REVIEWER COMMENT

Authors present analysis of differences in bio-optical properties between the low-latitude ocean and the Southern Ocean (SO) using many 1000's of BioARGO float profiles which include multispectral radiometers. Their focus is on relationships between colored dissolved organic matter (CDOM or a_y) and chlorophyll retrievals and show a rough independence between these two properties. From what I can tell, the analysis of deriving a_y spectra seems valid as supported by the sensitivity analyses presented. There is a lack of statistical rigor in the analyses presented and the discussion presented is neither insightful nor useful. I think that the paper could be a fine contribution to the literature once a couple of issues are resolved. These issues are delineated in the text that follows.

AUTHORS RESPONSE

We appreciate the clear assessment by this reviewer, noting the quality of the work yet pointing on some weaknesses. We have accordingly updated the manuscript and some analyses, and we hope these changes answer the concerns raised here. Thanks for having helped us to significantly improve the manuscript.

Because of the profound reorganisation of the manuscript, we do not submit a version in track-change mode, which would have been illegible. We have tried to indicate in the following responses where specific changes can be found in the revised manuscript.

REVIEWER COMMENT

I found section 3.1 very confusing. First, I think the description of figure 2 is incorrect. The text and caption both state that what is plotted is the relative contributions of optical properties to equation 4, including a_y. But, the right-hand side of equation 4 is a_y. So, this does not make any sense. I think what is plotted in figure 2 are the relative contributions that the various optical properties make to K_d.

AUTHORS RESPONSE

About Eq. (4) and Fig. 2 (now Fig. 7 after reorganisation of the results section):
Eq (4) can be reorganised as follows:

$$\frac{K_d(\lambda)\mu_d(\lambda)}{1.0395} = a_y(\lambda) + a_w(\lambda) + a_p(\lambda) + b_{bw}(\lambda) + b_{bp}(\lambda)$$

The left-hand side is close to the total absorption coefficient but not exactly (otherwise the right hand-side would only include components of the absorption budget, not the b_{bw} and b_{bp}). This is from Gordon (1989) (cited) and is based on radiative transfer calculations.
The 5 terms on the right-hand side are those displayed in Fig. 7. The intent is to show how much a_y contributes to this sum (in relative terms).
We have also added another panel to this Figure, which shows the same relative proportions when the a_y, a_p and b_{bp} and calculated as a function of Chl using Morel Gentili (2009), Bricaud et al (1998) and Morel and Maritorena (2001), respectively. We think this helps seeing the difference between what current bio-optical models

predict and what we observe in the data set (acknowledging that a_p is also calculated from Bricaud et al (1998) when we derive a_y from the float data).

REVIEWER COMMENT

Figure 3 is fine, showing invariance of several of the approaches to the retrieved a_y value. However, I do not understand what Table 2 is trying to communicate. Line 230 states that "K_d is the main contributor to differences in a_y" and cites Table 2, but I do not see how the "dispersion of the mean a_y" from the sensitivity analysis shown in Table 2 demonstrates that.

AUTHORS RESPONSE

Yes, sorry, our explanation was not that clear.

To derive the numbers in Table 2 for K_d we calculate the average K_d for each of the histograms corresponding to the three different ways of calculating it (let's call these K1, K2, K3), then we calculate the average of these 3 values (K_m), and then calculate the dispersion (%) as

$$100 \times \frac{\left(\frac{1}{3}\right)(|K1 - K2| + |K1 - K3| + |K2 - K3|)}{K\_m}$$

Same thing for μ_d and a_p.

The legend of Table 2 now reads:

"Average dispersion (%) of the mean a_y values with respect to their average for the three instances of each sensitivity study and the three wavelengths. For each parameter (K_d, μ_d and a_y), the dispersion is calculated as the mean absolute difference among average values for this parameter for each of the three sensitivity studies, divided by the average value calculated for the three studies together."

REVIEWER COMMENT

Section 3.2 shows differences in the K_d vs. Chl relationships from the low-latitude ocean and the Southern Ocean. The analysis presented is completely qualitative and needs to be more quantitative. I suggest that the authors assess whether there are statistically significant differences among the various relationships presented. The colors selected in figure 5 are very difficult to see (especially the yellow and white text).

AUTHORS RESPONSE

Yes, this is indeed quite an oversight.

We tested how our K_{bio} vs Chl relationship for the SO differ from what they are for the low-latitude ocean using a t-test.

They are statistically different for 380 nm and 412 nm but are not at 490 nm. This is actually rather expected considering the larger uncertainties in deriving a_y at 490 nm as compared to the two other wavelengths.

This is now specified in section 3.2 (lines 270-280).

