Ocean Sci. Discuss., 6, C903–C906, 2009
www.ocean-sci-discuss.net/6/C903/2009/

Ocean Science Discussions

Interactive comment on "Spatio-temporal complexity analysis of the sea surface temperature in the Philippines" *by* Z. T. Botin et al.

F. Boschetti (Referee)

fabio.boschetti@csiro.au

Received and published: 15 December 2009

I have read the above mentioned paper. Please notice that I have an applied mathematics and complex system science background and these are the two aspects of the paper I feel I can contribute a review to. I am not comfortable with commenting on the oceanography aspects of the paper and I hope you will receive useful comments on this topic by others reviewers.

I believe the paper describes an interesting approach and a numerical method which is both fairly simple to implement and easy to extend to a wide variety of data. I believe these results are worth disseminating to the larger community and naturally fit the purpose of your journal. The paper is well organised and the overall material is well presented; I thus recommend the paper be accepted. I also believe that some further

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

clarifications could be provided in some sections and include below a few comments which may help the clarity of the paper.

Main comments:

1. I think the algorithm is relatively well explained, but since it involved 3 main modules (selection of spatial regions via K-means, STC calculation via moving blocks and EOF compression of plot) I think a flow-chart of the overall scheme would be very useful.

2. Unlike figure 5, Figure 4 seems quite hard to analyse visually and to extract some clear patterns. Consequently, I find the discussion in section 3.2 not very satisfactory. Could the authors devise a numerical measure which could detect in a less subjective manner some of the features they discuss?

3. Section 2.3; this is the part of the paper I find the least clear, in the sense that I would not be able to replicate the algorithm if I wished to do so. Why only 6 areas where chosen? Are they representative of the entire domain, or are they somehow anomalous? How were their borders selected? Was the K-means algorithm run on the overall domain? Was it given only temperature input data or both temperature and spatial data?

4. Page 2833, lines 16-18 “Other information-based measures [. . .] have been applied to the analysis of temporal data (time series) or to spatial data but not to both types of data”. This is not correct, see Shalizi, C. R. and Shalizi, K. L., 2003, Quantifying Self-Organization in Cyclic Cellular Automata, in Noise in Complex Systems and Stochastic Dynamics, Lutz Schimansky-Geier and Derek Abbott and Alexander Neiman and Christian Van den Broeck, Proceedings of SPIE, vol 5114, Bellingham, Washington, for example.

5. Page 2834, line 15; “The 50km resolution data has been recently refined to 4 km resolution”; it is reasonable to believe that this conversion may affect the STC

OSD

6, C903–C906, 2009

calculation, by imposing either distortions or excessive smoothing. Maybe the author can comment on this. What was the main reason for using the refined data set?

6. Page 2835, first paragraph. Since the authors suggest the data is not stationary, I wonder why two different thresholds were not used. In fact, it is possible to conceive that the algorithm could provide reasonable information even by choosing a moving threshold, at least for each spatial location. Could the author discuss this issue?

Minor comments:

1. Line 26 “our objective is to characterize these dynamics”; in fact, I think the paper in its current form describes an algorithm to detect variations in a specifically defined complexity measure rather than characterise them. Could the authors provide an interpretation of the physical or oceanography meaning of the STC? Or of its variation?

2. Line 26-28 “SST dynamics of the region may affect ecological properties such as the resilience of a system to warming events” I agree this may be possible, but the paper does not address this issue nor does it provide any evidence that it could be so.

3. Page 2833, line 18; typo “;” should be “and”

4. Page 2833, line 27; without being too technical, the authors may explain what they mean by ‘complex’ in the context of this analysis.

5. Page 2834, line 14; 50km - $>$ 50 km resolution

OSD

6, C903–C906, 2009

6. Page 2836, line 24 onward; ".. unlike Shannon entropy which assigns the highest value to randomly generated data, STC assigns intermediate values for randomly. . . "; this is slightly misleading: as far as I understand, for n=1 STC would also assign high value to random data, so the issue is not whether to use Shannon entropy or not, but on what structure Shannon entropy is calculated.

7. Page 2837, line 26; "EOFs provide the most efficient method of compressing data"; this is a very strong statement, which is not correct in general, surely not for highly non linear data.

Fabio Boschetti

http://www.per.marine.csiro.au/staff/Fabio.Boschetti/

Interactive comment on Ocean Sci. Discuss., 6, 2831, 2009.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper