Responses to Emmanuel Boss, UMaine.

Reviewer comment: This paper investigates the relationship between CDOM (the absorbing part of DOM) and chlorophyll using BGC-Argo data. A simple bio-optical model to decompose the contributions to the diffuse attenuation of the measured irradiance is used with additional inputs from the chlorophyll fluorometer and backscattering sensors on the float. For each wavelength a CDOM estimate is computed and compared to chlorophyll to investigate how they relate both in the SO as well as further north. The subject of the paper is very interesting and pertinent as there is currently a debate regarding the skill of remote sensing retrievals in the SO. Understanding why they may not work, such as if assumption in them are wrong, would help advance the science we can do from remote sensing in the SO by removing biases.
Authors response: thank you for this positive assessment and for many useful comments that helped us improving this manuscript substantially. Our responses are below and also in the annotated PDF file that was returned with the review. 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: The paper is relatively clearly written though the notation/formatting could be significantly improved to avoid confusion and make it easier to read (for example CDOM, the subject of the paper, is written as a_y, *a*y and *a*_y, see attached pdf). Such sloppiness in the notation of the subject of the paper does not reflect well on the author (two of which I know well and are excellent and careful scientists).
Authors response: you are right, notation and formatting got mixed up at some point in the multiple draft iterations and this escaped our attention when submitting the final manuscript. This is fixed now.

I have some significant comments that I feel, if addressed, will significantly improve this manuscript. I am also returning an annotated PDF.

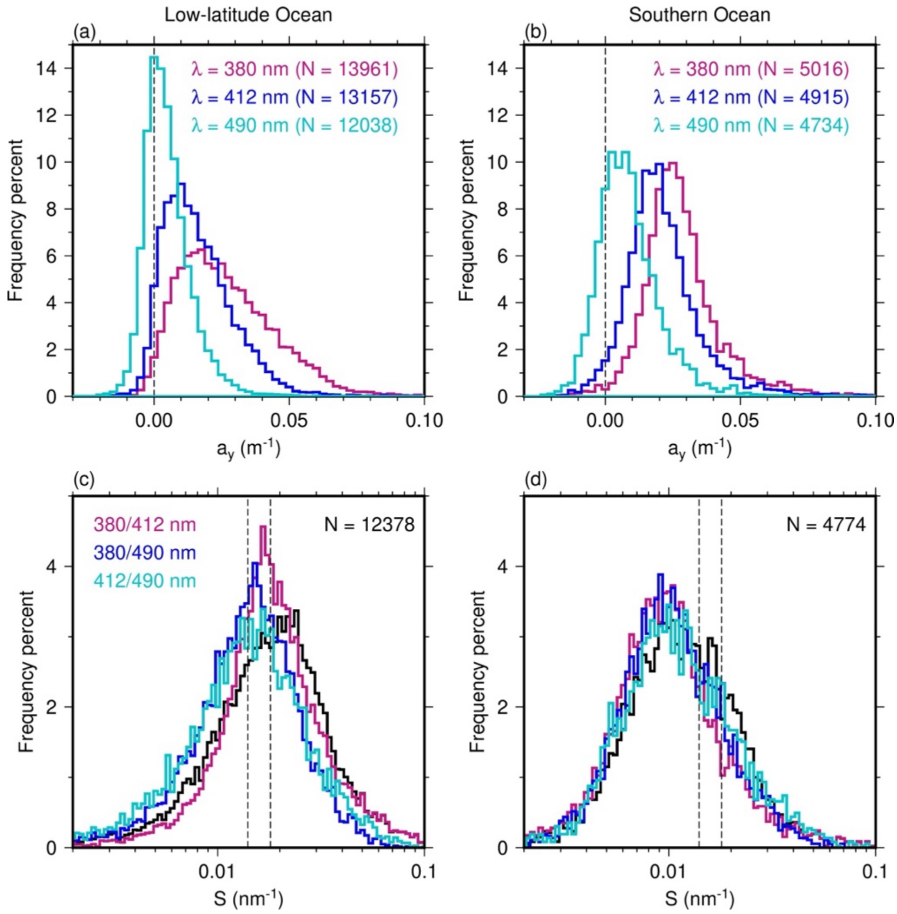
1. **Reviewer comment:** I feel like there is no real closure in this paper. This could be done, for example by showing the degree of agreement between the different wavelengths when computing an a_y spectra. Since each radiometer is an independent sensor, it provides you an opportunity to test the quality of your retrieval for each profile. You only provide us the one estimate of the

spectral slope of a_y for each region. It would be very informative to see it for each profile. It could be used by you as an independent quality check as CDOM slopes are well constraint in the literature. You could, for example, provide a histogram of a_y(380)/a_y(412), a_y(380)/a_y(490), and a_y(412)/a_y(490) (or their reciprocal), and compare to what is expected. You are making so many assumption (e.g. a_p to chl relationship, chl to Fchl relationship etc') that it is hard to evaluate if your uncertainties are indeed as small as you think they are. This closure exercise will help determine it.

