Dear Editor,

We modified the manuscript considering the comments of the referees. Point by point answers to the comments of the referees are attached to this letter. The authors have no conflict of interests and the two authors have agreed to the content of the manuscript. We approve the manuscript and agree to the submission to the special issue of "Remote Sensing". We also confirm that this manuscript has not been submitted either in full or in part to any journal nor it is in the process of reviewing.

The modified paper and the paper in track changes have been uploaded as a zip file.

The pdf of the modified paper and the paper in track changes has been uploaded as one pdf.

Ali, looking forward to your decision.

Replies to the three reviewer comments are given below:

Replies to referee-1:

The authors thank the referee for the critical comments that improved the quality of the paper. Point-wise answers are given in blue below:

Bulk surface flux calculations rely on relative wind (near surface wind relative to surface ocean current). This study attempts to assess the impact of ocean surface current (if neglected) on wind stress estimation using satellite ASCAT scatterometer wind product, with particular attention to the Indian Ocean. It also tries to estimate the impact of ocean current on wind stress curl and wind work, if neglected. I recommend this manuscript to be rejected. The authors are encouraged to resubmit it after the following concerns are addressed.

Major concern:

Satellite scatterometers measure more surface wind stress (relative wind) than wind, as reported by Kelly et al. (2001) and others. This is stated in the Introduction (Line 41 and various other places) of this manuscript. Backscatters measured by scatterometers are directly related to wind stress and relative wind. Kelly et al. and others claimed this as the advantage of satellite winds over in situ measurements (which often do not measure surface currents). Some of the satellite wind products were tuned to winds from in situ measurements. This adds some confusion. But clearly, Kelly et al. and others did find out that satellite wind products are more representative of the relative winds.

As reported by Kelly et al. (2001), scatterometers measure surface roughness alone (not winds directly) through sigma-0 created by the wind stress which in turn is due to the wind speed and to some extent surface currents as well. The wind stress is determined by the velocity shear across the air-sea interface, not just by the wind, and these are referred to as the relative wind (Seo at al. 2019: doi.org/10.1016/j.dsr2.2019.07.005). Since no equation operationally exists as of today based on physical principles Geophysical Model Functions (GMFs) are developed to empirically estimate wind speed at 10m height from the sigma-0 values assuming a neutrally stable atmosphere (which of course need not be always true) to get the wind vectors from scatterometers and wind magnitudes from altimeters.

Hence, scatterometer measurements of stress are based on a backscatter signal associated with surface capillary waves. Since the GMF is developed between the scatterometer backscatter and the in situ winds (without incorporating currents), this model provides winds alone but not the relative winds.

Thus, all the scatterometer derived products provide wind vectors and these products are widely used in various applications including the ocean and atmospheric models. None of the products are declared as relative winds. If the products were "relative winds" an adjustment to get "winds" from "relative winds" is critically required about which the authors are unaware. In this study, we used the wind products provided by APDRC (http://apdrc.soest.hawaii.edu/datadoc/ascat.php).

Shouldn’t all of these suggest that wind stress estimates using scatterometer winds won’t need tobe referenced to the surface current anymore?

Since the development of a GMF does not considеer surface currents as an input, we estimated stress from the wind speed by two equations (i) without currents and (ii) with currents (equations (1) and (2) in the manuscript).

Specific comments:

1) Line 92-93: Change “ … Somali currents … flows northwards … southward …” to “ … Somali current … flows northeastwards … southwestwards …”

Corrected as suggested. Thanks for your critical observation.

2) Line 115: Any reference for the ASCAT winds?

The reference for ASCAT winds Bentamy and Denis, 2012 is added in the revised manuscript. Thank you for the suggestion.

3) Line 157 and Fig.1: It is hard to believe that ocean currents contribution to the wind stress estimate are all negative in the entire Indian Ocean. When ocean currents happen to be in the opposite direction to the wind, adding ocean currents to the bulk stress calculation would increase the wind stress. It is entirely possible that the currents in the Indian Ocean are all in the direction of surface winds. But please show the mean wind vectors in Fig1a. and ocean currents in Fig.1b.

Stress is estimated using two formulae:

(i) $\tau = \rho_a C_d\, (U_w) * \, (U_w)$ *for without currents*

(ii) $\tau = \rho_a C_d\, (U_w - U_o) * \, (U_w - U_o)$ *for with currents*

Where U_w and U_o are the wind and current speeds respectively.

