**Ocean Science
Discussions**

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

Z. T. Botin et al.

rcdelros@gmail.com

We would like to thank the reviewers who carefully read our paper and provided very helpful comments. We have made two major changes to the paper:

Major change 1: Use of a moving average threshold value

Following the excellent suggestion of the reviewers, we used a moving average window to compute the threshold value used for converting the sea surface temperature (SST) data to binary data for the spatiotemporal complexity (STC) analysis. The new results, based on this moving average threshold, are substantially different from the previous results.

Recall that previously, we had used a fixed threshold of approximately 25 degrees

Celsius, which was the average SST in the Philippines prior to the Pinatubo eruption. In some cases (e.g., in times of extremely hot weather, particularly after the eruption), all of the SST values are above 25 degrees, causing all cells (except for the land mass) in the binary matrix to be assigned a value of 1. The result was consistently low values of STC for each year during the summer. By using a moving average as threshold, we are able to better represent the relative spatial variability in SST values at each moment in time. Given the non-stationarity of our data set, as well as the large annual cycle in temperature values, we believe the moving average threshold is a better choice to use for these analyses.

Overall, the use of the moving threshold has the effect of clarifying our results. We now have much clearer figures and are able to detect a significant difference in the SST dynamics at the country level for different ENSO years.

Our new STC plots for the Philippines are given in the attached figures (Figures 1 to 3 – all figures are in the attached Supplementary File). For consistency, we have also recalculated the STC of the six thermal regions using moving average thresholds (Figure 5 and Table 1). We give an interpretation of the new results in our responses to the specific comments of the referees below.

Major change 2: Classification of ENSO years

We changed the classification method used to identify El Nino, La Nina or normal years. In the version of the paper under discussion, we used the classification in
http://www.oc.nps.edu/webmodules/ENSO/ensoyears.html.
This index only classified years up to 1996. After submission to Ocean Sciences, we found a more updated classification at
http://ggweather.com/enso/oni.htm.
Thus, we classified all years between 1985 and 2005 as either El Nino or La Nina according to the updated classification, and the remaining years are considered "normal" in our discussion.

Our answers to the specific comments of the reviewers are below.

First Reviewer

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.

Response: A simple flow chart will be included in the revised manuscript.

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?

Response: The reviewer is referring to the thermal region analysis. We agree that this figure is very difficult to analyze visually. This figure will be removed from the revised version. The new analysis of the thermal regions based on a moving threshold shows them to all have relatively high STC (see Figure 5 below). No significant difference in the SST dynamics of the thermal regions can be detected using the STC analysis.

As a consequence, the discussion of the thermal region results (section 3.2) will be replaced by a short comment describing the fact that the STC analysis does not detect differences between them.

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?

Response: Due to data limitations imposed by the software DISCO, we calculated the monthly average SST value of each pixel of the Philippine-wide data. Input data files for DISCO were prepared by arranging each pixel data in one row, with each column containing averaged SST data for a month. Then the K-means algorithm (in DISCO) was employed on the entire domain and was allowed to search from 5 to 10 clusters. It chose 6 clusters as the optimum number, as determined by the "Minimum Description Length" (MDL). The centroid of each cluster was obtained and a rectangular region, with the centroid as its center, was selected to fit the cluster.

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.

Response: Thank you for this reference to conference proceedings of which we were not aware. The method of Shalizi et al. (2003) is an interesting method based on analyzing spatial series. It involves calculating probable future states in space based on past and future states and could be potentially useful for describing the degree of randomness or regularity in spatiotemporal data. We have included a reference to their work in the revised manuscript, but retain our belief that information-based measures applied to spatiotemporal data are, at the very least, rare! The sentence in the manuscript has been revised to state the relative scarcity of spatiotemporal measures as opposed to their absence.

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

Response: We overlooked this sentence and should have deleted it in the submitted manuscript. In the calculations we performed before writing the manuscript, we used 50 km resolution for the whole-country analysis to lessen data size but in the manuscript figures, we used the 4 km data provided directly from NOAA. This data-resolution aspect is deleted in the revised paper.

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 con- ceive 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?

Response: We have revised the manuscript to use a moving threshold. See the major changes section above.

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 inter pretation of the physical or oceanography meaning of the STC? Or of its variation?

Response: There is no physical or oceanographic meaning attached to STC. STC just measures a pattern. We then interpret that pattern, and use changes in the pattern as a proxy for changes in the system's dynamics. The objective is thus both to characterise the complexity of the system's dynamics using the STC measure and to use this characterisation to detect changes in STC over time and to detect differences between spatial regions.

2. Line 26-28 "SST dynamics of the region may affect ecological proper ties 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.

Response: We will delete this sentence in the revised version. Our goal in the paper

is to introduce the method for possible use in analyzing different remotely sensed data (e.g., rainfall, temperature, wind) and in this paper, we concentrate on SST.

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

Response: OK.

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

Response: A sentence has been added here to define "complexity" in the context of this analysis.

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

Response: OK

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 Shan- non entropy or not, but on what structure Shannon entropy is calculated.

Response: The reviewer is correct. For n=1 we would have 2 possible occupation levels (empty or full). For random data, they would have the same frequency, giving an even distribution and thus STC = 1. The text has been modified to specify that we are dealing with cases where n > = 2, which, for a spatiotemporal analysis, is necessarily the case.

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.

Response: Agreed. The word "most" has been replaced by "an" in the revised manuscript.

