Comments on: Spatial and radiometric characterization of multi-spectrum satellite imagesthrough multifractal analysis

Carmelo Alonso, Ana M. Tarquis, Ignacio Zúñiga and Rosa M. Benito

Although the authors' similarity report found some similarity with other papers by the authors, my main concern was a lack of similarity with one of them! Indeed, the authors completely failed to cite the first and still the most comprehensive multifractal analysis of satellite derived vegetation indices. Without this, their own paper is without adequate context, their results are simply isolated numbers – and as we argue – the numbers that are kept are stochastic variables and hence will lack reproduceability. This failure is remarkable because the second author of the paper was a key author in the earlier more thorough and quantitative one the same subject.

In this paper, the authors use the multifractal *dimension* formalism of Halsey el 1986 that was developed for characterizing the deterministic phase spaces of strange attractors. In [*Lovejoy et al.*, 2008] co-authored by the second author in the present paper A. Tarquis (henceforth the "Tarquis paper"), it was explained in considerable detail why the dimension formalism is ill-suited for stochastic multifractals. Here, the images are assumed to be densities of multifractal measures, each realizations of a stochastic process. Co-author A. Tarquis can surely explain why the co-dimension formalism is more appropriate for the present application. She can also explain why the assumption of the existence of Holder exponents does not generally hold for stochastic multifractals and how the co-dimension formalism avoids this unnecessary (and doubtfully valid) assumption.

At the very least the paper must acknowledge the existence of the co-dimension formalism and refer to the Tarquis paper. The authors should also give the formulae:

$$f(\alpha) = d - c(\gamma); \quad \alpha = d - \gamma$$

$$\tau(q) = d(q-1) - K(q)$$

where d is the dimension of space (here d =2) and $c(\gamma)$ is the codimension of the singularity of the density of the multifractal measure γ (γ is related to the singularity of the measure α by the formula $\alpha = d - \gamma$ above) and K is the moment scaling function of the density of the multifractal measure (i.e. it directly characterizes the scaling of the moments of the image rather than the integral of the image). These formulae are necessary in order to compare results obtained in the two formalisms (i.e. with the rest of the literature).

One of the advantages of the codimension formalism is immediately obvious from the formulae: $c(\gamma)$, $K(q)$ are independent of the dimension of the embedding space d whereas $f(\alpha)$, $\tau(q)$ are different where ever one looks at subpaces of the process (i.e. the same process but observed at different *d*). An related advantage of the codimension formalism is that when one performs the moment analysis (e.g. their figs 3, 6) that the moments will not dominated by the trivial, deterministic scaling factor $l^{d(q-1)}$ but will directly show the key (and usually much smaller) $l^{-K(q)}$ part (see the expression above; such an analysis is called "trace moment analysis"). As it is, the quality of the scaling of the statistics is practically impossible to judge from the authors' figures.

In addition - also as explained in the Tarquis paper - the moments $q<0$ will in general diverge so that special care is needed to avoid spurious estimates.

As carefully explained in the Tarquis paper, the multifractal spectrum f(α) - or better, c(γ) - is a function; empirically it corresponds to estimating an infinite number of parameters. Since the framework is of stochastic processes, and in general stochastic multifractals have unbounded spectra (i.e. c(γ) is generally unbounded), the authors differences $\Delta_\pm$ are simply random variables, they will provide very poor characterizations of the process. Why don't the authors characterize the multifractality as explained in the Tarquis paper (using C_1, and the multifractal index α - not the same as the authors' α)? An added bonus would be that they could quantitatively compare their results with others in the literature (including those in the Tarquis paper!), rather than simply obtaining an isolated result with no context, no point of comparison. There are other ways of quantitatively characterizing the multifractality, but the singularity range used here is a particularly poor choice.

Another problem with the authors' characterization technique is that it ignores the issue of multifractal phase transitions that is extensively dealt with in the Tarquis paper. The authors should check that their moments (up to the rather high value of q = 5) are not spurious.

Some of the other conclusions of the Tarquis paper could also be recalled and the authors' new results could be then be quantitatively compared.

Conclusion: This paper should not be published without proper citations and comparisons with the Tarquis paper.

Detailed Comments:

Section 2.2, line 2: The authors state:
"A multifractal analysis is basically the measurement of a statistic distribution and therefore gives useful information even if the underlying structure does not show a full self similar behaviour (Plotnick et al., 1996). "

This is incomprehensible since isotropic multifractals assumed to be self-similar (i.e. scaling and isotropic), and the authors do not consider anisotropy in this paper. It is more correct to say that:

"A multifractal analysis is an analysis of how the statistical properties of a scaling field (or series) varies with scale. It therefore does *not* give useful information when the underlying structure is not scaling."

References:

Lovejoy , S., A. Tarquis, H. Gaonac'h, and D. Schertzer (2008), Single and multiscale remote sensing techniques, multifractals and MODIS derived vegetation and soil moisture, *Vadose Zone J.*, *7*, 533-546 doi: doi 10.2136/vzj2007.0173.