**Ocean Science
Discussions**

Interactive comment on "Multifractal analysis of oceanic chlorophyll maps remotely sensed from space" *by* L. de Montera et al.

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General Comments:

This paper applies the codimension multifractal formalism to ocean colour data at high resolution. The paper is generally well written and overall is a useful contribution to the study of phytoplankton variability. However, I have a few comments and suggestions. My main scientific concern is the heavy emphasis which is placed on passive scalars. I understand that the authors ultimately find an exponent H $\approx$0.4 which is near the classical passive scalar value of 1/3, however, there are reasons for caution on this, some are indicated below:

1) Phytoplankton are far from passive. They are grazed by zooplankton, a process

which is apparently responsible for the "planktonoscale" scale break at about 100 m; (this was the proposed mechanism for the whitening of the spectrum at 100 m - corresponding to H $\approx$ -0.3 - in the cited Lovejoy et al 2001a,b and Currie and Roff 2006 papers; this was mentionned but not very clearly in the text on p. 57). The authors' data is at scales >1km, so this effect is not directly important. However, if the colour data are to be used to estimate plankton abundances (as the authors correctly discuss in section 8), then the ultimate scale where the scaling breaks down is important, so that this issue cannot be totally neglected.

2) Phytoplankton also display – at least on occasion – rapid growth; "blooms" - so that presumably the characteristic concentration doubling time is at least occasionally very important. Any other conclusion is tantamount to assuming that blooms are simply consequences of passive scalar advection! The latter effect is classical, and if totally dominant, leads to H = 0 (e.g. Denman and Platt 1976). Using in situ data, Lovejoy et al 2001b claimed H $\approx$ 0.12 - in between the two extremes implying that both effects are always present - and developed a dimensional analysis around this. Physically, the question is whether plankton patch lifetimes are roughly the eddy turn over time (the passive scalar hypothesis), or whether they are roughly the growth doubling time. . . or somewhere in between depending on both characteristic times. The authors should discuss this, and they might want to consider the image by image spectra to see if there is evidence of spectral exponents other than $\beta \approx$ 5/3, and for breaks, with transitions between regimes.

3) Finally, the spectrum of clouds is fairly smooth ($\beta \approx$ 2 depending somewhat on the wavelength), and even without "visible" clouds, there will be atmospheric effects that could potentially increase H. For example, this would be the effect of an exponential transmission function. In this regard, the authors' argument about the importance of nonlinear transformations of variables is quite pertinent (and what follows is contrary to both the referee and author positions in the supplementary material and the response to it). For example, this includes the fact that nonlinear transformations will generally

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lead to different scaling exponents – and the corollary that the scaling of the chlorophyll will indeed imply the scaling of the radiances - but with generally different statistical exponents. The reason is that in general cascade processes are singular measures, in the small scale limit, they do not converge at mathematical points (only in the neighbourhoods of points), they do not lead to pointwise singularities. Thus – at least for multifractals generated by cascades, the Holder exponents do not converge. The referee's argument about bi-Lipshitz invertibility therefore does not apply to them (there is no small scale at which the function is smooth enough, regular enough). Indeed, for universal multifractals, the change in the scaling exponents for η powers of a multifractal process are particularly easy to calculate; the C1 exponent is simply replaced by C1$\eta^{**}\alpha$. Note that these exponent changes occur in purely scaling processes. A key point is that the scale at which the nonlinear transformation is applied breaks the scaling and the process is "renormalized" by its ensemble average. More details on what happens to individual singularities may be found for example in: (Schertzer and Lovejoy, 1994). Incidentally, the fact that cascade multifractals are strictly speaking outside of functional analysis certainly weakens the case for the general application of tools of functional analysis such as wavelets for multifractals. At the same time, the structure functions used by the authors are in fact "poor man's" wavelets, so that the authors already use wavelet analysis!

Specific comments:

1) I have already indicated that it is a shame that the authors did not attempt to use spectral analysis on an image by image basis to see if there was evidence for the predicted breaks associated with growth dominated regimes. Similarly, in their flux (trace moment) analysis (fig. 5) they normalized the flux of each image before combining them into the ensemble estimate. This unnecessarily removes any variability due to the scales larger than the image scale and unfortunately prevents them from using their analysis to estimate the outer scale of the cascade process (which is presumably near planetary scales). This is a pity and a minor modification to their analysis would

enable them to recover this information.

2) Another simple improvement in the flux/moment analysis could be made: the data could be analyzed separately in the east-west and in the north-south directions. Even if the exponents turn out to be the same, presumably at least the outer scales will be somewhat different.

3) On p.60, it is mentioned that "self-similarity is at the root of the theory". This is not correct, the theory only requires scale invariance, not just the isotropic special case of self-similarity. Indeed, analysis of the type of scaling anisotropy is presumably important in understanding ocean eddy and chlorophyll patch morphologies.

4) Also on p. 5, the authors use the term "ultra metric"; I think this technical term should be explained to the readers.

5) Also on p. 5 just before eq. 5, the authors should remind the reader that the issue of Gaussian or Lévy refers to the generator of the process (i.e. the log of the bare process), not the process itself.

6) The paragraph following eq. 16 is not convincing: patches of rain drops and patches of zooplankton may share the property of scale invariance, but more a detailed relationship is unlikely.

7) Bottom of p. 63: a Gallicism, use "fractional derivative".

8) The end of section 8, and fig. 9: the authors should supply a better explanation of their PDF distribution and clarify what exactly is plotted in the figure.

9) The conclusions are too short and too weak. The authors could recall their parameter estimates and comment on future directions for scaling studies of phytoplankton and other ocean properties.

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References:

D. Schertzer and S. Lovejoy, Multifractal Generation of Self-Organized Criticality. In: M.M. Novak, Editor, Fractals In the natural and applied sciences, Elsevier, North-Holland (1994), pp. 325-339.

The other references are the same as in the paper.

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