

Interactive comment on “Fractal metrology for biogeosystems analysis” by V. Torres-Argüelles et al.

L. Sorriso-Valvo (Referee)

sorriso@fis.unical.it

Received and published: 22 June 2010

General comments

The paper describes a number of different techniques to estimate the fractal dimension (or the Hurst exponent) from images of different samples of soil. The results are compared and discussed. The article is potentially publishable and results are interesting. However, before publication there is at least one major point to be clarified.

I have to admit that my knowledge about soils is absolutely inadequate to comment on that part of the paper, but as far as I understand, the authors give a good description of a soil degradation by comparing different samples of the same land, and explain the origin of the transition.

Specific comments

As far as the statistical analysis is concerned, I have one major comment (probably easy to answer from the authors). The comparison between the techniques is based on the evaluation of the "standard deviation" provided by the commercial toolbox Benoit, used as an indicator of the uncertainty of the resulting Hurst exponent. From my experience, the parameters provided by toolbox can be sometimes unclear and obscure, since the customer often do not have knowledge of what exactly the parameter is meaning. This introduces a weak point in the paper, namely the relevant parameter used for the comparison is not defined in the manuscript, and the reader is left with a very generic reference to a "standard deviation" provided by the toolkit. I recommend the authors to explain in the article what exactly this standard deviation is, how it is computed, and therefore why it can be safely used (if the case) to represent the uncertainty of the estimates.

A second comment on the results concerns the discrimination between different tools. As an example, I read in table 5 that H_box (image) and H_RS (Pdf) give similar good, small standard deviation (thus accuracy), but very different results for H. How do the authors discriminate between these results? Shouldn't the different methods provide similar results?

A third and last comment: when translating the images into firmgrams, the authors put in a single vector the rows of the image matrix. This operation is sensitive with respect to isotropy. What happens if, instead of rows, authors would use columns (thus putting side by side the values in column, rather than in rows)? If the data are isotropic, there should be no difference, but this should be tested and reported in the manuscript. A more conventional way to do that would be to compute both the vertical and horizontal firmgrams, and then average the two cases (if reasonably similar), or comment separately (if sensibly different).

Technical corrections

BGD

7, C1506–C1508, 2010

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* Dimension indicated as 2 \rightarrow d (to avoid confusion, Euclidean space dimension should be indicated generically as d, OR it should be explicitly said that in this work the images are bi-dimensional)

* Use of the name "time" series does not look appropriate, since there is no time involved in this study. I understand the purpose of the authors, but then they should at least explicitly mention that the "time" is not related with any physical variable.

* A reference for the pipette method would be useful.

* Table 1: (check the alignment of the labels for all tables 1-4); the result for D-19-Chernozem from the two techniques is not consistent. this should be acknowledged in the text.

* Tables 6 and 7 are not reader-friendly. The authors should try to use a correlation plot instead? (2d image, using a few color code bins for the correlation value, ex: from blue (-1) to red (+1) passing through white (0)).

Interactive comment on Biogeosciences Discuss., 7, 4749, 2010.

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