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Comment on gmd-2021-256

Anonymous Referee #1

Referee comment on "Prediction error growth in a more realistic atmospheric toy model with three spatiotemporal scales" by Hynek Bednář and Holger Kantz, Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-256-RC1, 2021

The topics addressed in this manuscript are very interesting since the study of the error growth in multi-scale nonlinear systems is fundamental to geophysical modelling, at various levels of complexity. The authors present a large set of results obtained from simplified multi-scale atmospheric models and discuss their relevance in comparison with ECMWF forecasting system data.

I think this manuscript needs some minor revision before publication. My remarks follow below.

1) I wonder why the authors did not carry out their analysis (i.e. error growth and error gorwth rate in nonlinear multi-scale systems) by adopting a scale-dependent point of view, i.e. measuring the scale-dependent error growth rate by means of the so-called Finite-Scale (or Finite-Size) Lyapunov Exponent (FSLE), a quantity that was introduced and developed specifically for this kind of studies, see E. Aurell et al., Phys Rev Lett. 1996, 77(7), 1262-1265; E. Aurell et al., 1997, J. Phys. A: Math. Gen. 30 1. The time-dependent description is not necessarily wrong or inaccurate but, in general, can be affected by "scale-interference" issues that do not help the investigation of power laws and scaling exponents, see also, at this regard, the discussion reported in Boffetta et al., 2000, Chaos 10 (1), pp 50-60.

2) lines 33-34: Why a scale-dependent error growth (small errors growing faster than large errors) should imply that the true Lyapunov exponent of the system is infinite ? The scale.dependent error growth rate actually is expected to converge to a constant value in the limit of infinitesimal errors, and that limit is assumed as the true maximum Lyaunov exponent of the system. The benefit of using a scale-dependent description of the error growth is just to have information on the error growth over all the observable scale range, regardless of the actual value of the maximum Lyapunov exponent, which is not very relevant if the smallest scales cannot be resolved.

3) lines 34-35: In a paper dealing with scale-dependent prediction errors, I think the FSLE-related bibliography cannot be reduced to only one paper (Cencini and Vulpiani, 2013). See point 1) for some more references.

4) line 225: In the formula of the Max Lyap Exp there should be an epsilon (i,e. the infinitesimal initial error size) in the denominator.

5) lines 403-404: In the linit of small scale errors, the growth is expected to be exponential, if the system is chaotic, so a power law is necessarily an approximation of the true behavior. If a power law looks like a reasonable approximation of small scale error growth, does this imply that the error size is significantly above the smallest scale limit ?

6) lines 416-424: it seems that, at least as far as ECMWF forecasting system is concerned, the authors' conjecture, although succesfully tested on their simplified 3-scale model, does not improve significantly the description of the error growth evolution with respect to other fitting functions.

In conclusion, I would like to stress once again that some points could be better clarified and the whole manuscript could be substantially improved if the authors cosidered checking their results with a FSLE-based error gowth analysis. Notice, apparently the quantity defined as error growth rate by the authors might look equivalent to the FSLE but there is a fundamental difference: the FSLE is an intrinsic scale-dependent indicator, i.e. it is measured by taking the average of the growth time at fixed scale, while the error growth rate reported in the manuscript, if I got it right, comes always from a fixed-time average. In case they were exactly the same quantity this should be clearly specified in the text.