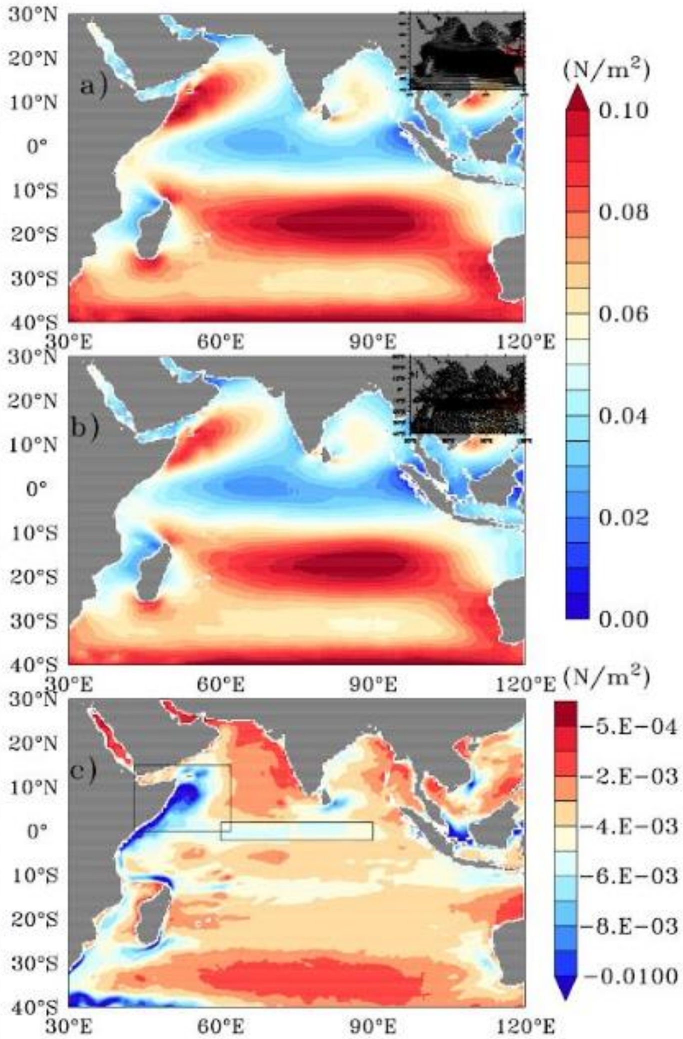
Nowhere in the document, we mentioned that "ocean currents contribution to the wind stress estimate are all negative in the entire Indian Ocean". Probably the referee is confused with the plots in Fig. 1 wherein we plotted "wind stress with currents" minus "wind stress without currents". These values are obviously negative because "$U_w - U_o$" is always less than "U_w" alone as current speeds (which are lower) are subtracted from the wind speed (which are higher).

Whether the currents are in the opposite direction or the same direction as the winds, they contribute to the stress through roughness. However, the contribution of currents to the stress is small, which is generally neglected. We quote the following from the same paper (Kelly et al. 2001) referred by the referee:

"Over most of the ocean, wind speeds are of order ten times larger than current speeds, so this effect is not usually large. Thus, the wind speed inferred from scatterometer data should be lower than the wind speed measured by anemometers when the current is in the same direction as the wind. Conversely, scatterometer wind speed should be higher when the current opposes the wind."

Here, the Kelly et al. used "lower" and "higher". They never mean a negative value. Secondly, by definition, stress is pressure per unit area, which can never be negative.

Adding wind vectors in Fig. 1(a) and 1(b) as suggested is making the figures congested and the vectors are not at all visible as shown below. Hence, we did not modify Fig. 1(a) and 1(b).



However, wind vectors have been given separately below. If the referee wants, we can add these two figures separately in the manuscript. But the authors do not feel the necessity of it.

