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Ocean Science Discussions



# *Interactive comment on* "Variability and stability of anthropogenic $CO_2$ in Antarctic Bottom Waters observed in the Indian sector of the Southern Ocean, 1978–2018" *by* Léo Mahieu et al.

**Léo Mahieu et al.**

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General comments

The study presents results from a time series in the Indian sector of the Southern Ocean, which together with historical relevant data span a 40-year period. Using this time series, the authors evaluate the evolution of anthropogenic CO2 (Cant) in the Antarctic Bottom Waters (AABW). It is an interesting and generally well written work, and generally good figures and tables. There are some need for clarity in some parts and there is some concern of the treatment of data gaps, but most of this should be rather easily dealt with, and I recommend publication after minor revision. A detailed



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list of comments follows below.

Response: The authors are thankful for the fast answer and the positive interest given to the manuscript, as well as for the numerous valuable comments.

My main comments are related to the definition and subsequent presentation of AABW, and, the data gap between 1987 and 1998 and how this is handled and presented. To start with the definition of AABW, this is not an issue in itself, since the denser definition has been used before, and also, since almost any definition can be accepted as long as it is clearly presented. The latter is the problem here, at least for someone not as familiar with the area and these water masses (I usually work in the high-northern latitudes). The definition and choice is clearly described in 2.3, but, then the reader is referred to Fig. 3, where AABW is noted in the layer above the focus of this study, while the data evaluated is in the layer annotated “Considered data”. When then the results of the property evolution of AABW are further presented in Fig. 4, at least I got somewhat confused. Whether this is only me or not, this may call for some added clarity. I would suggest to annotate your AABW layer (hence at neutral density >28.35) as AABW (or AABW* or similar), to make this clear, and then make a distinction with the more common AABW.

Response: the authors understand the concern of the reviewer. To solve this potential confusion, we suggest labelling the AABW as define in our manuscript (neutral density >28.35 kg.m-3) Lower Antarctic Bottom Water (LAABW).

Nevertheless, this mostly refers to Fig. 3, and I have several concerns with this figure, as detailed below. Hovmöller plot is a wonderful thing, and can be very illustrative. However, it can also be deceiving, especially when there are gaps in the data, and the gridding is allowed to interpolate over these gaps, which often can create features that give a false picture of actual evolution. Fig. 3 suffers from this when plotting the older data (1978–1987) together with the OISO time-series data starting from 1998. There are several peculiar features in Fig. 3, especially for Cant and AT. The fact that most of





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the other plotted parameters show overall stable layer properties, over the full period, may seem to reduce this concern, but I am not convinced. In addition, I'm not fully convinced about the benefit of showing depths from 1500 m, when almost all results and discussion are concerned with the layer below 4000 m. Even more so when the upper layers seems to show most of the strange features, for example the minimum in Cant in the older data (which may in part show the issue with the TrOCA method, with even negative concentrations, which are not realistic, in the most upper part of the deep waters).

Response: The authors agree that the figure needs to be upgraded, clarified and simplified. The suggestions of the referee have been taken into account by redrawing the Fig. 3 (now Fig. 2) using only the OISO data (from 1998 to 2018). The extrapolations were very misleading indeed, so the figure is now drawn with weighted-average gridding (and limited extrapolation around the data point). The aim of this figure is to show the differences in AABW and LCDW characteristics before focusing on the variability and trends observed in the bottom layer (it also shows that the neutral density 28.35 is a better definition for a more homogeneous bottom layer that we now define as LAABW). In addition, the control quality of the data is performed in the old deep waters (well characterized in the figure by the maximum in CT). Following the recommendation from the other Referee, we propose to add a figure in Supplement Material (Fig. S1) showing the consistency of our dataset at the two OISO stations where samples were collected down to the bottom, the OISO-ST11 presented in the manuscript and the OISO-ST17 sampled in the Subtropical Zone (30° S-66° E). This figure shows a limited number of measurements that are out of the range of tolerance, but one has to keep in mind that interannual (or multiannual) variations may occur and this calls for great care before applying an adjustment. Since 1987 (when the cruise INDIGO3 was performed), a shift in AT is suggested at high latitudes by the comparisons of INDIGO3 data (unadjusted, following the GLODAPv1 and CARINA recommendations) with other cruises data (adjusted, following the GLODAPv2 recommendations). This comparison shows differences that range between -4 $\mu$mol.kg-1 and +10 $\mu$mol.kg-1 (Fig. S2).