Second Reviewer

Specific comments: These are my major concerns, most important first:

1. STC is supposed to be measuring patterns of anomalies and structures in the SST space-time field. Unfortunately the fixed threshold (25.2 C) of the SST matrix into binary (p2835 L5-15) ensures that the dominant 'pattern' will be the seasonal SST cycle (even in the tropics), causing larger blobs when the region is warmer and smaller blobs when cooler. It is explained (p2836 L13-24) that many smaller blobs will generate a higher STC value, and larger blobs a lower STC. There is only one small section at the end of the discussion (p2845 L15-20) which recognises the possibility that higher overall temperatures may influence the STC; but otherwise throughout the paper there is an implicit assumption that the STC is indicating the complexity of the SST field.

Figures 3 and 4 provide evidence for this problem, in that the STC values are often truncated at a low value (<0.2). I believe this indicates the weeks when the majority of the region was either above or below the threshold, and hence the method could not measure the complexity/heterogeneity. The authors have not considered this possibility. The peaks between the truncated troughs are probably when the average SST of the region was closer to the threshold value.

Response: We agree and apologize for having overlooked the possibility that, by using a fixed threshold, we ended up with truncated troughs corresponding to periods when all of the SST values were above the fixed threshold. We have re-done all of our calculations using a moving average threshold with different moving window sizes. The results are shown in Figures 1 and 3. See also our comments re: major changes above.

A revised paper would need to explicitly consider the background SST signal, using (a) a scatterplot of STC vs mean SST; (b) mean SST vs time on the same axes as STC. I predict that the scatterplot would show clear structure, perhaps with a peak STC around the SST threshold value and lower STC when the mean SST is high or low.

Response: This is a good idea. Please refer to Figure 5, in which we see that with the new moving threshold, high STC values are associated with times of high average SST. Looking at the STC versus the standard deviation of the SST values shows a clear contrasting relationship: high STC values are associated with a low standard deviation of the SST (Figure 4, right column). This relationship is intriguing, and suggests that the ocean has a high relative degree of spatial heterogeneity (STC) during the hot summer months and low spatial heterogeneity (yet high overall (non-spatial) variance) in the cooler months. This corresponds to prevailing weather patterns, that allow for large contiguous patches of cooler or warmer waters to form during the cooler months. The high STC of summer months could also be due to the typhoons whose short durations in contrast to the rest of the year do not introduce much variability to the data at this two week temporal resolution.

The method may possibly work by using an adaptive rather than fixed threshold, for instance the mean or median SST for each weekly time slice. The median would ensure that there are equal numbers of pixels above and below the threshold, and hence STC would measure the complexity of SST structures.

Response: We agree with this suggestion and have tested the algorithm using a moving average threshold with different temporal window sizes. See previous points and attached figures.

This issue invalidates all the results and interpretation. (Including EOF: I expect the dominant modes are related to the duration of the too-low/too-high SST periods.) I wonder whether graphs of mean SST vs time would show clearer distinction between El Nino/La Nina/Normal years than STC? If so it is difficult to justify the value of STC for this application.

Response: The EOF analysis was affected by our use of a fixed threshold. The EOF analysis of the new results shows clear dominant modes for El Nino/La Nina/Normal years at the whole country level (Figure 4). Contrary to previous results, the EOF anal-

ysis of the STC of the thermal regions does not show any clear differences between the thermal regions, nor does it differentiate ENSO events at the level of the thermal regions.

2. There are no statistics to prove any connection between the STC or EOF metrics and the ENSO, only qualitative observations. The paper would need standard time-series comparison statistics with significance testing. p2843 L4 chooses an ad hoc definition of 'significant', but this should always be 'less than 5

Response: The discussion comparing STC or EOF metrics and ENSO will be deleted in the revised paper.

3. The SST data matrix is divided into time slices to allow analysis of STC vs time (p2836 L4-12). I am concerned that the choice of 3, 4, 5 weeks is much too short to represent the spatio-temporal complexity, so only the spatial complexity will be measured. Indeed with a time slice of 3 weeks there will only be one time position for the 3x3x3 kernel cube. This concern is borne out in Figure 3, where there is negligible difference between the 3 slice durations.

Response: Here, in fact, so as to see how STC varies over time, we have chosen a short temporal window (3, 4, 5 time slices). Since the data is measured bi-weekly, this corresponds to 6, 8 and 10 weeks. Using a larger time slice would eliminate any of the seasonal variations in the data, rendering the temporal analysis uninteresting. We have therefore continued to use time slices of 3,4 and 5 in our updated analysis.

4. Section 2.3 identifies 6 sub-regions with distinct thermal properties for detailed study (p2837 L14-21). I am concerned that the clustering based on the SST trend per pixel may actually conflict with the STC method. If the result is to identify regions each with similar SST trends, then won't these be less temporally complex than typical?

Response: Our aim in using thermal regions obtained via clustering is precisely to determine if their STC are higher or lower than typical. As our new results (with the

moving average threshold) indicate, the STC of the thermal regions do not exhibit obvious differences (Figure 5 and Table 1) and show mostly high STC values.

Response to Technical Corrections: We will incorporate the necessary technical corrections in the revised paper.

Response to Additional comments after reading other reviews: We have used a moving average threshold as suggested by the reviewers.

Please also note the supplement to this comment:
http://www.ocean-sci-discuss.net/6/C1095/2010/osd-6-C1095-2010-supplement.pdf

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