Authors response: let us first nuance that we do not claim that our uncertainties are low. The Monte Carlo analyses give average uncertainties around 20-25%.

As for the spectral slope, we are not sure what you mean by "to see it for each profile." Isn't that what the histogram in Fig. 7 shows? (that is now Fig. 2).

To answer the other part of your question, we have modified that Figure by adding the 3 individual calculations of the slope in addition to their average (see below; light blue for 412/490, blue for 380/490, and purple for 380/412).



2. **Reviewer comment:** Alternatively, you could use kd(490) and Fchl to constrain better chlorophyll (using a Xing like method) and then see if a_y(380) and a_y(412) are better related.

Authors response: we could use $K_d(490)$, indeed, yet we think there is a risk of some circularity here because the Chl vs. $K_d(490)$ relationships that we could use (like the Morel and Maritorena, 2001, for instance) would assume a certain share of absorption in the blue between CDOM and phytoplankton based on global, low-latitude data. This work tends to show that this partition between CDOM and phytoplankton absorption is not valid in the SO. This could lead to large Chl over- or underestimates.

That being said, we have tested the impact on our analysis of using a recently published map of the fluorescence-to-Chl ratio (Sauzede et al., 2025; link below), instead of using the Schallenberg 3.79 value in the SO and a value of 2 elsewhere. This map (lookup table) is based on using K_d to derive Chl from floats and shows values of the F/Chl from as low as 1 to nearly 10.

Ref: https://www.seanoe.org/data/00945/105732/

The results are slightly modified yet do not change the conclusions we draw here. Therefore, we decided not to adopt this variable F-to-Chl ratio from Sauzede, however, because of the risk of "circularity" mentioned above.

3. **Reviewer comment:** The adjustment done to the chlorophyll and bbp is not well justified. It is hand waved. In addition, at low chl, Fchl could be significantly affected by CDOM (see another Xing paper) biasing your adjustment.

Authors response: there are two things here:

1- The restoration of the deep values to the mode of their distribution calculated from all profiles. This is commonly done and is a way to remove residual noise. These adjustments are not hand-waved. They are based on the data. For Chl, this mode is 0.0055 mg m^{-3} for both the SO and low-latitude data sets, which is very low. It is 3 10^{-4} m^{-1} for b_{bp}. This is a minute yet useful correction to remove some noise in the data set.
The possible impact of CDOM on FChl would rather happen at depth, and we only use surface data in this work, so we do not think it is an issue.

2- What is definitely somewhat hand-waved is the further adjustment we introduced for Chl (not for b_{bp}), after seeing that we had many surface Chl values < 0.02 mg m^{-3} (actually even lower than 0.01 mg m^{-3}), which are unrealistic. As you know 0.02 mg m^{-3} is about the minimum surface Chl concentration ever measured in the open ocean (e.g., SE Pacific gyre or eastern Mediterranean Sea). This correction was however only applied to the low-latitude ocean data set (was admittedly

unclear as written). We have also checked that this was not an artifact of the fitting procedure we apply to the raw profiles.
Therefore, although we would prefer not having to do so, we decided to keep this adjustment in order for the data distribution to make sense with respect to what is known about the global Chl distribution at the surface of the oceans.

4. **Reviewer comment:** Table 1 – not clear if you assume no uncertainties at 412 and 490 when they are not provided.

Authors response: indeed, the Table did not make clear enough that we use a single uncertainty whatever the wavelength for K_d, μ_d and b_{bp}. We have modified the Table layout so that it is explicit now.

5. **Reviewer comment:** You mix Discussion (or interpretation) into your Result while you provide two separate sections. I suggest, in particular because of the high uncertainties of your assumptions, that you clearly separate the two. Have the Result section be an objective description of what you found and use only the Discussion for speculation about the cause for your observation.

