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Ocean Science Discussions
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# *Interactive comment on* "Revisiting Tropical Instability Wave Variability in the Atlantic Ocean using SODA reanalysis" *by* Hatsue Takanaca de Decco et al.

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Dear Editorial Board,

We would like to thank you for considering our manuscript entitled: "REVISITING TROPICAL INSTABILITY WAVE VARIABILITY IN THE ATLANTIC OCEAN USING SODA REANALYSIS", for publication in Ocean Science. Please, find in the following our detailed response to the comments and suggestions of Reviewers, point by point.

The most important change in this version was the modification of figures 2, 9 and 11. This change was a recommendation made by Reviewer #2, which we agreed that



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would improve the comprehension of the discussion in the text. Minor changes were made in the Introduction.

We are very glad with the revision process of Ocean Science and we thank you for this opportunity!

Please, find attached the letter for the Reviewers.

All of our best,

The authors. **********
Response to Reviewer #1:

Comment #1: "The manuscript attempts to investigate the seasonal variability and the generation mechanism of tropical instability waves (TIWs) in the Atlantic Ocean using reanalysis data." Reply #1: We would like to thank you for your interest in our study. Your contribution had a great value to us. Yes, this is the main goal and we thank you for your interest on our study. Although stated as a general goal, we considered that the study generated results that broaden the discussion of TIWs energetics. Our main purpose is to review TIWs energetics in the Atlantic Ocean when using a more specific method for extracting TIWs signal, which was the bidirectional filter.

Comment #2: "Unfortunately, the manuscript is highly disorganized and diffuse, and I am left wondering what the new finding of this work is." Reply #2: We apologize for this and attempted to improve the manuscript following the suggestions made with the review.

Comment #3: "Below are some examples of those points which definitely need further improvement." Reply #3: Ok, we really appreciate that.

Comment #4: "I am afraid that the manuscript is not suitable for publication in Ocean Science." Reply #4: We are committed to dedicate our time on improving the manuscript and its discussion.





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Comment #5: “It appears that one of the major conclusions of this work is that barotropic instability is the main generation mechanism of TIWs in the Atlantic Ocean.” Reply #5: The major conclusion of the work is that the annual periodicity of TIWs is the most important variability of these waves. The pattern of energy exchange between TIWs and the Equatorial circulation was also a new result as the baroclinic instability was found to be the responsible for the energy flow back from TIWs to Equatorial circulation. This is highlighted between Lines 480 and 494 of Page 14.

Comment #6: “However, the importance of barotropic instability in generating TIWs has already been pointed out by numerous previous studies. Just confirming a well-known fact using different data set is not sufficient to be a paper suitable for Ocean Science” Reply #6: Yes, we agree that barotropic instability in generating TIWs has already been pointed out by previous studies and we have cited them in the manuscript, included in the discussion.

Comment #7: “Another important conclusion of this manuscript is that the intensity of the barotropic energy conversion rate varies in the annual and semi-annual cycles, which is shown by performing spectral analysis. But this is again a quite natural result.” Reply #7: The discussion of how barotropic energy conversion rate related to TIWs signal varies within a year has not been studied yet. We are not aware of any paper in this specific subject. Just the Philander’s discussion about the context of TIWs within Equatorial dynamics and we cite his papers in the discussion in the manuscript. Please, if you have any suggestion for reading, we are open to broad the review about TIWs in the Atlantic Ocean and we will be very glad with this contribution.

Comment #8: “The authors note that “the TIW eddy kinetic energy was almost constant at seasonal scale (Pg. 10, Line 340)”, but they also note that “the present analysis revealed and reinforced the intense seasonality of TIWs (Pg. 11, Lines 379-380).” Which is the correct message?” Reply #8: This comment refers to the presentation of the results that show the presence of TIWs signal in all the four seasons of the year (Figure 8). In page 11, the sentence “The present analysis revealed and reinforced





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the intense seasonality of TIWs, but this was not the dominant form of variability", at Lines 379-380, highlights that we are corroborating previous studies but the seasonal character of TIWs is not the dominant periodicity found by us. This is the same idea exposed on Page 10, Line 340 of the submitted version of the manuscript.