Most of the crossovers that suggest a positive offset for INDIGO3 data (between +6 $\mu$mol.kg-1 and +10 $\mu$mol.kg-1) are found south of 60°S, suggesting that AT may have decreased in deep waters at high latitudes since 1987. This is why we first decided for no adjustment in the submitted manuscript (as in the GLODAPv1 and CARINA data products, whereas the INDIGO3 data in GLODAPv2 were corrected by -8 $\mu$mol.kg-1). However, at the OISO-ST11, AT data from the INDIGO3 cruise are also about 8 $\mu$mol.kg-1 higher than the mean value in deep waters (2000-3000m), in good agreement with the other crossovers at high latitudes. In order to reduce the potential bias that could result from either over-adjusting the data (GLODAPv2 recommendation) or not adjusting the data (GLODAPv1 and CARINA recommendation), and because most of the crossovers at mid-latitudes suggest a small positive offset, we propose to apply an intermediate adjustment of -4 $\mu$mol.kg-1 in the revised manuscript (the impact on Cant is +2 $\mu$mol.kg-1). The Fig. 3 (before Fig. 4) presenting the interannual variability of the LAABW properties and the Table 2 presenting the calculated trends will be adjusted correspondingly. Fig. S2 will be completed by the list of the cruises presented. The Figure S1 also shows that the low AT values between late 1998 and 2004 are found both in the Antarctic zone and the Subtropical zone. This is surprising, but there are no reason to believe that the data are biased since CMRs were used for all OISO cruises, and the instrument and data processing were the same during the first OISO cruise in January/February 1998 (showing AT values close to the mean in Fig. S1) and the following cruises.

The interpolation of this minimum patch leads to unfortunate wordings in the results, such as on line 236, with “a sudden increase. . . between January and December 1998” seems to refer to the low values calculated for the 1987 data and the clearly higher concentrations calculated for the OISO data. (I also don’t really understand the “between Jan and Dec 1998” part, since the first OISO data were sampled in Feb 1998, and the next in Dec the same year.) Apparently there are some need for clarifications here, but also to be cautious when interpreting interpolated values over large gaps. One way to solve this is of course to exclude the older data from the Hovmöller plots.

These can still be used in the comparison/evaluation, and included in Fig. 4.

Response: the reviewer is right about the issue for the 1998 samplings mentioned (same as Reviewer 1). This is because the first OISO cruise started in January 1998, but the station 11 was actually sampled in the beginning of February as mentioned in Table 1. This will be corrected. We also agree that extrapolation can be misleading and we thank the Reviewer for pointing this issue. Having removed the GEOSECS and INDIGO data from the Hovmöller plots (Fig 2., before was Fig. 3), the extrapolation is no more an issue for interpreting the signal observed for the first OISO cruises, but the increase in Cant between February 1998 and December 1998 remains (from < 6 $\mu$mol.kg-1 to about 10 $\mu$mol.kg-1).

To continue on this figure (Fig. 3), for the bottom layer, the fact that it is stretched below the deepest samples seems to create at least the distinct maximum in mid-2000s. Perhaps this will be reduced if the maximum depth/pressure is set to the deepest sample, to exclude extrapolations below that depth.

Response: Having removed the INDIGO1 data from the Hovmöller plots, this no more an issue because the deepest sample is collected at the same depth for all cruises

Specific comments

L18: Do the changes here (+7 and +13, respectively) refer to the whole period? Please clarify.