Authors response: indeed, there is a bit of a mix here. The entire Results section has been reorganised, on the one hand to avoid including discussion statements and, on the other hand, to improve the logical flaw, from "raw" results (now Figs 2 and 3), then relationships to Chl (4 and 5), seasonal patterns (Fig. 6), and then analysis of the components of Eq. (4) and sensitivity analyses (Figs. 7, 8 and 9).

Reviewer comment, continued: Also, you may want to add a Summary section where you summarize the most important implication of what you found and suggest future work.

Authors response: with the major changes of the discussion section, we felt that a summary section was not really necessary.

6. **Reviewer comment:** It is not clear what Fig. 2 represent and how it is linked to Eq. 4. I suspect it is the relative contribution of different terms to (a+bb), but this is not what is written.

Authors response: yes, that is what it is. We have modified the writing and hope it makes it clearer now. It is probably clearer when the equation is 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 what is now Fig. 7.

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).

7. **Reviewer comment:** The discussion relating Fig. 8 to DOC in the upper ocean is **wrong**. There is actually more DOC in low latitude compared to high latitudes (see Hansel, 2009, Oceanography). And, generally, CDOM and DOC do not correlate in the open ocean.

Authors response: we have removed this statement, which was anyway more of a discussion nature than belonging to the Results section. Instead, the sentence is now: "This is consistent with the global a_y distribution derived from reflectance ratios by Morel and Gentili, (2009) (orange dots), except south of about 40°S where the values we derive here are lower."

8. **Reviewer comment:** The discussion regarding case 1 and case 2 could be more nuanced. Bricaud and Morel have a paper from 1981 showing decorrelation between chl and a_y. When I asked Morel he told me that locally one may see a lack of correlation but when one uses a large dynamic range from many contrasting oceanic environment, a general relationship may emerge. When looking at your data I see a huge spread around your best fit lines, with the lines being a poor representation of most of the data (e.g. if I had to predict a_y from [chl], even not in the SO, the likely error could be very large, to the point of being useless for many applications. Maybe if you used a heat map for your regression plot (with color being the concentration of datapoint of a given value one could

see better how the relationship derived relate to the statistical distribution of the points and appreciate that the fits are much better than I claim here).
Authors response: as for the Figure style, we have modified what are now Figs 3 and 4 using density plots instead of the dots. About the Case I/II discussion: considering a large dynamic range is indeed what makes this concept practically work and is why it is so powerful in deriving global satellite ocean colour products. This was not made clear in this section, and it has now been added. In addition, this section has been significantly rewritten.

Dear authors: I am often wrong. If you feel that my comments are off the mark, please contact me and I would be happy to change them if convinced.

Responses to Simon Belanger

General Comments:

Reviewer comment: This manuscript addresses the absorption of light by colored dissolved organic matter (CDOM, a_y) in Southern Ocean (SO) waters, a region characterized by strong turbulence, deep surface mixed layer and challenging sampling conditions. The topic is interesting and relevant, as it highlights a distinctive feature of the SO. By analyzing ARGO float data, the authors convincingly show that CDOM concentrations are significantly lower for low chlorophyll-a (Chla) concentrations in the SO compared to other oceanic regions. Below a Chla concentration of 0.1 mg m^{-3}, the correlation between a_y and Chla becomes very weak. This finding may appear counterintuitive given that photobleaching processes are relatively inefficient in this region, where light levels are low and the mixed layer is very deep. I appreciate the efforts of the authors relative to the method developed to estimate CDOM absorption from ARGO floats and the accuracy assessment they provided. This is a strong point. The authors correctly point out the implications of this feature for the use of global empirical bio-optical relationships applied to satellite ocean color data, which could bias Chla estimates in the SO. The manuscript is generally well written, although there are numerous errors in the notation of symbols such as a_y and the mean cosine (μ_d), particularly in Section 2.
Authors response: We appreciate that the reviewer found this work interesting and the use of the BGC-Argo data relevant. Thank you for comments that helped us to improve the manuscript significantly. 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. About the comment that results "may appear counterintuitive given that photobleaching processes are relatively inefficient in this region": we realise that our wording led to some misunderstanding. What we show (Fig. 2, now Fig. 7) is actually that there is more (not less) CDOM absorption per unit chlorophyll (Chl) in the Southern Ocean than for the low-latitude ocean when Chl < ~0.15 mg m^{-3}, and indeed rather less for Chl above this threshold. To clarify the matter, we have split the discussion between:

1- the anomalies w.r.t. current models of the K_{bio} vs Chl and a_y vs Chl (MM2001 and MG2009 in our case). This is where the implication for ocean colour interpretation takes place, and

2- the differences between average K_{bio} and a_y vs Chl for the low-latitude and SO, where reasons for such differences are discussed.

