it some objective measure of the estimated parameter or can it be interpreted only in the context of this estimation procedure (to draw a reasonable a priori ensemble).

The value of the parameter 'w' in equation (3) should be given, as well as a more concise reasoning for its choice. Has the value of w any influence on the final estimate of parameter uncertainties or on the convergence of the scheme?

Using the energy norm as a target has been shown to be superior to using geopotential height. This is contributed to the fact that deviations of model parameters from the analysis are constrained at all levels and not only at 500 hPa. It would be nice if this mechanism could be explored in more detail, for instance by showing zonal averages (pressure - latitude slices) of total energy contributions.

I do not understand the discussion of 'ambiguity' in terms of bias in the Discussion. Any bias, even if it changes sign within the model domain, will give a contribution to the squared analysis minus forecast differences used in the energy norm and thus will be penalised.

Also the term 'ambiguity of 500 hPa skill as a target' does not seem appropriate. The problem is not that 'many model realisations fulfill the target', but that these model realisations lead to inferior scores (other than those enforced to be superior by choice of the target). Only if all scores regarded to be relevant were included in the cost function (with appropriate weights) it could be assured that all scores would be improved (on

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average). This is probably no practical approach as not all desired properties may be addressed within the EPPES approach.

Technical Corrections

The citation (Ollinaho et al.,2012) (QJRMS) should be (Ollinaho et al.,2013)

Interactive comment on Geosci. Model Dev. Discuss., 6, 6717, 2013.

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