

## ***Interactive comment on “Multi-year satellite observations of instability waves in the Tropical Atlantic Ocean” by A. C. V. Caltabiano et al.***

### **Anonymous Referee #1**

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Combining multi-year satellite observations of sea-surface temperature (SST), winds, atmospheric water vapor (VAP), liquid cloud vapor (CLD) and precipitation rains RAIN), the authors aim first at describing the main properties and interannual variability of the tropical instability waves (TIW) in the Tropical Atlantic Ocean and then at inferring both the nature and the spatial extent of the coupling processes that take place between the ocean and the atmosphere in the presence of the TIW.

This study provides the first multi-year analysis of the SST signature of the tropical instability waves in the Atlantic from a satellite perspective, thus allowing a close description of the impact of the Tropical Instability Waves on the Sea Surface Temperature and on the atmosphere just above from intra-seasonal to interannual timescales. The main results of this study are that the air-sea coupling mechanism acting at the location of the TIWs originates from the density stratification change within the atmospheric

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boundary layer due to the local SST anomalies associated to the TIWs, and that this coupling may have an influence on the Intertropical Convergence Zone (ITCZ) in the tropical Atlantic, unlike the tropical Pacific where the ITCZ takes place farther north and so beyond the TIW influence.

Despite these results and the thorough bibliographical review that is proposed by the authors, I would not recommend this paper to be accepted in its present form and would suggest either to re-submit it as a short paper, or to go deeper in the analysis of the satellite observations to detail and reinforce the conclusions of the present version of the manuscript. My main concerns about this paper are the following:

(i) This paper does not provide a multi-year analysis of the instability waves in the Tropical Atlantic Ocean, as claimed by the title of the paper, but of the SST signature of the tropical instability waves and the resulting air-sea coupling processes. It seems to me that this difference is essential insofar as the TIWs, from an oceanographic viewpoint, may still exist even in the absence of a clear signature in the SST field, for example anytime outside the SST fronts or the years when the cold tongue is less intense. Besides, part of the TIW variability, as seen by satellite observations of the sea surface temperature, may be directly forced by the temporal variability of the structure of the equatorial Atlantic basin, namely the amplitude of the cold tongue, the precise location of the SST front at its poleward limits and the intensity of the zonal surface currents. Since observations of the temporal variability of the cold tongue are available in the dataset used by the authors, I think that these questions should be addressed in details in a long paper dealing with the TIW from satellite measurements. Consequently, to my opinion, the study should either focus exclusively on the analysis of the coupling processes at the location of the TIWs (in this case, the title of the paper should point out that and the results should be the subject of a short paper), or should also address the question of the TIW variability in the light of the SST variability at other temporal and spatial scales.

(ii) A large part of the results described in this paper is based on the analysis of the

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co-variability of SST and of winds, once a two-dimensional westward-only FIR filter with a bandpass of  $5^{\circ}$  to  $20^{\circ}$  in the longitude and 20 to 40 days in time was applied to both SST and winds to isolate the TIW-induced anomalies. Two noticeable, and somewhat surprising, features of the filtered SST and wind fields may have an impact on this analysis, though they are not discussed in the paper: first, figure 4 suggests that strong SST anomalies are observed during the boreal winter months, though of lesser amplitude than their boreal summer counterparts, for example at  $3^{\circ}\text{N}$  in 1999, at  $2^{\circ}\text{N}$  in 2000 or at each latitude in 2001; do these SST anomalies correspond to Tropical Instability Waves and have they been described already in the literature? Second, the comparison of figures 4, 6 and 7 suggests that, unlike SST anomalies that occurs exclusively in the central Atlantic, wind anomalies are strong throughout the basin width, especially in the eastern basin, and that winter and summer signals have about the same amplitude. Is there a way to conclude whether the wind anomalies are only TIW-induced or may result of other processes? If we assume that these anomalies are only induced by the SST anomalies of figure 4, how could one explain that the wind anomalies occur primarily before, and east of, the SST anomalies, thus suggesting that, contrary to what the authors claim, winds anomalies are not seen as a response to SST anomalies, but rather force, once generated in the east, SST anomalies during their westward propagation! This point needs obviously to be discussed and clarified. Finally, as noted by authors, the SST TMI dataset covers a 4-years period (1998-2001), whereas the wind QuikSCAT dataset is only available for 2 years (2000-2001). This discrepancy in the temporal coverage may be all the more crucial since 2000 is a year of weak TWI, unlike the three others years. In particular, as suggested by figure 5b, the reference point ( $1^{\circ}\text{N}, 15^{\circ}\text{W}$ ) for the co-variability computations proves to be arbitrary for the year 2000 since no clear maximum can be seen in the SST standard deviation for that year; can it not be a source of important errors when computing co-variability over only two years, as it is the case for the winds?

(iii) The influence of the TIW on the ITCZ in the tropical Atlantic is obviously the major novelty of this study, but still needs to be completed and clarified. Even though

the present version of the paper provides observational evidence of an atmospheric interaction between the TIW and the ITCZ in terms of regression maps of SST, CLD, VAP and RAIN anomalies, it does not quantify the strength of this interaction. Is this influence a first order one or only of second importance for the ITCZ? in other words, what are the relative amplitudes of the TIW-induced anomalies in comparison to the full signal? Moreover, does this interaction correspond to a fully coupled process between the TIW and the ITCZ or does it only account for a passive response of the ITCZ at the presence of the TIW?

(iv) About half of the paper consists in a detailed review of the bibliography about the TIWs, in the introduction or in the 'Variability of the Atlantic TIW' section. Despite the high quality of this review, I would suggest to shorten it and to detail the interpretation and the discussion of the results. Besides, I would suggest to remodel, for the sake of clarity, the second half of the paper: the 'Variability of the Atlantic TIW' section should begin at line 4, page 13, while the present 'Variability of the Atlantic TIW' section should be renamed 'Discussion'.

Specific comments —————

page 3, line 9: it is only the SST signature of the TIW variability that can project on the atmosphere.

page 9, line 4: the reference point is chosen to be 1°N and 15°W. Is there any reason to keep it constant when discussing interannual variability, especially in 2000 where no clear maximum in the SST standard deviation can be seen in figure 5.

page 10, lines 11-13: the authors note that the SST anomalies associated with TIW were stronger in 2001 at 2°N and 3°N than previous years at those latitudes. Could it not be related to a more northern location of the SST front (at the northward limit of the cold tongue) during this particular year? Here, as in other locations in the paper, I would have appreciated additional figures to illustrate and discuss the role of the amplitude and the spatial extent of the cold tongue on TIW properties.

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Technical notes —————

page 2, line 11: "demonstrate" should be replaced by "suggest".

page 3, line 20: define ABL.

page 11, line 11: "(Hashizume et al., 2001)" should be replaced by "Hashizume et al. (2001)".

page 14, line 6: you could recall here that this result is true for the Tropical Atlantic Ocean.

page 14, line 11: "have been" should be suppressed.

page 15, line 12: "sowed" should be replaced by "shown".

page 15, line 13: "barcolinic" should be replaced by "baroclinic".

page 17, lines 13: "have" should be replaced by "may have". In the following sentence, one would expect a more detailed description of the influence of the TIW: what kind of influence? Is this influence significant for the ITCZ?

page 17, line 18: "are" should be replaced by "is".

page 27, fig. 5: should "zonally averaged" not be replaced by "temporally averaged"?

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