We have also updated the abstract.

As for the notation and formatting: sorry, this got mixed up at some point in the multiple draft iterations and this escaped our attention when submitting the final manuscript. This is fixed now.

Specific Comments

1. **Reviewer comment**: Introduction (lines 38–44): The introduction provides extensive details on OCRS algorithms for CDOM estimation, citing several approaches. At this stage, I expected the manuscript to focus on algorithm development, which is not the case. These details seem unnecessary if the main objective is not algorithm design. The key point is simply that if the SO has distinct CDOM levels, applying empirical algorithms developed for the global ocean is risky. Later (lines 51–53), the manuscript mentions the a_NAP versus b_bp relationship, which is not well known in the SO, but this is not directly related to CDOM and appears off-topic.

   Moreover, given the inefficiency of photobleaching processes in the SO, one might expect CDOM accumulation in the absence of effective degradation mechanisms. In other words, the introduction is overly focused on OCRS algorithms and does not sufficiently emphasize the unique characteristics of the SO that could explain anomalies in the CDOM–Chla relationship ,which is the main topic of the current paper. I recommend citing relevant studies (such as Reynolds et al 2001; Siegel et al. JGR, 2005 – *Independence and interdependencies among global ocean color properties: Reassessing the bio-optical assumption* (see their Figure 7)), in the introduction that points toward different CDOM background in the SO relative to the global ocean.

Authors response: this is indeed somewhat out of scope. The introduction has been accordingly significantly rewritten. We hope it provides a better rationale for this work.

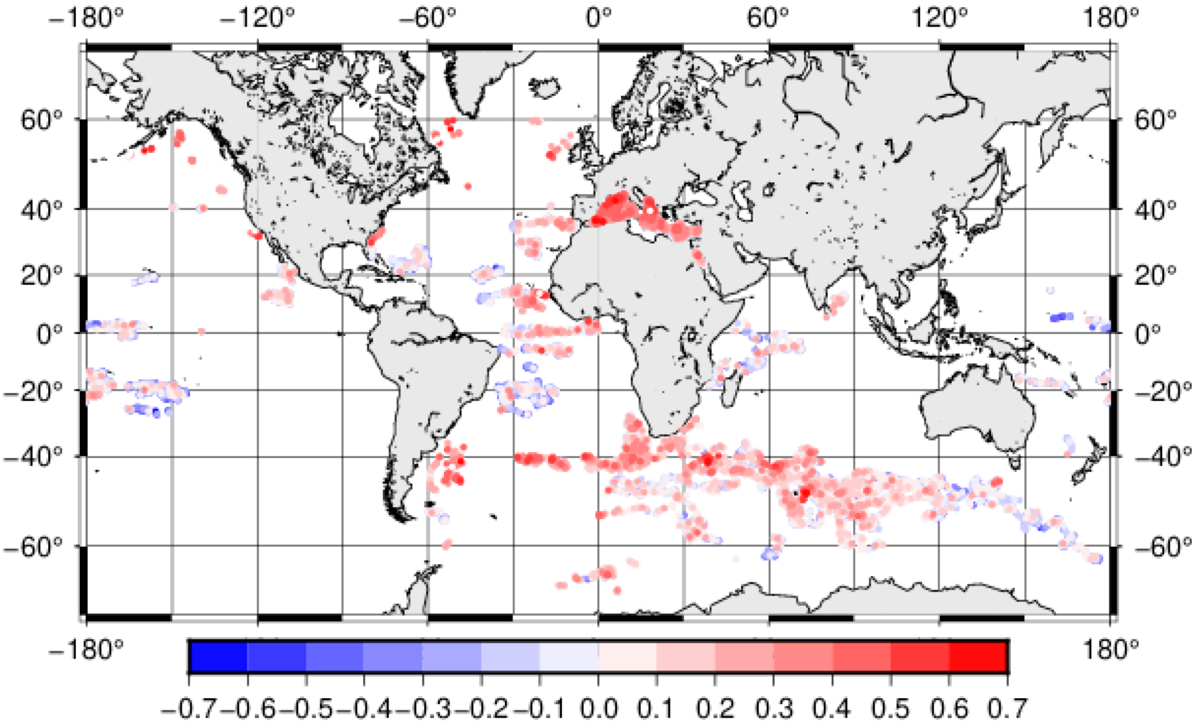
As for photobleaching: during summer in polar areas, the total shortwave radiation, hence the photosynthetically available radiation, reaches values as high as in equatorial areas. Significant photobleaching can then be expected during this season. It is clearly negligible during the polar winter.

