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Biogeosciences Discussions

Interactive comment on "Temporal variability of the anthropogenic CO_2storage in the Irminger Sea" *by* F. F. Pérez et al.

Anonymous Referee #2

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"Temporal variability of the anthropogenic CO2 storage in the Irminger Sea" by F. Perez, M. Vazquez-Rodriguez, E. Louarn, X. A. Padin, H. Mercier, and A. F. Rios

General remarks

The presented manuscript deals with estimating the anthropogenic carbon (C_ant) in the Irminger Sea from oceanic sections spanning a period of 25 years. The focus is on those water masses that contribute to the North Atlantic Deep Water, and, as e.g. Labrador Sea Water, are affected by changes in remote or local water mass formation. The authors aim at quantifying changes in the C_ant inventories which they address as changes in the oceanic C_ant storage. While facing changes in the global climate, it is highly desirable to have time series which indicate changes in the oceanic uptake and

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storage of C_ant. These interesting issues are addressed in the manuscript and make it certainly suitable for considering publication in Biogeosciences. However, a revision of the manuscript is definitely necessary. Details are given in the following.

The abstract should be filled up with more results rather than broad and general comments. Relevant key points should be worked out more properly. Some repetitions especially in the 'Discussion' section could be removed. Essentially, the section about 'Methods' is too short. First of all, I would expect some more details about the source of data, at least those, which have not been measured by the group of authors. Additional data from 1991 (Meteor cruise M18) as well as 1994 (Meteor cruise M30/3) and 1997 (Meteor cruise M39/4, M39/5) are certainly publicly available either as part of the CARINA data set or at the former WOCE data server, and to my knowledge these cruises include carbon related measurements. Wouldn't it be worth to include these data in the analysis since it would yield another data point for 1994 and it would provide additional estimates for 1991 and 1997 which can be used for direct comparison with the presented estimates? Did the authors only analyze bottle data or did they also look at CTD data ? Layer averages of S or Theta can be estimated much more accurately from CTD data. The missing DSOW temperature signal in 1997 as discussed in the text seems to stem from focusing on bottle data. Statements concerning differences in the horizontal and vertical resolution of the particular sections are missing. The cruise from 1991 termed 'AR7E' was carried out shortly after the winter season has ended. The 27.68 isopycnal outcropped in the Irminger Sea at this time. The section also did not cover the boundary current region. Any effect resulting from these issues is not discussed. Does this mean, it does not have any effect ? The method which was applied to deduce C_ant is presented in a manuscript which is not yet available to the public. It is therefore necessary to outline at least the basic ideas, assumptions, and improvements with respect to established methods. In later sections of the manuscript, further methodological approaches are introduced which might be gathered together in section 2. Concepts like the mean penetration depth (MPD) have been applied similar to the studies of Alvarez et al. (2003). The reader of the present manuscript might not

BGD

5, S733–S745, 2008

easily understand these concepts without going back to the earlier paper and some statements related to MPD are not really convincing (see Specific Comments). Details should be provided with respect to the calculation of the C_ant inventories. Regarding the fact that authors only have section data along more or less the same line, why should this be considered representative for the entire Irminger Sea. Azetsu-Scott et al. (2003) scaled their section-derived inventories with a basin-wide survey. Have similar approaches been undertaken here as well? At least, this needs some discussion. The interchangeable use of storage, uptake, and inventory is sometimes a bit confusing. The structure of section 3 (Results) should be better elaborated. There are several back-and-forth jumps in the discussion, from one water mass to another, from one parameter to another. This might lead to confusion. Many aspects concerning changes in T and S of different deep water components have already been discussed by e.g. Yashayaev and Dickson (2008, ASOF book), Yashayaev et al. (2008, ASOF book) or earlier by Falina et al. (2007, JGR). Based on their own T/S data, basically, the present authors come to similar conclusions. Of course, they need these to interpret the carbon-related results, but one might shorten this section a bit and strengthen the focus. The section dealing with Csatant or its percentage is not really clear and needs to be revised. Some material is presented as a table (e.g. table 2). Increase versus decrease of certain properties as discussed in the text is, however, easier to infer, if it is presented as figures rather than as data in a table. I also strongly recommend adding isopycnals to figure 2, so the reader can detect changes in the stratification and layer thicknesses of the particular water masses. In some occasions I found it a bit disappointing how the authors use their citations. In some cases, references are missing or are incomplete in the reference section. In other cases, citations are used to corroborate statements made by the authors, but they are not always appropriate and should be used more carefully. In the following, specific remarks are listed and sorted by page numbers and then line numbers. These might help the authors to identify those statements which need clarification. After comments related to the text, comments related to tables and figures are added.