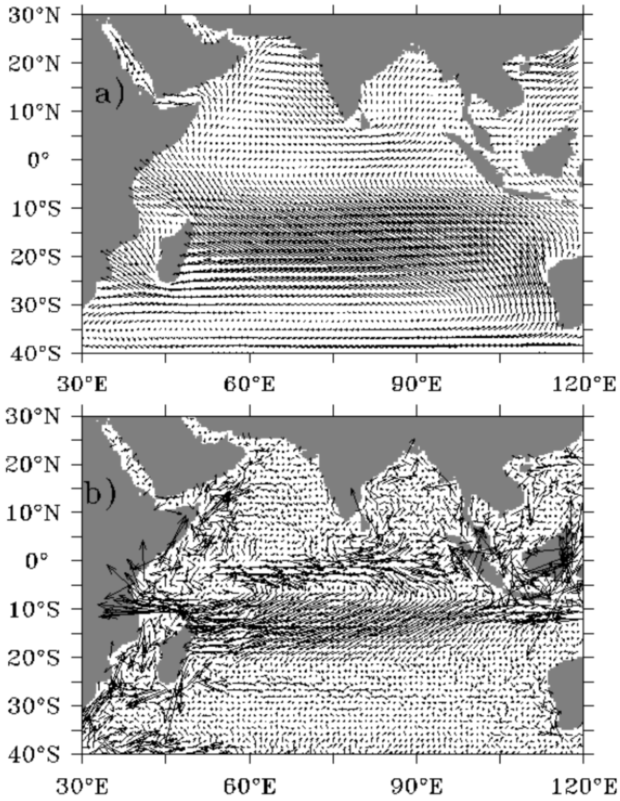


Fig: (a) average wind vectors and (b) average surface current vectors.

4) Line 158-159: Please make sure the following statement is correct: “that stress without currents is more than the stress without current”.

Thank you for your observation. The sentence should have been “that stress without currents is more than the stress with current”.
We corrected this in the revised version.

5) Line 164-166: Change “Since the currents are more in this region, the difference is also more.” to “ Since the currents are STRONGER in this region, the difference is also LARGER.”

Changed as suggested. Thank you.

6) What is value of Cd in equation (2)? How is it determined?

C_D is a dimensionless drag coefficient and its value ranges from $1.1-1.5\times10^{-3}$. In this study, the value used is $1.3X10^{-3}$ following Kraus, 1972. This is mentioned in the revised manuscript. Thanks for the question. It is determined using the following equation.

$$c_d = \frac{2F_d}{\rho u^2 A}$$

C_d	=	*drag coefficient*
F_d	=	*drag force*
ρ	=	*mass density of the fluid*
u	=	*flow speed of the object relative to the fluid*
A	=	*reference area*

However, we used a constant value for C_d as given above.

7) Line 231-233: See Major Concern.

The value of (τ_{Cur} - $\tau_{no\text{-}Cur}$) is always negative because stress with currents is less than stress without currents as given in equation (ii) to answer No. 3.

8) Line 266-268, equation (4): Is this the vertically averaged Ekman current? Please double check the equation? Shouldn't there be a "2" in (2Af)^(1/2)?

This is not vertically averaged Ekman current. This is the speed of the surface Ekman current in terms of the magitude of the wind stress. The Ekman current is computed using the below equation following https://houraad.github.io/MPO503/Lecture%2011.xhtml

$$U_e = \frac{\tau}{\rho(A|f|)^{1/2}}$$

We have double checked and there is no "2" in the equation.

9) What is the value of A used for calculating Ekman velocities?

A, the eddy viscosity is given a value of 10^{-2} m^2/s. This is added in the revised version of the manuscript.

10) Shouldn't the surface Ekman velocity, instead of the vertically averaged Ekman velocity, be used in equation (5) to calculate the wind work? Please justify.

The wind power input computed using equation (5) for surface Ekman velocity as pointed/suggested by the reviewer. The confusion arouse due to the usage of different notations U_e and U_{ekm} for the surface Ekman current in equations (4) and (5) respectively in the earlier version of the manuscript. To avoid confusion," U_e" is now used in both the equations to represent surface Ekman Currents in the revised version. Thank you for pointing this out.

Replies to reviewer-2:

The authors thank the referee for the critical comments that improved the quality of the paper. Point-wise answers are given in blue below:

The authors present the results of a study where they investigate the impact of surface currents on the calculation of wind stress from scatterometer observations in the Indian Ocean.