This point is also addressed below.

2. **Reviewer comment**: Spatial Analysis: the manuscript would benefit from a more detailed spatial analysis of anomalies in the CDOM–Chla relationship. While a global relationship exists (Fig. 6), it is relatively weak, with substantial variability in a_y for a given Chla. Could the authors map anomalies (i.e., CDOM predicted by the global relationship versus CDOM observed by floats) to identify coherent spatial patterns? For example, regions with positive anomalies (+ CDOM per unit Chla) and others with negative anomalies (– CDOM per unit Chla, such as the SO). Figure 8 is a good starting point, but further spatial analysis (i.e. CDOM-anomaly maps) would provide valuable insights into CDOM dynamics.

Authors response: we had a look into this, yet no clear pattern emerges, with positive and negative anomalies being quite evenly (randomly) distributed (see map below). Although we used a large number of float profiles, this is still insufficient to assess regional patterns.

However, we did perform an analysis of the seasonal patterns by calculating monthly average of the anomalies and of the spectral slopes and assessing their relationships with the mixed-layer depth. Results are displayed in what is now Fig. 6 and discussed in section 4.3. They tend to show that photobleaching is significant in summer and is one reason for the negative anomalies we observe.



What is represented here is the decimal log of the ratio between individual ay values and the value predicted by the relationships we derived (of the form: A + BxChl^E). Note that this representation can be misleading because points with positive or negative anomalies can be superimposed one of top of the others. Plotting separately the negative and positive anomalies on two maps does not reveal more spatial coherence, however

3. **Reviewer comment**: Discussion (Section 4.2): The discussion lacks depth regarding the interpretation of CDOM dynamics. The authors should explore potential mechanisms explaining the weak CDOM–Chla relationship and the low CDOM background in the SO compared to the global ocean. The absence of any reference to photobleaching processes, which play a key role in CDOM degradation at global scale, is a major gap. Previous studies (e.g., Fichot et al., 2023, *Earth-Science Reviews,* Fig. 14; Zhu PhD thesis, 2023; figures 3.5 and 3.6) have quantified photobleaching rates. Given that photobleaching is inefficient in the SO, microbial degradation likely plays a dominant CDOM removal role. In addition, the thickness of the mixed layer may also be critical. As Reynolds et al. (2001) noted: *"The deep winter mixing is likely to limit the accumulation of a long-lived CDOM pool in the SO."* Long residence time of CDOM in the MLD likely favor long-term degradation of CDOM by microbes. Finaly, absence of terrigenous CDOM in the SO should also be clearly mentioned.
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 $\boldsymbol{a_y}$ estimates

4.2 Comparison with bio-optical models

4.3 Possible reasons for the different contribution of $\boldsymbol{a_y}$ 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.

More specifically:

What Fig. 4 (former Fig 6) shows is that we do not observe in the SO the low a_y values for Chl < ~0.2/0.3 mg m^{-3} that the low-latitude data show. This is what drives the flattening of the slope of the a_y vs Chl relationship. Therefore, here we rather show an excess of CDOM as compared to what is observed in the other oceans when Chl is low.

We also note that Fig. 14 in Fichot et al. 2024 is for the mid of winter, where of course photobleaching cannot be significant because incoming irradiance is very low. We discuss photobleaching in the second part of section 4.3, where we also emphasize the critical role of mixed layer depth.

We also make clearer at the beginning of 4.3 that this study only considers locally- (biologically-) produced CDOM, with terrigenous contributions being likely minor. Inputs of CDOM through sea ice melting cannot be ruled out in the SO, however (Ortega-Retuerta, 2010a,b).

References used here:

Ortega-Retuerta, E., Reche, I., Pulido-Villena, E., Agustí, S., and Duarte, C. M.: Distribution and photoreactivity of chromophoric dissolved organic matter in the Antarctic Peninsula (Southern Ocean), Marine Chemistry, 118, 129–139, https://doi.org/10.1016/j.marchem.2009.11.008, 2010a.

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.

I strongly encourage the authors to expand Section 4.2 to address these points.

I attached the PDF with a few additional comments.

Responses to the anonymous reviewer

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 $\boldsymbol{a_y}$ estimates

4.2 Comparison with bio-optical models

4.3 Possible reasons for the different contribution of $\boldsymbol{a_y}$ 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