Response: these are not changes, but Cant concentrations. The following rephrasing is suggested: 'from the average concentration of 7 $\mu$mol.kg-1 calculated for the period 1978-1987 to the average concentration of 13 $\mu$mol.kg-1 for the period 2010-2018.'

L23: A rather tiny remark, but the use of "pluriannual" may be grammatically correct (I'm not a native English speaker), but consider using "multiannual" (or multi-annual), which are more common (I believe). The same is used on L360.

Response: We agree that this is maybe not the best word to use. It will be replaced by

‘multi-annual’.

L59: I’m expecting a reference in the end of this sentence. This may be refer to the reference in the previous line, but you may consider moving this to the end.

Response: We agree that the reference is misplaced. It will be moved to the end of the sentence.

L95: I can’t find a definition of “AAC” anywhere. Please write out and define the first time.

Response: We agree that the definition of ACC is missing (Antarctic Circumpolar Current). That will be corrected.

L96-97: Unclear sentence. Need some rephrasing/re-writing. Suggestion: “. . .Weddell Sea, where deep and bottom waters are produced. . .”.

Response: The sentence will be rephrased as suggested.

L98-100: In the same sentence, there are several instances where the full water mass name is not spelled out, for example “the Ross Sea (RSBW; . . .”. This may be intuitive, but I don’t think the full names of some of these are written out at any place in the manuscript so would suggest to consider doing that at some place.

Response: The full names will be added explicitly.

L100: Rephrase: In the Prytz Bay, AABW formation has also. . . This sentence is overall quite unclear, especially the last part, so please consider rewriting for clarification.

Response: It is indeed quite unclear. We propose the following rewriting : ‘AABW formation has also been observed in the Prydz Bay (Rodehacke et al., 2007; Yabuki et al., 2006). There, three polynyas and two ice shelves have been identified as Prydz Bay Bottom Water (PBBW) production hotspots from seal tagging data (Williams et al., 2016). This PPBW flows out the Prydz Bay through the Prydz Channel and get mixed with the CDBW.’

L105: The “Warm Deep Water” is not described, so not easy to follow without a previous knowledge of the area and the present water masses. Please clarify.

Response: we agree that it may be difficult to follow. The Warm Deep Water is slightly modified Circumpolar Deep Water (by mixing with surface waters when it enters the Weddell Basin). For simplification, we suggest rewriting as follows: The exported WSDW originates from the Circumpolar Deep Water (CDW) that enters the Weddell basin and mixes with WSBW and High Salinity Surface Water (HSSW) (see Fig.2 in van Heuven et al., 2011).

Section 2.4: Part of this section, and in particular from L133, deals with results of Cant from the methods not yet described. I would suggest to move this to the Result section, at least the Cant parts, or maybe part of the Discussion.

Response: the authors agree that this section does not fit in the material and method part of the manuscript, but rather in the discussion section as suggested.

L152: Since the “P” in GLODAP refers to “Project”, the “project” after should be avoided (I think). You could rephrase this into something like: . . ..not yet qualified (or included in) the most recent GLODAPv2 product.

Response: the mention of GLODAP will be rephrased as suggested.

L161: The stated accuracy for temperature and salinity seems too low. The standard CTD accuracy, for example found at the GO-SHIP home page (Hydro-manual) is 0.002 for both. Please check.

Response: the authors agree and will correct the accuracy for temperature (0.002°C) and salinity (0.005 for measurements using a salinometer).

L161: As far as I can see, this is the first time “AT” is mentioned, but not defined. Please add this. L166: Same for “O2” as for AT above. Please define first time.

Response: AT and O2 will be defined here.

L170: You mean “onshore”?

Response: the reviewer is right about this mistake.

L184: Clarify which “Redfield ratio”. You mean the C:O ratio? Please add this.