As for the comparison between our K_{bio} vs Chl relationship and the MM01 model, we cannot statistically quantify their similarity because we do not have the MM01 data set that was used to derive their relationship.

At least we can say that, for λ = 380 and 412 nm, our fits and the MM01 and MG09 fits generally both fall within the standard deviation of the average K_d or a_y values (dark blue dots with error bars).

In addition, whether or not they are statistically different, departures are to be expected because both relationships are established with data from different oceanic regions. As such the comparison provides more of a general quality control of the method.

We have changed the colour for the line fits in what are now Figs 3 and 4, hoping that they are now easier to read. We have also opted for density plots to make clearer what the data distribution is (request from another reviewer).

REVIEWER COMMENT

Section 3.3 (Figure 6) gets to differences in the relationships between a_y spectra and Chl concentrations. As in the last section, there is a lack of statistical rigor and the colors selected for the figure needs changing. The authors need to show that the differences commented upon are actually statistically significant. Most importantly, a test of independence between a_y and Chl would be that the derived power-law slope is not statistically different than zero. This result would prove the author's hypothesis in a compelling way.

AUTHORS RESPONSE

The 95% confidence intervals for the regression slopes in Fig. 6 (now Fig. 4) do not include zero and the p-values for the null hypothesis (slope=0) are < 0.001, which means that the slopes are all statistically different from zero (even the small slopes we got for the SO data set).

This is to some extent the reason for using "quasi-independent" in the title.

The following sentence has been added in section 3.3 (paragraph lines 305-310):

"Confidence intervals and a t-test show that all slopes (the B exponent in the A × Chl^B relationships) are statistically different from zero, showing that the dependence of a_y on Chl still exist but is weak for the SO."

REVIEWER COMMENT

As mentioned above, the discussion is very weak and does not get anywhere useful. The existing sections need to be further elaborated upon and expanded new points to be introduced. One suggestion would be to address previous work on why the SO bio-optical properties differ and satellite Chl values are underestimated compared with lower latitudes. Some researchers thought it was unusually low pigment normalized phytoplankton absorption spectra (Mitchell and others) or glacial flour changing particle backscattering (Dierssen) or differences in

photochemical processes. This would help round out section 4.2. Also, you should read through Yamamoto et al. JGR (2024) which does a global analysis of CDOM sources and sinks that would help think about the processes that are creating and destroying CDOM and why the SO might be different.

AUTHORS RESPONSE

Yes, we acknowledge that the discussion was insufficient and sometimes even misleading. It has been fully reorganised in the sub-sections listed below and includes better referencing to previous work and a more in-depth discussion of the potential causes of the differences we observe between the SO and the low-latitude ocean. We also separately address the difference between our observations in the SO and what current bio-optical models predict from Chl (4.2), and the differences we observe between the SO and the low-latitude ocean (4.3 & 4.4).

4.1 Uncertainties of a_y estimates

4.2 Comparison with bio-optical models

4.3 Possible reasons for the different contribution of a_y in the SO as compared to low-latitude waters

4.4 Are departures unique to the SO or do they apply to the whole temperate Southern Hemisphere?

4.5 Do Southern Ocean waters belong to Case 1 waters?

We hope this new version better carries the important messages from this work.

REVIEWER COMMENT

Last, this is not the first time that Southern Ocean CDOM patterns have been addressed (see Ortega-Retuerta et al. 2010, there are likely others).

AUTHORS RESPONSE

The Ortega-Retuerta et al. 2010 paper was already cited in the manuscript. We have nevertheless made our references somewhat more comprehensive, also following request from the other reviewers.
The additional references include:

Fichot C.G., M. Tzortziou and A. Mannino, 2023. Remote sensing of dissolved organic carbon (DOC) stocks, fluxes and transformations along the land-ocean aquatic continuum: advances, challenges, and opportunities, Earth-Science Reviews 242 (2023) 104446, https://doi.org/10.1016/j.earscirev.2023.104446

Ortega-Retuerta, E., Siegel, D.A., Nelson N.B., Duarte C.M., and Reche, I.: Observations of chromophoric dissolved and detrital organic matter distribution using remote sensing in the Southern Ocean: Validation, dynamics and regulation, Journal of Marine Systems, 82, 295-303, https://doi.org/10.1016/j.jmarsys.2010.06.004, 2010b.

Yamamoto, K., DeVries, T., Siegel, D. A., & Nelson, N. B. (2024). Quantifying biogeochemical controls of open ocean CDOM from a global mechanistic model. Journal of Geophysical Research: Oceans, 129, e2023JC020691. https://doi.org/10.1029/2023JC020691