Comment #9: "I cannot figure out what Figure 9 shows. The authors conclude that TIWs vary annually based on this figure, but if this figure is the power spectra of temperature and other variables (as described in the figure caption), the spectral peak at the annual period cannot be a signal of TIWs, whose period is ∼30 days." Reply #9: We used the TIWs signal (periods from 15 to 60 days and wavelengths from 4° to 20° of longitude) as a filtered and separated time series to study the variability of these waves. We applied the fast fourier transform analysis to this time series for the average areas defined in Figure 1. The energy spectra for each variable of this time series of TIWs isolated signal is shown in Figure 9. As a recommendation of the second Reviewer, we have modified the frequency scale in Figures 2, 9 and 11. We would like to invite you to see if these changes can bring more clarity for your comprehension.

Comment #10: "The introduction is specially too diffuse. The authors should summarize the results of previous studies and remaining issues in a more organized way and state the specific purpose of the present study more clearly." Reply #10: Our goal with the Introduction was to summarize the main results of previous studies, since it is a paper with a review approach. We covered the literature review to show the reader a broad picture of TIWs studies and the conclusions generated with them. We will be able and are committed to improve the Introduction with specific comments about it. We changed the statement of the goals of the manuscript. Please, find it at Line 146 of Page 4 of the reviewed version of the manuscript.

Comment #11: "The authors seem to emphasize the use of "bidirectional filter (spatial and temporal filter) in their spectral analyses. If this method is the important progress in the present study, they should demonstrate the advantage of using this filter in comparison with a filter in space or time domain alone." Reply #11: We have not





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showed the differences between time series filtered only on period and/or wavelength and the TIWs signal extracted with the bidirectional filter because we believed that the result of the analysis, when compared to literature, is the advantage of this method since any previous study has used it. Also, one of the first versions of this manuscript included this comparison and it left the text too long and lost the objective of the study. This is the reason we do not show it explicitly in the manuscript.

Please also note the supplement to this comment:
http://www.ocean-sci-discuss.net/os-2016-84/os-2016-84-SC1-supplement.pdf

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Interactive comment on Ocean Sci. Discuss., doi:10.5194/os-2016-84, 2016.



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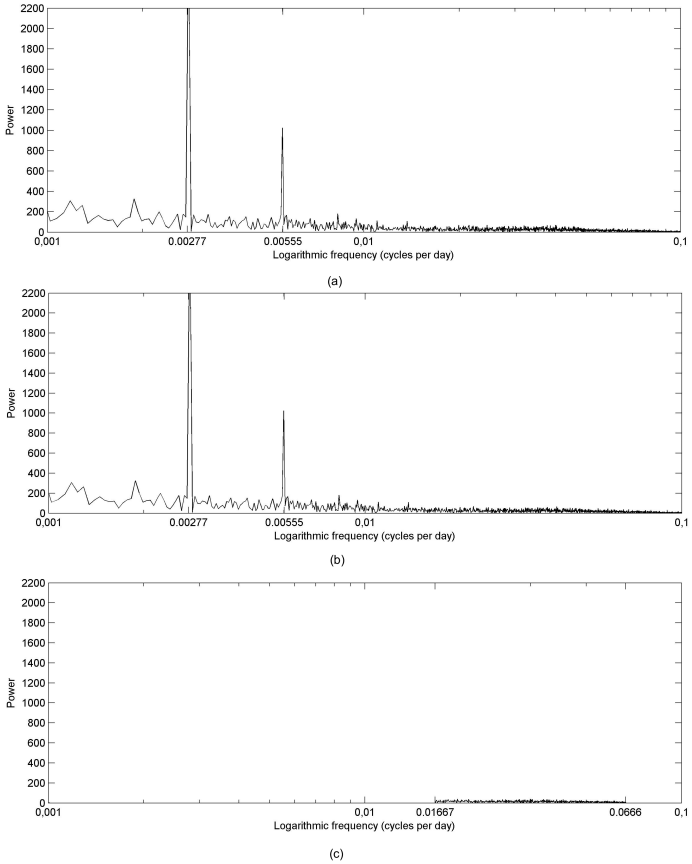