Response: Initially, only C/O2 and N/O2 ratios were involved in the definition of the parameter ‘a’ (Touratier and Goyet, 2004b; Lo Monaco et al., 2005b). In the latest definition of the method Touratier et al. (2007) presents an upgraded definition of this parameter by combining the Redfield equation coefficients for CO2, O2, HPO42- and H+ and the same rules of construction as Broecker (1974) did for tracers NO or PO. Because we want to keep the explanation simple in the manuscript, we suggest to rephrase L184 as follows : ‘where a is defined in Touratier et al. (2007) as combination of the Redfield equation coefficients for CO2, O2, HPO42- and H+. For more details about the definition and the calibration of this parameter, please refer to Touratier et al. (2007).’

L217: Either remove “after”, so it reads “. . . and only impacted by. . .”, or if more correct, add “subduction”, so it reads “and after subduction only impacted by. . .”.

Response: the word ‘subduction’ will be added as suggested.

L233: “LCBW” is here mentioned for the first time, without definition or any description anywhere in the manuscript, as far as I can see. Please add this.

Response: LCDW refers to the Lower Circumpolar Deep Water laying above AABW in the entire Southern Ocean. Details about this water mass will be added in Section 2.2 where it is first mentioned.

L235-236: This is what was commented on in the generall comments above, with the “sudden increase”. Please revise and clarify. It is more likely that there was a more gradual evolution, and none of the other parameters calls for any sudden changes. Also, the data quality and methods between the older data and the OISO data may differ, so extra causion is taken when comparing them.

Response: we removed the older data form the Hovmoller plot, but the change in Cant in LCDW remains (from <6 $\mu$mol.kg-1 in Feb 1998 (similar as for the older data) to about 10 $\mu$mol.kg-1 for the following cruises).

L240: The maximum in Cant in 2004 is one occasion, and followed by five (almost six) years without any data. I would be cautious to over interpret this. However, it co-incides with a maximum in oxygen, which could indicate a ventilation event.

Response: we agree with the referee about being cautious with the measurements in 2004. Indeed the maximum in Cant is due to the maximum in O2 (not associated with a maximum in CT).

L256-260: The lower concentrations of AT in the years around 2000 at all depths below (at least) 1500 m (have you checked the whole water column?) seems a bit odd. Especially when this is not seen in any of the other parameters. Also, when comparing two years in the 1980s with data more than a decade later, one should be extra cautious in the interpretation, not the least when the two years/occasions in 1985/87 show the highest concentrations seen over the evaluated period. Certainly the years after 2000 show much lower concentrations, which may be a phase due to a change in different forcing, but to suggest reduced calcification from only a few years/occupations of data is very speculative, and clearly something that change a few years later.

Response: As mentioned in the general comments, the low AT values between late 1998 and 2004 are found both in the Antarctic zone and the Subtropical zone (Figure S1), but they are not observed in the surface layer (this will be added in the revised manuscript). The hypothesis about reduced calcification could explain this contrast between the surface waters and the deep ocean.

L259-260: Is it realistic that the increase in CT is lower than the accumulation of Cant?

Response: The small increase in CT over the period 1987-2004 could be caused by a reduction in CT,nat around the year 2000 (associated with the low AT values). This

said, we also have to keep in mind the uncertainty on the Cant calculations. This will be clarified in the results and in the discussion.

L261: While there is a rather clear trend in oxygen during this period – although I would be careful in talking about trends over such short periods, especially when comparing to a year with a maximum (2004) – there is no trend in Cant. Instead the latter shows some clear interannual variability. Also, the "trend" in temperature is indeed very small, and even if not significant, the change, or better, variability, in salinity is rather large. Consider these points when revising this part. Your statement on L267-268 highlights this issue.

Response: we agree with the reviewer that there is no clear trend in Cant over 2004-2018. We will change "decrease in Cant" for "no increase in Cant". The same is true for temperature and salinity.

L270-271: There is also a maximum in temperature in 1985, so this could indicate more mixing with WSDW, which are both fresher and warmer.

