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Interactive comment on "Atmospheric forcing by ALADIN/MFSTEP and MFSTEP oriented tunings" *by* R. Brožková et al.

R. Brožková et al.

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On behalf of all authors I would like to thank both referees for their stimulating and constructive comments. These helped us to improve the manuscript at the places where the text was not completely clear and to point out interesting scientific questions, as well as to correct the technical shortcomings mentioned by the referees.

Answer to the specific comments:

To rely on the background field for short length scales is a technique used not only in the blending algorithm but also in the incremental data assimilation. For example, the ARPEGE 4DVAR analysis is run at the resolution of T149, while the forecast resolution is T358 (with a stretching factor 2.4 to increase the resolution over Europe). Similarly, ECMWF analysis runs at the resolution T255 and the forecast runs at the resolution

T799. Thus the shorter waves in the analysis are kept from the previous guess. To be correct, the forecast at full model resolution still influences the analysis by the forecast minus observation term computed in the so-called outer loop of the 4DVAR algorithm but it does not change the fact that the analysis is made at a lower resolution. The blending algorithm is inspired from this incremental approach: the initial state is composed from the analysed information at the longer scales and from the background field at the shorter scales. Of course, it can happen that the background field is more erroneous than usual; then there are consequences on the quality of the analysis in any case. The most important is to get the longer waves correct since the shorter waves rather adapt to them typically after two or three hours. Another important issue of the algorithm is the tuning of the split between the longer and shorter scales, alike it is in the data assimilation. The performance of blending was checked also by the comparison to observations over a longer period. The statistical scores were slightly improved, namely within the first hours of the integration. We think that our first redaction missed to explain better the incremental feature of the blending technique and therefore we added a small paragraph into the manuscript to clarify this point.

The horizontal diffusion coefficient depends on the flow deformation: the stronger deformation, the stronger diffusion, this is normal and in accordance with theory. The problem is to tune this dependency so that the kinetic energy cascade is controlled following the power (-5/3) rule and that there is no energy cumulated at the tail of the spectra. This ensures that we have on one hand a security in the model to control excessive developments, on the other hand that we do not remove from the solution realistic albeit sharp meso-scale features. Here also we added more explanations into the manuscript in order to be clearer.

It is a very pertinent remark of the referee that the SLHD diffusion acted quite strongly on the Black Sea case cyclogenesis (Fig. 2). We consider this as a positive feature of the SLHD diffusion to prevent any unrealistic deepening. But indeed, when introducing the moist gustiness and retuning the sea roughness length, we obtained a similar im-

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provement with respect to the control experiment to the one obtained with SLHD. It was a shortcoming not to mention it in the paper at first place. SLHD is not acting anymore so strongly when added to the model version with improved physics. The impact of the two changes does not cumulate and the rather correct simulation of the meso-scale cyclone is preserved.

We have also developed the conclusions and followed all the technical comments of referees in the revised manuscript.

Interactive comment on Ocean Sci. Discuss., 3, 319, 2006.