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

P. Ollinaho et al.

pirkka.ollinaho@fmi.fi

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- *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 relation to that paper should be made more clear from the beginning.*
  The EPPES methodology has now been explained more thoroughly. **Text added, Chapter 2, p. 4-5.**

- *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).*
  Forecast sensitivity studies are now cited. **Text added, Chapter 1, p. 3.**

- *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 priory ensemble).*
  The posterior distribution width is indeed an objective measure of the parameter uncertainty. The prior uncertainty should not play a very important role in this, although the distribution width will converge slower if the prior distribution width is too wide or too narrow. **Text added, Chapter 2, p. 4 and p. 5.**

- *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?*
  The value of w does influence the estimation procedure; it controls how many of the ensemble members influence the hyperparameter update, i.e. w acts to scale the pdf of the analysis field errors. This is done to prevent i) a too narrow error pdf, where only the ensemble member closest to the analysis would influence the distribution update, and ii) a too wide error pdf, giving all members equal likelihood. **Text amended and added, Chapter 3, p. 6-7.**

- *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.*
  We feel the article is already quite heavy with figures. Thus only a description of the vertical structure of the improvements was added. The figure is nonetheless attached. **Text added, Chapter 4.2.3, p. 11.**

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- *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.*
  Text removed, Chapter 5, p. 13.

- *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).*
  Our reasoning behind the ambiguity in this is as follows: Geopotential height is a summary quantity and is thus insensitive to the vertical profiles of the quantities which define it (temperature and humidity to great extend). Therefore a geopotential height profile negatively biased close to the ground and positively biased higher up could still lead to a good 500 hPa geopotential height (z500) RMSE score. Two different temperature and humidity profiles could therefore lead to same z500 RMSE scores. Moreover, "wrong" atmospheric states can lead to similar z500 RMSE scores as atmospheric states close to reality. To avoid confusion, we have clarified the text to emphasize that the same structure would only be observable at 500 hPa level. **Text clarified, Chapter 5, p. 13.**

- *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 average). This is probably no practical approach as not all desired properties may be addressed within the EPPES approach.*
  We agree that calculating the cost function from all relevant fields would be impractical. The energy norm implementation is experimented here in order to find a relatively simple cost function, which nonetheless would lead lead to a univocally improved model. **Text added, Chapter 1, p. 3.**

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- *The citation (Ollinaho et al.,2012) (QJRMS) should be (Ollinaho et al.,2013).* **Text amended.**

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