Response: we agree with the reviewer that more mixing with WSDW (or CDBW) could also explain the higher Cant concentrations and lower S in 1985 (the signal in temperature is not well marked due to the large error bars). This will be added in the text.

L275-278: This is a very long sentence. I suggest to divide it, with period after ". . .the underlying deep waters." Then remove "and", and start on "Since", or change the start of the sentence. For the last part of this sentence (L277-278), the suggestion ofincreased contribution from the Ross Sea is not clear to me since the oxygen decrease, while the salinity goes up and down. Or are you only referring to the one occupation in 2012? (If this is the case, it seems to detailed to explain a single year taken out of a long time series.)

Response: the suggestion made by the reviewer to shorten the sentence will be used. Our aim is to discuss the variability in Cant concentrations that could reflect variations

in the contribution of different types of AABWs. We suggest that the lower Cant concentrations observed in 2011, 2012 and 2013 may be due to an increased contribution of older types of AABW. We agree that pointing to RSBW as a possible candidate because salinity was higher in 2012 is too speculative. This will be removed.

L280: The stated freshening of 0.01, for which period is that observed? Please clarify.

Response: The sentence will be corrected as follows: 'The freshening in S of -0.006 decade-1 between 2004 and 2018 that we observed on the Western side of the Kerguelen Plateau was also observed on the Eastern side of the Plateau by Menezes et al. (2017) over a similar period.'

L312-313: ". . .(15 umol kg-1) due to mixing with older CDW."

Response: the sentence will be corrected.

L317: "that contain very high amounts of Cant . . ."

Response: the sentence will be correct as suggested.

L318-320: The last sentence of this paragraph basically repeats what have been said above. Consider to remove.

Response: the authors agree with the reviewer and will remove this sentence.

L325: Here you write out "Southern Ocean" after having used the abbreviation throughout the manuscript, even the sentence before. Consider to revise.

Response: "Southern Ocean" will be changed to SO.

L340: "evaluated" should here instead be "estimated", or "calculated", or "found" (I think).

Response: "evaluated" will be replaced by "calculated".

L386: Consider rewording ". . .vary in a very large range. . .". Suggestion: "show a very large variability", or maybe, "vary over a very large range".

Response: the rewording ‘vary over a very large range’ will be used.

L387-388: “(–221 mmol C m-2 d-1; Roden et al., 2016).

Response: we will correct this according to the reviewer suggestion.

L416: Both these water masses (RSBW and ALBW) have higher salinity, and while oxygen show a reduced trend the salinity goes up and down, so this explanation does not hold for all years during this period.

Response: we understand the concern of the reviewer. The mention of the WSDW will be added, as for the response of the comment L275-278.

L424: “explains most, but not all, of the observed. . .”

Response: the sentence will be corrected.

L463: GLODAPv2 version are written as “GLODAPv2.2021 (.2020 is soon to be released). You do mean 2021 and not 2020?

Response: the data will not be included in GLODAP in the 2020 version, but in the following one.

L851-853: Table 2 (and in general): You may want to consider if you want to keep AOU as parameter, when you mostly refer to oxygen. The trends are almost exactly thesame (but opposite of course), and gives the same message.

Response: we agree with the reviewer. AOU will be removed from Table 2 and from Figure 2 (and from the corresponding parts in the text).

Technical comments

L22: This is, however, modulated. . .

Response: the comas will be added.

L35: The references should, typically, be chronologically ordered. Please check

throughout the manuscript. (There are more examples of this, but I won't comment on this more.)

Response: we agree with the referee. We will check for other occurrences.

L71: This is, however, not the. . .

Response: the comas will be added.

L91: ". . .(405 and 465 km, respectively)."

Response: the coma will be added.

L107-113: Examplified with ". . .East of the Kerguelen. . .", this section has many of these "directions/locations" (east/west/. . .) spelled with a large letter, even not part of a name. I think this is not correct, and if so, please change.

Response: this will be corrected.

L118-119: . . .28.27-bottom, respectively. . .

Response: the coma will be added.