BGD

5, S733–S745, 2008

Specific remarks

page 1588

line 15: The reference "Canadell et al. (2007)" is entirely missing in the reference section.

lines 16-18: At first, the uptake of CO2 is governed by the air-sea gas exchange. The deep water formation as one of the key processes of the MOC is then the gateway for CO2 to enter the deep ocean. Following Wallace (2001) storage and uptake should not be confused. This is is used interchangeably throughout large parts of the manuscript and should be clarified.

lines 23-25: It should be noted that there is on-going discussion concerning the interpretation of the results presented by Bryden et al. (2005). The presented time series has a very coarse temporal resolution (5 particular estimates in 5 decades) and the slowing signal is comparable to the uncertainty (see Kerr, 2005, Science).

lines 25-26: Citing McManus et al. (2004) in this context is probably not quite correct, since they did not pose a warning related to a possible future shutdown of the MOC as a response to global warming.

page 1589

line 2: Reference "Drijfhout et al. (2006)" is missing entirely.

line 5: Reference "Böning et al. (2006)" is entirely missing in the reference section. Several OGCMs are mentioned. Please, be more precise. If you do not simply refer to Böning et al. (2006), please, list further studies since Böning et al. (2006) basically analyzed a set of three different OGCMS from the same family of models which differ in their horizontal resolution.

lines 5-6: Please add a reference, a possible weakening of the MOC in the 21st century so far has not been detected in the observations without ambiguity, so you likely refer

BGD

5, S733–S745, 2008

to one or several model studies.

lines 9-10: Reference 'Yashayaev et al. (2008)' is listed as 'Yashayaev and Dickson' in the reference section. But probably the authors refer to the different study of Yashayaev et al. (2008) which was published in the same book. This should be clarified, since the reference is used several times.

line 16: Something like 'thickness of up to 2000m' should be inserted to clarify this sentence. It should be noted that the penetration of convection down to depths of about 2000m is rather an exception and not the rule. Though Lazier et al. (2002) noticed the active convection of the early 1990s reaching down to 2000m, they did not relate this to the North Atlantic Oscillation index.

line 19-20: Please, add some more information about the relationship between the strength/phase of the NAO and convection activity in the Labrador Sea, e.g. why a positive NAO-index supports/enhances deep convection. The reader might not be necessarily familiar with these issues. Also, please, add a reference.

line 23: What is "weaker" in comparison to "strong" convection? Please provide numbers, e.g. convection depths.

lines 24-25: Azetsu-Scott et al. (2003) and Stramma et al. (2004) focused on that particular product of convection that emerged in the late 1990s. However, the existence of ULSW has already been discussed earlier by Pickart (1992) and Pickart et al. (1996, 1997). They observed uLSW to be formed in the DWBC region of the Labrador Sea.

line 28: The decadal time series of LSW layer thicknesses have been presented earlier by Kieke et al. (2006).

page 1590

line 1: Please, explain CFC, since this is an abbreviation which should be introduced prior to using it.

BGD

5, S733–S745, 2008

lines 1-5: Figure 4 of Azetsu-Scott et al. (2003) does not really indicate a decline in the CFC-12 concentrations of cLSW. Changes between the observed annual values from the period 1995-2000 appear to fall within the range of associated uncertainty. Kieke et al. (2007) presented CFC-12 inventories, which certainly declined. But in fact their inventory is a product of layer thickness and CFC-12 concentration, and changes in the layer thickness might be greater than changes in the CFC concentration.

line 7: 'seven' should be changed to 'six'

line 15: Please, mention the data source for the non-OVIDE sections.

line 16: Please, mention whether the cruise from 1991 also did include CRM data for CT measurements.

lines 20-22: Tanhua et al. (2005) recommended to apply one single and constant off-set to all available TTO-NAS sections, and they provided an explanation. I would suggest to mention this here. Otherwise, the reader cannot easily infer, why your approach, which is simply following the approach of Tanhua et al. (2005), seems to be appropriate. What precisely is 'approximately -3.0 μmol/kg' ?

page 1591:

line 1: It should be clearly stated whether figure 1b indicates one particular density field from one particular section or an averaged density distribution. In 1991, for example, the 27.68-isopycnal outcropped in the Irminger Sea. It would also be much helpful if the chosen isopycnals would be included in the respective subplots of figure 2. Since the Vázques-Rodríguez et al. manuscript is not yet available, more details are necessary to understand the methodological approach. For example, what is the particular improvement in comparison to existent methods ? The reader might not be too familiar at all with the method 'how to derive C_ant'. Furthermore, what is the rationale for assuming that choosing a subsurface layer at 100-200 m should avoid seasonal variability of surface properties ? Like the Labrador Sea, also the Irminger Sea is assumed

BGD

5, S733–S745, 2008

to be affected by oceanic convection, though at shallower levels. Therefore, also the chosen subsurface level might still be pertained to seasonal effects.