**Fig. 1.**





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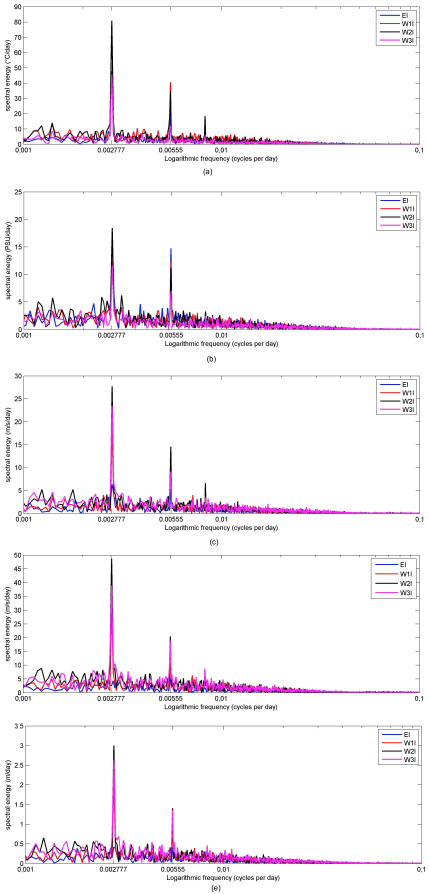
EI
W1I
W2I
W3I
spectral energy (°C²/day)
spectral energy (PSU²/day)
spectral energy (m²/day)
Logarithmic frequency (cycles per day)
0.001
0.002777
0.00555
0.01
0.1
(a)
(b)
(c)
(e)

**Fig. 2.**





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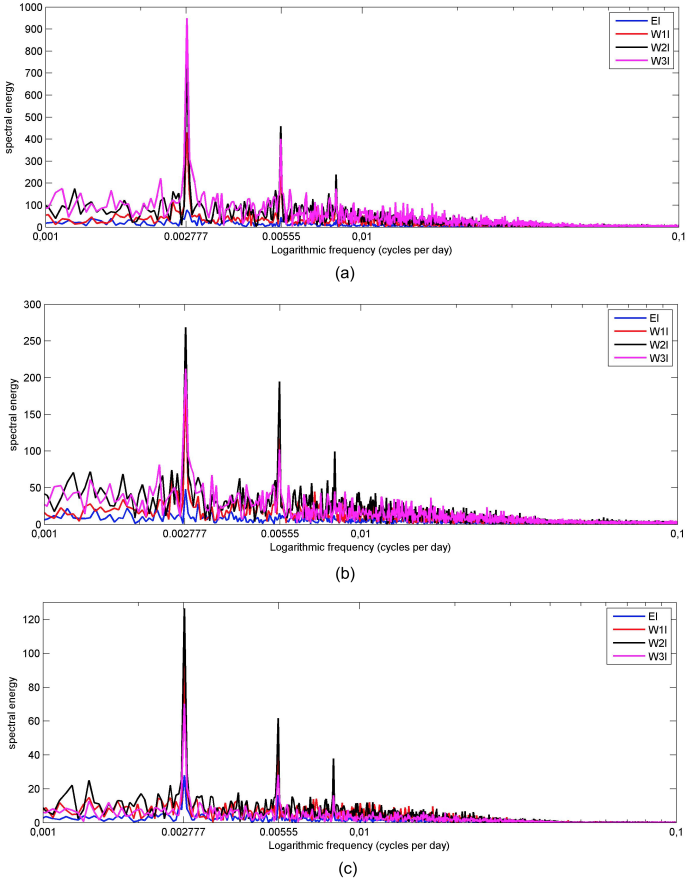


**Fig. 3.**



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