Jacquet et al. present new data of Ba_{xs} concentrations, O_2 consumption rates from direct measurements and prokaryotic heterotrophic productions (PHP) from the ANTARES station in the Mediterranean Sea. The aim of this research is to investigate the connections between these three parameters (Ba_{xs} concentrations, O_2 consumption rates and PHP) in order to validate the Ba_{xs}-O_2 consumption transfer function first proposed by Dehairs et al. (1997) in the Southern Ocean. The authors found higher Ba_{xs} concentration associated to deeper PHP and to greater O_2 consumption rate. Finally, they found a relatively good agreement between O_2 consumption rates estimated by the Ba_{xs} transfer function from the Southern Ocean (Dehairs et al., 1997) and by direct measurements, confirming the use of this transfer function in the Mediterranean Sea.
Previous studies used Ba_{xs} as a tracer of O_2 consumption and thus as a tracer of POC remineralisation, but they either assumed the universality of the Southern Ocean transfer function (e.g. Cardinal et al., 2005) or proposed new transfer function without direct O_2 consumption measurements (e.g. Lemaitre et al., 2018). It is therefore of interest to investigate the conformity of this transfer function by directly measuring O_2 consumption rates and PHP. For that reason, the findings of this study are highly valuable for the community.
However, the authors report data from only one station (only one data added in both the PHP/Ba_{xs} and JO_2-Ba_{xs}/JO_2-opt correlations) which is weak to support their conclusions. Statistical analyses (p-values, errors on the slopes, etc) are needed. Also, a direct comparison of the Ba_{xs}/JO_2-opt correlation from this study (where the authors show 4 data points; Fig. 3a) with the one proposed by Dehairs et al. (1997) in the Southern Ocean would be very useful and more convincing, to me. Many details are also missing in the methods to really understand how Ba_{xs} concentrations, O_2 consumption rates and PHP were measured. Moreover, I would appreciate if there was a discussion about the variations found between ANTARES, PAP and DYFAMED stations, about the differences observed between the Southern Ocean and Mediterranean Sea correlations (Baxs background for example) and about the implications of this study in the water column C budget of the Mediterranean Sea. Finally, all the data (Ba_{xs} concentrations, O_2 consumption rates and PHP) should be presented in a Table.
Overall, the manuscript is well written and will be a good fit for publication in Biogeosciences, but considering the lack of details and comparisons, considering the relatively large error bar associated to the JO2-opt, and considering that this study adds only one data point to the JO2 correlation, I would suggest the authors to soften their conclusion on the 'universal validity' of the Dehairs's transfer function.

A. Specific comments

1-Introduction

In general, this section should be developed and should mention all the studies on Baxs concentrations as a tracer of POC remineralisation but also all the recent studies investigating barite formation and the role of barite on the Ba cycle.

Lines 55-61: Please develop the paragraph about the use of Baxs as a geochemical proxy: more studies have worked on Baxs in the past (e.g. Bishop, 1988; Collier and Edmond, 1984; Ganeshram et al., 2003; Gonzalez-Munos et al., 2003). Moreover, there are some recent studies that should be mentioned/discussed about lab experiments and Ba isotopes giving extremely interesting new insights on the formation of barites and on their role in the Ba cycle in the ocean. Please, see for example the studies of Martinez-Ruiz et al. (2018, 2019), Horner et al. (2015), Cao et al. (2020), Hsieh et al. (2017).

Lines 66-68: Lemaitre et al. (2018) do not use the Southern Ocean transfer function proposed by Dehairs et al. (1997). These authors proposed a new function specific to the North Atlantic. Consequently, they (sort of) 'revised' the validity of this transfer function at least for the GEOVIDE study area. You could also use this study as an additional reason to check the validity of the Southern Ocean transfer function in the Mediterranean Sea.

Line 67: Instead of Lemaitre et al. (2018), you could also cite Cardinal et al. (2005), Dehairs et al. (2008), Jacquet et al. (2008 a, b, 2011, 2015), Planchon et al. (2013).

