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Ocean Science Discussions

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

Anonymous Referee #1

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General comments:

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The paper is well written and easy to undestand. Some of the discussions are very interesting, as for instance the introduction of the FIF model (although this is an old, well-known feature) and the discussion of biases in geophysical models. The application of the fractional cascade to assess the validity of the multifractal model, although not really novel, is an interesting approach. However, I have two (probably mild) objections to this paper.

First, I do not feel that the results obtained are really new. Seuront et al. have many papers on the multifractal characterisitics of plankton concentration that arguably obtain

essentially the same results, although in their case the range of scales studied is much smaller. However, there are several papers in the literature (e.g., Nieves et al. given in the references, and others from Mahadevan et collaborators) which study the scaling properties of satellite chlorophyll maps; from my point of view, a comparative discussion with those previous studies would better put into context the findings of this paper.

Second, the scaling quantity used for FIF is probably not the best suited one, particularly as the authors complain on the effects of noise at smaller scales. Wavelets were introduced long ago to deal with noise and discretization effects in multiscaling systems, and some old papers from Arneodo's group explain how to apply them to satellite images; other, more recent approaches (the authors mention several in the references) could probably used to overcome this problems. Although this does not restrain the validity of the results presented here, the use of these techniques could be considered to improve these results, possibly in future works.

Overall the paper is very good, and it probably deserves to be published once these recommendations are taken into account.

Specific comments:

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Errorbars are lacking, so we have no determination of the uncertainity in the estimated parameters.

On page 66 it is claimed that if chlorophyll concentration is the result of the application of a non-linear function f onto the reflectivity, the reflectivity itself does not need share the same scaling laws that chlorophyll. Well, in fact if the function f is locally invertible and smooth, it is guaranteed to be locally bi-Lipschiz, so over those neighborhoods were f is bi-Lipschitz the scaling laws are directly preserved (i.e., the Holder exponent of the chlorophill exponent and of the reflectivity are the same at the same point). The real problem with reflectivities is that in fact you need to combine several different channels

to retrieve the chlorophyll concentrations and this gives rise to cancellations (e.g., the scaling exponent of channel 1 is cancelled by a similar contribution but of opposite sign of channel 2), so the scaling exponents of chlorophyll need not to coincide with those of the compounding channels. Cancellations are mainly due to the presence of yellow matter or suspended sediments; aparte from the places where those effects are important, the scaling exponents of chlorophyll concentration and the used reflectivity channels will usually coincide.

Technical corrections:

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None so far.

Interactive comment on Ocean Sci. Discuss., 8, 55, 2011.