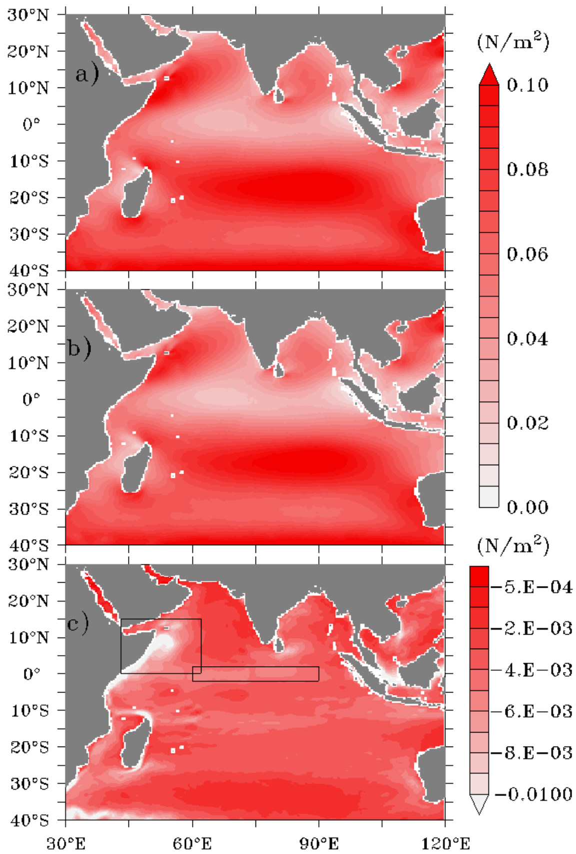
I am generally happy with the paper and I think that it can be published after consideration of the comments below. My suggestions may be useful to further improve the manuscript.

We thank the reviewer for the time taken to carefully read the manuscript and for the thorough review and comments. We have incorporated most of the comments in the edited manuscript and we feel that these changes have remarkedly improved the quality of the manuscript.

First a general remark. I would strongly suggest to change the colour palette in Figures 1, 3, 4, 6 and 7. The current palette goes from a dark colour (purple) via a light colour (yellow) back to a dark colour (red/brown) which is confusing in my opinion and makes it difficult to distinguish between lower and higher values on the maps. I suggest to try a single colour palette, e.g. going from white to dark red or going from white to dark grey.

Thank you for the suggestion. We have tried using single color palettes as suggested by the reviewer. However, we felt that using such a palette, it is hard to distinguish between the subtle variations of wind stress in different regions. A sample plot is given below.

However, we changed colour palette that ranges from blue to red and avoided the dark-light-dark pattern, which is confusing as remarked by the referee. We hope the referee will agree with this change.



Page 1, line 34-36: Please explain the meaning of the brackets '()' and bars '||' in equation 1.
Thank you for the critical observation. We have modified the equation-1 in the revised version of the manuscript.

Page 3, line 113: 'European Organization' should be 'European Organisation for the Exploitation ofMeteorological Satellites (EUMETSAT)' I guess?

Sorry for the mistake. Corrected as suggested.

Page 3, line 114: I suggest to mention the source of the ASCAT data here for clarity: Asia Pacific Data research Centre (APDRC). I have seen that it is named in the acknowledgements at the end of the manuscript but it is better to mention the data provider here as well like is done for the OSCAR currents.

As suggested by the reviewer, the data provider for both ASCAT winds and OSCAR currents are mentioned in the data section and the acknowledgment section as well in the revised manuscript. Thank you for the suggestion.

Page 3, lines 127-130: Here the authors explain how they compute the stresses with and without ocean currents. It is not clear however how they perform the temporal averaging. Are the ASCAT winds averaged over 5 days (the temporal resolution of the currents) before computing stress and are the obtained 5-day stresses then time averaged over a month, season or year? Or are the winds and stresses first averaged separately to a month/season/year and is the stress computed based on time averaged winds and stresses? I assume that both methods may produce significantly different results. Please elaborate on this in the text and justify the chosen averaging strategy.

The temporal averaging is done as described below:

ASCAT winds and OSCAR currents have different temporal and spatial resolutions. The ASCAT surface winds are re-gridded to match the spatial and temporal resolution of OSCAR surface currents before computing the wind stress. For this, the ASCAT winds are first averaged over 5 days and 0.33 degree grids before the stress computation. Once the ASCAT winds are brought to the same resolution as OSCAR currents, wind stress is computed using equations 1 and 2. The stresses computed are then time averaged to monthly, seasonal and annual means for the respective analysis. In general this averaging helps in comparing the results at different temporal resolutions. This also helps if someone wants to compare two days' daily observations or see how the daily or monthly values change temporally with respect to the climatology, such analysis has not been done in this paper though.