2-Sampling and Analyses

This section must be developed. The reader needs more information and details on how and how well you measure Baxs, O_2 consumption rates from optodes and prokaryotic heterotrophic productions. Also, please show all your new data in Figures and Tables.

Line 115: Please show the full Baxs, pAl and bio Ba depth profile, i.e. from surface to 2000m, in Fig.2a. This will also confirm that the Baxs background stays at around 130pM at depths > 500m.

Lines 115-116: If I am correct, the samples used for the data presented in Fig. 2a have not been collected on the same day or exact location. Please prove that there was no evolution of water mass or biology between each sampling. If there was any change, could this influence your Baxs or pAl concentrations?

Lines 120-121: Please give the precision and accuracy of your analyses.

Lines 124-125: 'sea-salt particulate Ba contribution was found negligible'. What is negligible? Please give numbers.

Line 125: Give more details on the Ba/Al ratio you are using to correct the lithogenic fraction. I suppose it is from the UCC but how does this value compare to the lithogenic inputs at ANTARES? This station is relatively close to the coast and is likely subject to lithogenic inputs, it is therefore important to be sure about the Ba/Al ratio used to correct the lithogenic fraction. Without that, your estimation of Baxs concentrations may not be correct. For comparison, Lemaitre et al. (2018) do not take into account data from two stations where the pBa-litho accounts for 28 and 44% of total Ba. At ANTARES, the Ba biogenic fraction range from 50 to 80%, meaning the lithogenic fraction is not negligible.

Line 126: How did you determine the standard uncertainty? From the RSD given by the Element for Ba? From error propagation, taking into account the RSD of Ba and Al?

Lines 131-136: There is no reference at all in this paragraph – it is thus difficult to understand the technic for someone who is not familiar with this. Please explain, at least briefly, how you measure O2 concentrations with this technic and how you calculate the O2 consumption rates – an equation might help? Can you prove the precision/accuracy of this method? I suppose you need relatively precise measurements to determine an O2 consumption rate. However the errors associated to this measurement and to the final calculation seem high (Fig. 3), why?

Lines 137-142: Same here, please give more details on the protocol you use for determining the PHP. Why do you use 3H-leucine? How do you then calculate the PHP (equation)?

3-Results and Discussion

The authors should give more details to convince the reader about the validity of this Baxs-JO2 function in the MedSea. A direct comparison of the slope of the transfer function you obtain here (Fig. 3a) with the one from the Southern Ocean would be helpful. Some statistics would also help. Moreover, I think this section would get more interesting if there was some comparison with the literature and some explanations on why some of your results slightly differ compared to those of other study areas (essentially, more explanation on the story of the MedSea data – not only about the use of Baxs in this area to trace O2 consumption). The figures could be clearer as well.

Line 149: 'pAl concentrations are low…' 170nM is not low! On the contrary, it clearly shows a lithogenic input and this makes your Baxs estimations doubtful as the lithogenic correction may not be perfectly constrained. How much is the pBa lithogenic fraction in the depth layer that is interesting for this study (i.e. 100-500m)? Can it be considered as negligible? If yes, why? Please see my previous comment about the Ba/Al ratio and discuss more about the lithogenic correction at ANTARES station.

Line 156: You mention the pBa biogenic fraction in the interested depth layer is >80% but is it high enough to be assured of a good Baxs estimation? What is the error associated to this correction (this could go to the methods section)?

Lines 157-160: How do you explain the difference between the Baxs background observed in the Southern Ocean and in your study? For example, Lemaitre et al. (2018) also observed a Baxs background at 180pM in the North Atlantic. What is different in the MedSea?

Lines 165-166: How do you explain the difference of adsorption between ANTARES and DYFAMED stations? Is it related to different bloom timing or intensity?

Lines 168-169: Please show the full depth profile, i.e. from the surface to 2000m, in Fig. 2a. That would be useful to clearly see the background level.