L172-173: Change font; the part of the sentence from "for deep samples. . ." are in a different font (maybe "Cambria").

Response: the font will be changed.

L220: Change font for "value for".

Response: the font will be changed.

L306: Add a comma: "2018 (Fig 3a), probably . . ."

Response: the coma will be added.

L340: Add a ".": Pardo et al. (2017)

Response: the dot will be added.

L347: For consistency, change “South-Western” to “South-western” (similar as on L325).

Response: this will be corrected.

L449: Remove “.” for consistency: (e.g. Frölicher et al., 2014).

Response: the coma will be deleted.

L451: References in chronological order.

Response: we agree with the referee. This will be corrected.

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Interactive comment on Ocean Sci. Discuss., https://doi.org/10.5194/os-2020-37, 2020.

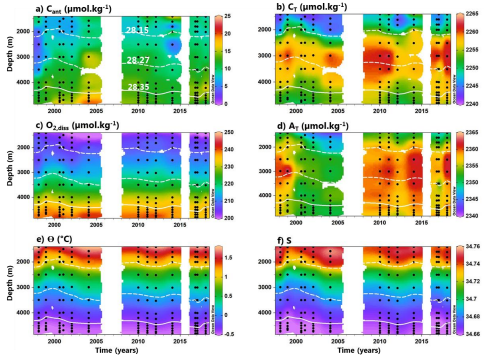


**Figure 2. Hovmöller section of (a) Cant via TrOCA, (b) CT, (c) O2, (d) AT, (e) θ and (f) S based on the OISO data presented in Table 1. Data points are represented by the black dots. The white isolines represent the water masses separation by γn (from the bottom: LAABW, UAABW and LCDW). Figure produced with ODV (Schlitzer et al., 2019).**

**Fig. 1.** Figure 2. Hovmoller section

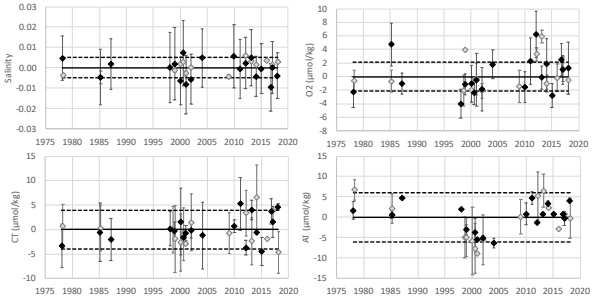


**Figure S1. Mean differences observed in deep waters ($O_2$ minimum) between the measurements obtained during one cruise and the mean value calculated over the full period at the two sites where samples were collected down to the bottom: the OISO-ST11 in the Antarctic Zone (56.5° S-63° E, station investigated in this study, in black) and the OISO station 17 in the Subtropical Zone (30° S-66° E, in gray). The dashed lines indicate the limits for considering an adjustment (as defined in the CARINA and GLODAP syntheses). The data plotted here are adjusted as recommended in the GLODAPv2 synthesis, except for $A_T$ in 1987 (INDIGO3 cruise) that was adjusted by -4 µmol.kg$^{-1}$.**

**Fig. 2.** Figure S1. Quality control of the bottom OISO measurements



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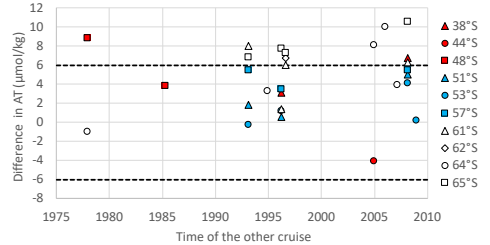


Figure S2. Difference in $A_T$ found in deep waters at crossovers with the INDIGO3 cruise (INDIGO3 - the other cruise). Data are adjusted as recommended in the GLODAPv2 synthesis except for the INDIGO3 cruise for which $A_T$ was not adjusted.

**Fig. 3.** Figure S2. Crossovers of the INDIGO3 AT measurements



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