

Interactive comment on “Variability and stability of anthropogenic CO₂ in Antarctic Bottom Waters observed in the Indian sector of the Southern Ocean, 1978–2018” by Léo Mahieu et al.

Anonymous Referee #2

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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 list of comments follows below.

My main comments are related to the definition and subsequent presentation of AABW,

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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. 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 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). The interpolation of this minimum patch leads to unfortunate wordings in the results,

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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. 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.

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

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.

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.

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

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

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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.

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.

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.

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.

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. 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.

L170: You mean “onshore”?

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

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. . .”.

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.

L235-236: This is what was commented on in the general 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

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differ, so extra caution is taken when comparing them.

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 overinterpret this. However, it co-incides with a maximum in oxygen, which could indicate a ventilation event.

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/occasions of data is very speculative, and clearly something that change a few years later.

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

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.

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.

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 of

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increased 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 occasion in 2012? (If this is the case, it seems to detailed to explain a single year taken out of a long time series.)

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

L312-313: “. . .(15 $\mu\text{mol kg}^{-1}$) due to mixing with older CDW.”

L317: “that contain very high amounts of Cant . . .”

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

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

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

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

L387-388: “(-221 $\text{mmol C m}^{-2} \text{d}^{-1}$; Roden et al., 2016).

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.

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

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

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 the

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same (but opposite of course), and gives the same message.

Technical comments L22: This is, however, modulated. . .

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.)

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

L91: “. . .(405 and 465 km, respectively).”

L107-113: Exemplified 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.

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

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

L220: Change font for “value for”.

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

L340: Add a “.”: Pardo et al. (2017)

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

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

L451: References in chronological order.

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