Lines 169-170: At DYFAMED station, Baxs concentrations seem to keep decreasing for depths >600m, why is it not stabilised at 130pM?

Lines 180-183: Please, discuss the result of the PAP station if you present it. It is below the trend, why? Moreover, what is the p-value of this trend? Is it a significant correlation with and without the new ANTARES and PAP data? Is it possible to add data from DYFAMED?

Line 184: Are these PHP profiles similar to the one at ANTARES station? Could you plot them all in a figure and add the ANTARES data in a table?

Lines 185-189: 'Indeed, mesopelagic Baxs…' These lines repeat your sentence lines 181-183 '..indicating higher DWA Baxs in situations where a significant part..'. Please re organise this section to avoid repeating things.

Lines 189-190: 'Our MedSea resultS are located..'. You provide only one new result from ANTARES station, please change the plural to singular form in this sentence. Also, this sentence repeats what you say lines 180-181. Maybe you should delete it.

Lines 190-195: Please develop this section according to the new literature (e.g., Martinez-Ruiz et al., Horner et al., Cao et al., Hsieh et al..) and find a transition with your previous sentence.

Line 200, Fig. 3a and b: It seems that there is a mistake with the units. They do not correspond to those in Jacquet et al. (2015), would it not be mmol/m2/d instead? If I am correct, please change all your JO2 data in umol/L/d and compare the slope you obtain in Fig. 3a with the one from the Dehairs et al. (1997).

Lines 201-203: I agree this is a very interesting feature confirming your background Baxs concentration! Could this result give an insight on why there is a different Baxs background in the MedSea compared to other areas?

Line 209: Why do you use a factor of 17450 here while it is 17200 in Jacquet et al. (2008) or Lemaitre et al. (2018)?

Lines 214-219: There is a large error bar associated to your ANTARES data point for JO2-opt (Fig. 3b), why? I agree that considering this large error bar, your data fits the trend observed during KEOPS. However, this large error bar and the poor distributions of the data points (either low JO2 for KEOPS or high JO2 for ANTARES) make this correlation too weak to state that there is no difference between both regions. What is the p-value of this correlation with and without ANTARES? Is it possible to add data from PAP or DYFAMED stations? I would be more convinced by a comparison of your Baxs-JO2 trend with the one of the Southern Ocean. For now, the slope in Fig. 3a is very different from the one of the Southern Ocean (100 versus 17450). After fixing the unit problem, please discuss about this comparison.

Line 226: Please indicate what is Z in this study.

Lines 228-229: Please give the range of the fluxes from the literature and discuss them according to the one you estimate at ANTARES.

Lines 239-241: Expand a bit the discussion here. How does your study contribute to the MedSea carbon budget? Does it help balancing the water column budget?

B. Line notes

Abstract:

Lines 25-27: These are not new observations/conclusions. Please make it clear here that you are confirming what has been observed earlier in another area (Jacquet et al., 2015).

Line 25: 'higher Baxs (409 pM; 100- 500 m) [occurs] in situations where integrated PHP (PHP100/500= 0.90) is located deeper'

Line 26: ‘higher Baxs [occurs] with increasing JO2-Opt’

Introduction:
Line 63: ‘highly resolved, precise..’ seems a bit exaggerated as a sampling resolution of 50m depth is good but not high for me and I suppose the technics may be more precise today compared to 1997.

Line 70: I would delete this sentence as it repeats the sentence line 68 (‘Yet its validity has never been tested..’) and it separates two linked sentences.

Line 70: ‘These advancements..’ refer to the results of Jacquet et al. (2015) I suppose? Please make it clearer.

Line 83: Which fluxes are you referring here? Primary production, export, remineralistion?

Line 83: Please give a range of the fluxes determined by Santinelli et al (2010) and Ramondec et al. (2016).

Methods:
Line 111: I would name this whole section ‘Methods’ and would name the sub-section 2.2 ‘Sampling and Analyses’

Line 107: ‘and [(3)] Levantine Intermediate Water…’

Line 117: ‘total digestion of filters using a [concentrated] tri-acid mixture..’