Thank you for your critical comment. We have clarified this point in the modified version as well.

Page 5, lines 164-166: please replace ‘more’ by ‘larger’ twice.

We have replaced as suggested.

Page 7, line 221: ‘is always more than’ -> ‘is always larger than’.

Corrected.

Page 7, line 227: ‘the differences are more when’ -> ‘the differences are larger when’.

Corrected as suggested.

Page 11, line 313: ‘is more for lower wind speeds’ -> ‘is larger for lower wind speeds’.

We changed it as suggested.

Please also check the use of ‘less’ throughout the text. In my understanding it should be replacedby ‘smaller’ in some cases.

The term “less” is replaced with “smaller” in the revised version of this manuscript. We have made it consistent throughout. Thank you for the suggestion.

Replies to referee-3:

The authors thank the referee for the critical observations. Point-wise answeres are given in blue below:

This manuscript present the impact of ocean currents on wind stress in the Tropical Indian Ocean. However, there are two main issues which are not appropriate.

One issue is that the OSCAR current is not the current that related to the wind stress. It's derived using satellite sea surface height and a quasi-steady geostrophic model.

We completely agree with the referee that OSCAR currents are not related to the wind stress and in fact, we do not want to use the currents that are related to the wind stress because we want to see the impact of current on the stress estimations.

The OSCAR currents used in this analysis are direct computations of global surface currents using satellite sea surface height, wind and temperature. It represents the total surface current present at any point in the ocean *constituting both geostrophic and Ekman currents which are the major contributors of surface flow. The goal of this work is to assess the impact of surface currents on the bulk wind stress estimate. So OSCAR current is the best representation of the total surface current present at the ocean which in turn can modulate the wind stress computation. More importantly, using such a total surface current field gives a better representation of the wind stress due to currents more accurately. Hence, OSCAR currents are used in this work. We hope that the referee will agree with this explanation.*

The other issue is that the surface current modify the surface roughness which changes the interaction between the wind and the sea surface. The relationship should not be simply consideredas $Uw-Uo$.

As mentioned by the referee, the surface currents modify the surface roughness. Kelly et al. (2001) reported that scatterometers measure surface roughness alone (not winds directly) through sigma-0 created by the wind stress which in turn is due to the wind speed and to some extent surface currents as well. The term $Uw-Uo$ *(equation-1 of Seo at al. 2019: doi.org/10.1016/j.dsr2.2019.07.005) indicates that the wind stress is determined by the velocity shear across the air-sea interface, not just by the wind, and these are referred to as the relative winds. Since no equation based on physical principles operationally exists as of today to get the wind vectors from scatterometers and wind magnitudes from altimeters, Geophysical Model Functions (GMF$_s$) are developed to empirically estimate wind speed at 10m height from the sigma-0 values assuming a neutrally stable atmosphere (which of course need not be always true).*

Hence, scatterometer measurements of stress are based on a backscatter signal associated with surface capillary waves. Since the GMF is developed between the scatterometer backscatter and the in situ winds (without incorporating currents), this model provides winds alone but not the relative winds.

Thus, all the scatterometer-derived products provide wind vectors and these products are widely used in various applications including the ocean and atmospheric models. None of the products are declared as relative winds. If the products were "relative winds" an adjustment to get "winds" from "relative winds" is critically required about which the authors are unaware. In this study, we used the wind products provided by APDRC (http://apdrc.soest.hawaii.edu/datadoc/ascat.php).

The main objective of this study is to investigate what would be the difference between the wind stress estimated without considering the currents and with considering the currents. Hence, we estimated the wind stress (i) without incorporating the currents and (ii) with incorporating the currents. To find the difference between these estimations, we used ($Uw - Uo$) in the equation

$$\tau = \rho_a C_d\, U_w{}^2$$

We simply used $Uw - Uo$ *to see the effect of ocean current in the stress estimation. This similar to using*

$$\tau = \rho_a C_d\, U_w{}^2 - \rho_a C_d\, U_o{}^2$$

I strongly recommend the authors reconsider the method and the surface current data used.

Based on the explanations given above we feel that the referee will agree with using the OSCAR winds and in using the scatterometer derived wind products.