

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 (Chl<sub>a</sub>) concentrations in the SO compared to other oceanic regions. Below a Chl<sub>a</sub> concentration of  $0.1 \text{ mg m}^{-3}$ , the correlation between  $a_y$  and Chl<sub>a</sub> 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 Chl<sub>a</sub> 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 ( $\mu_d$ ), 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  $\text{Chl} < \sim 0.15 \text{ mg m}^{-3}$  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  $K_{\text{bio}}$  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 OCS 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 OCS algorithms and does not sufficiently emphasize the unique characteristics of the SO that could explain anomalies in the CDOM–Chl<sub>a</sub> 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.

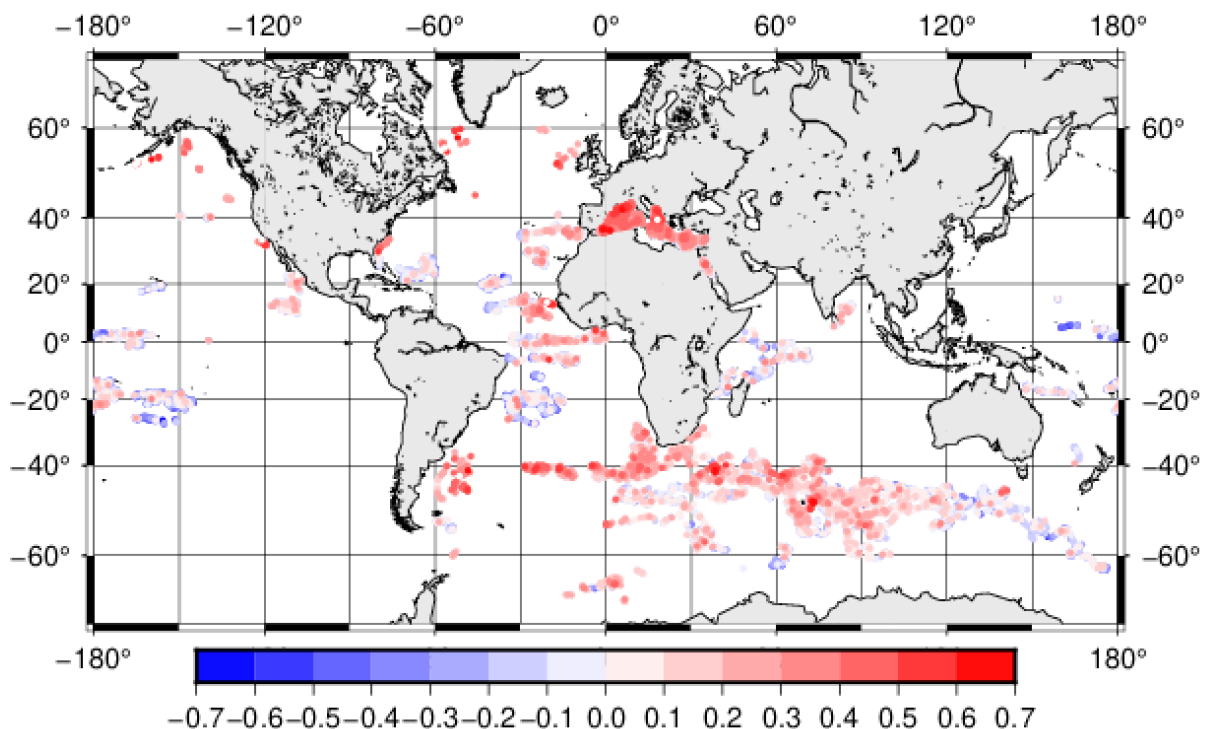
As for photobleaching: during summer in polar areas, the total shortwave radiation, hence the photosynthetically available radiation, reaches values as high as in equatorial areas. Significant photobleaching can then be expected during this season. It is clearly negligible during the polar winter.

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  $a_y$  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. Finally, 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  $a_y$  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.

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 <  $\sim 0.2/0.3 \text{ mg m}^{-3}$  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.