lines 9-15: The error analysis could benefit from adding more details or any reference to the applied method.

line 17: Please, introduce the abbreviations O2_eq and O2_meas.

lines 17-21: This long sentence should be cut into smaller pieces.

line 20: Please add 'section' after 'TTO-NAS'. The authors discuss the stratification, but the density field is not included in figure 2.

line 23-line 1, page 1592: The sentence starting with 'It must be reminded...' should be linked better to the sentences given before, so the implications becomes more clear.

page 1592:

line 3-4: Please, add reference Pickart et al. (2003).

line 6: Kieke et al. (2006) did not show data for 1991 from the Labrador Sea, but Azetsu-Scott et al. (2003) did, but the latter did not analyze time series of layer thickness. Please, verify this statement.

line 9: Yashayaev et al. (2008) already observed the temperature minimum one year earlier in 1996, but this cannot be resolved in the data set which is investigated by the present authors.

lines 9-10: This sentence is unclear since the authors list average DSOW temperatures of +/- 1.72 degC in 1997 (Table 2). Please, also add the year of FOUREX cruise. The missing DSOW signature in 1997 seems to be an artifact resulting from using bottle data. Analyzing CTD data yields a pronounced DSOW layer with temperatures well below 2°C. Thus, the explanation for the "missing DSOW signature" given by the authors a few lines later seems not to be appropriate.

BGD

5, S733–S745, 2008

lines 14-16: Please, also note that Yashayaev and Dickson (2008, ASOF book) describe the spreading of a salinity maximum in the NEADW which has its origin near the overflow regions.

lines 16-17: Kieke et al. (2006) have limited their analysis to the years 2003 and earlier, but the paper by Rhein et al. (2007, GRL) supports this view for the period 2003-2005.

lines 23-24: Please, clarify sentence.

line 24: 'West' should be changed to 'west'. What is precisely meant with 'west of the Irminger Basin' when the authors discuss a section from the Irminger Basin ? It should be something like 'in the western part ...'.

line 29: Please, mention where deep convection does occur. Do the authors refer to the Irminger Sea ?

page 1593

line 3: Reference should be changed to Rhein et al. (2002, JPO) and/or Lazier et al. (2002, DSR-1).

line 9: Please, introduce the abbreviation CGFZ. It is not necessarily the case that LSW and NEADW flow in opposite directions. L'Herminier et al. (2007) provided one snapshot. Another one from the same year was presented by Schott et al. (1999, GRL). These authors reported on a blocking event observed summer 1997, when the location of the North Atlantic Current above the CGFZ was responsible for eastward flow of LSW and blocking of westward flow of NEADW.

line 12: Please, add Rhein et al. (2007, GRL) as a further reference, since their analysis covered the years 2003-2005, while the analysis of Kieke et al.(2006) ended in 2003.

line 15: In contrast to this statement, AOU in the cLSW appears to be lower in 2006 compared to 2004.

BGD

5, S733–S745, 2008

line 24: 'errors' should be replaced by 'uncertainties'. Please add isopycnals to figure 2, so the reader can easily infer water mass boundaries.

lines 26-27: Please, clarify whether averages of layer thickness, salinity and temperature have been derived from bottle data or CTD data. At least for the DSOW observed in 1997 it is striking that the average temperature is quite lower than one can infer from the temperature distribution shown in figure 2.

page 1594

lines 14-15: Please, explain why DSOW is excluded from the correlation analysis.

line 21: It should be noted that strong entrainment close to the Denmark Strait sill can result in direct exchange between the LSW layers and DSOW. I cannot see to what extent this approach is further exploited. If one does actually plot AOU versus %C_ant_sat,which is listed in table 2 but not shown by the authors, it turns out that cLSW and DSOW occupy similar ranges of AOU at very different values of %C_ant_sat. This seems to be in contrast with the statement on lines 17-18. Please, clarify this section.

line 26: Which C_ant estimates are displayed here ? The ones estimated using the method of Vazquez-Rodriguez et al. or the ones estimated from AOU - %C_ant_sat ?

page 1595

lines 3-4: Please, add a reference to table 2 since many of the properties which are discussed in this context are not visualized in figure 3 but are listed in table 2. The temporal resolution of the time series is rather coarse, but many more CTD sections are available and could be used to generate annual time series of water mass layer thicknesses. It might be the case that the maximum layer thickness of cLSW occurred already earlier than 1997, but this cannot be resolved with the presented data set.

lines 5-6: Since the density distribution is not provided for each particular section, the reader cannot infer to what degree the layer thicknesses have changed over time. It

BGD

5, S733–S745, 2008

is therefore not easy to deduce from the provided material that cLSW has doubled its thickness throughout 1981-1997 in the Irminger Sea. But it seems that your refer to your table 2. Please, clarify this.