Line 130: ‘The background (or residual value) is considered as “preformed” Baxs at zero oxygen consumption left over after transfer and partial dissolution of Baxs produced during degradation of previous phytoplankton growth events. [The background is set at 130pM in this study].’

Results and Discussion:
Line 145: Maybe modify to ‘[Particulate Baxs] vertical disctribution’ to avoid any confusion for the reader.

Line 160: ‘For comparison, the [Baxs] background value…’

Line 173: ‘the particulate excess Ba (>BKG)’ is confusing for me. You never expressed Baxs like this before. Please keep the same wording all along the manuscript, maybe modify to ‘The maxima Baxs concentrations are centred..’

Line 174: ‘in this depth layer’ instead of ‘at these depths’

Lines 174-175: Explain what is the depth-weighted average, as you did for example in Jacquet et al. (2015): ‘i.e. the Baxs inventory divided by the depth layer considered Z’.

Line 175: ‘over the 100-500m depth layer’ instead of ‘this entire depth layer’. It will avoid any confusion with Fig.2b and all the different depth integrations.

Line 176: ‘Figure 2b shows [the] column-integrated PHP at 100m over the [one] at 500m (PHP100/500). Our PHP100/500 ratio at ANTARES station is of 0.90 and is compared to results obtained during KEOPS1…’

Line 180: ‘ResultS at the ANTARES..’ Are there more than one result? On Fig.2b, there is only one data from ANTARES station.
Line 181: ‘…follow the trend previously reported in the Southern Ocean [(blue dashed line in Fig.2b; Jacquet et al., 2015)]..’

Lines 181-182: Please make it clear that the ANTARES data confirms the conclusions found in Jacquet et al. (2015) and that it is not a new conclusion.

Line 204: ‘[In Figure 3b,] we applied..’

Line 217: ‘Overall, our results indicate [a] similar Baxs-JO2 relationship..’

Lines 257-258: You also show the DYFAMED station in this figure. Please mention it is for comparison and cite Sternberg et al. (2008).

Line 258: ‘[c] potential temperature-salinity-depth plots…’

Line 269: Could you integrate the DWA Baxs between 100-500m as well (to match with the PHP integration)?

Lines 269-271: ‘Regression of the same ratio is reported for KEOPS1 ([light blue symbols;] out plateau stations) and KEOPS2 ([dark blue symbols;] Southern Ocean; Jacquet et al., 2015) and #DY032 ([red square;] PAP station, NE-Atlantic; pers. data) cruises.’

Lines 269-271: Please clarify what the blue dashed line represents. Is it from Jacquet et al. (2015) or does it take into account all data points including the new ones from ANTARES and PAP stations?

Line 275: mmol/m2/d instead?

Lines 275-276: ‘..optode measurements (this study; [green square]), dark community respiration DCR (winkler titration; [red triangles]; JO2-DCR; Jaquet et al., 2015; KEOPS1)’

Lines 277-278: It is not clear if you speak about the y-axis or the black line. I propose to re write as ‘…and [Baxs contents (Southern Ocean transfer function from Dehairs et al. (1997); JO2-Ba]. The black line corresponds to the correlation found in Jacquet et al. (2015)’. If this is correct, please also mention that this correlation excludes some data points from A3 and E stations.

Figure 2a, in the legend: Ba[xs] ANTARES; Ba[xs] DYFAMED; [p]Al ANTARES. And please show the full depth profile (until 2000m).

Figure 2b: Please indicate from where the blue line comes from. And indicate the p-value.

Figure 3a: Please check the units and indicate JO2 in umol/L/d. And show the trend from Dehairs et al. (1997) in the Southern Ocean. Give the p-value.

Figure 3b: Please indicate JO2 in umol/L/d. Also, indicate from where the black line comes from. And indicate the p-value.

C. References:

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