lines 12-13: What is "a very small portion" ?

line 18: See comment to figure 3.

page 1596

line 8: Please, add references of some of these studies. What patterns do these studies describe?

lines 9-10: Add a reference.

lines 17-18: Azetsu-Scott et al. (2003) did not analyze data from the Irminger basin, they focused on the Labrador Sea.

line 19: 2001 should be 2000, since the analysis of Azetsu-Scott et al. (2003) covered the years 1991-2000.

lines 25-26: Again, Azetsu-Scott et al. (2003) focused on the Labrador Sea and not on the Irminger Sea.

page 1597

line 13: Also the method applied by Alvarez et al. (2003) is an indirect approach.

lines 17-18: It is not clear to me, why the MPD should be considered as an index of the convection activity in the Irminger Sea. The estimates vary only by about +/- 200m. What is the measure to distinguish between weak and strong convection ? Earlier it was stated that the TTO section was conducted in a period of low convection activity, but the MPD value from 1981 is as high as in 1997. Several studies present indications that the winterly convection in 1997 reached down to 900-1000m, which is quite deep for the Irminger Sea (see Falina et al. (2007), Bacon et al. (2003))

BGD

5, S733–S745, 2008

and might be considered as that particular year with best knowledge concerning local 'intense' convection activity. In contrast, the OVIDE period was considered as a period of weaker convection.

page 1598

lines 16-22: Please note that the annual decrease of the uptake as presented by Schuster et al. (2007) refers to a much larger area than the Irminger Sea. Furthermore, the results by Corbiere et al. (2007) were included in their estimate. line 22: Earlier on page 1595 it was stated that the decrease during 1997-2006 was only -1.5 mol C/m-2yr-1

References, pages 1599-1601

The references of Böning et al (2006), Canadell et al. (2007), and Drijfhout et al. (2006) are missing.

The reference of Yashayaev et al. (2008) should be listed at the end of this section.

The reference of Wallace (2001) is incomplete. Editors and publisher should be added.

Tables

Table 1, page 1602

The name of the cruise referred to as 'AR7E' should be '91/1', AR7E is the name of the respective WOCE-line. Column indicating date: for reasons of consistency please add missing zeros when indicating months.

Table 2, page 1603

At least for the DSOW observed in 1997 it is striking that the average temperature is quite lower than one can infer from the temperature distribution shown in figure 2. Inventories are only given as percentages. How large is the actual inventory and how it is defined ? The standard deviation of the average layer thickness should be included

BGD

5, S733–S745, 2008

as well for reasons of completeness. As one can deduce from figure 1b, the layer thickness close to the continental slope or Reykjanes Ridge can differ from the thickness in the interior Irminger Sea. The %Inventory of the subsurface layer in 1991 is strikingly lower compared to all other years but this not further discussed. How about outcropping effects that were observable in the data from this section?

Figures

Figure 1

1a) It cannot clearly be seen whether all sections cover the boundary current area. At least the AR7E cruise does not come very close to the shelf break. The TTO-leg is actually leg 6 with only one station from leg 4 (the easternmost) and not leg 4 as indicated in the figure. It looks like the authors only have chosen a selection of profiles. The reason might be that only those profiles provided the necessary CO2-relevant data. But there are more S/T profiles available, even CTD profiles. In figure 2 which appears later, the authors only present those FOUREX stations from west of 34°W. Those stations which are not used for the analysis can therefore be omitted from figure 1a. Otherwise, the reader might be confused. 1b) As was indicated already earlier, it should be mentioned whether the presented density distribution is from one particular section (which?).

Figure 2

The quality of figure 2 is quite low, and details are difficult to examine without magnification to several hundreds of percent. The density distribution should be included. Also ticks indicating the station position are necessary, since the horizontal distribution of the sections is quite different. Though the authors commented on their choice of visualized data in the figure caption, CTD profiles of all sections are available, and it is suggested to use these at least for the salinity and temperature distributions. T/S averages of the particular layers can be estimated much more accurately from the CTD data. It is furthermore suggested to decrease drastically the number of contour labels.

BGD

5, S733–S745, 2008

Currently, they rather make it difficult to deduce any details. Instead, it is recommended to insert dots which indicate the horizontal and vertical distribution of water samples. It should be mentioned in the figure caption that the western and eastern ends of the sections are not necessarily identical.

Figure 3

The box indicating the mean penetration depth is hardly readable and should be considerably enlarged or removed from the figure. As is given in the figure caption, the right axis indicates the inventory. Alvarez et al. (2003) define the storage as the change of the inventory with time, which is probably the same as the 'storage rate' in the present study. This is quite confusing and should be revised.

Interactive comment on Biogeosciences Discuss., 5, 1587, 2008.