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Interactive comment on "A volumetric census of the Barents Sea in a changing climate" *by* Sylvain Watelet et al.

Anonymous Referee #2

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Review of "A volumetric census of the Barents Sea in a changing climate" by Watelet et al.

The manuscript describes a data set of available temperature and salinity mapped to a regular grid in the Barents Sea. The mapping is not described in detail, but instead reference is made to a publicly available software DIVA. Some error estimates of the mapped fields are also provided with the data set, but the estimation procedure is not described. The mapped data is used to perform some basic analyses of temperature and salinity trends in the Barents Sea.

Technically, the manuscript is generally well written and easy to follow, some exceptions (and suggestions for improvement) are listed below. Some of the figures could be revised to improve the presentation and/or conciseness, see below, too.

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Discussion paper

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From the “Aims and Scope” site of ESSD (https://www.earth-system-science-data.net/about/aims_and_scope.html) I cite: “Articles in the data section may pertain to the planning, instrumentation, and execution of experiments or collection of data. Any interpretation of data is outside the scope of regular articles. Articles on methods describe nontrivial statistical and other methods employed (e.g. to filter, normalize, or convert raw data to primary published data) as well as nontrivial instrumentation or operational methods. Any comparison to other methods is beyond the scope of regular articles.”

In this sense the current paper is a data description paper, that should not contain “any interpretation” (but it does). As a methods paper it lacks the description of “nontrivial statistical and other methods”. These issues aside, my main concerns are:

(1) There is a lack of detail in the description of the methods. For the generation of a data set from existing sets, I would have expected at least a rough explanation of the procedure beyond naming the software that has been used, for example, fundamental equations (objective function?) and constraints etc. In the same way, the error estimation method is named (“the clever poor man’s method”, something I have never heard of) with a proper reference (to paper in a journal that I don’t have access to, embarrassingly enough), but that’s all of the information that the reader gets. I think that for this type of journal and this type of derived data set at least a rough outline of the methods is appropriate.

(2) Some choices for gridding are not explained. For example, for a small region like the Barents Sea, why would one use a “lat-lon” grid instead of a proper projection with (nearly) constant grid spacing, or at least a scaled latitude coordinate (dlat = dlon*cos(lat)), so that the grid boxes are nearly square. With the chosen 0.1x0.1 deg grid, grid cells are elongated (making the mapping “anisotropic”) and their volume varies by up to a factor of 2. With this choice, the “volumetric” analysis also contains this factor error of up to 2. As a consequence the volumetric t-s diagrams are not convincing.

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(3) The manuscript claims to provide a comprehensive gapless data set, but then restricts the analysis to certain seasons, regions and years. The point of the entire data set as a whole is not clear to me, if even the authors of the data set don't want to use all of it. After all, the very applaudable inclusion of error estimates should allow to provide robust analyses (including error estimates), even when the underlying data is sparse and the corresponding errors are large. Some explanation seems in place, why we need this data set, if the even the authors don't trust all parts of it.

(4) I have issues with the use of "freshwater" and "equivalent freshwater content" in this manuscript. This strange and non-official convention (see the official definition of freshwater according to section 3.22 of the TEOS-10 Manual (IOC et al. 2010) as 1 minus the Absolute Salinity (in kg/kg)). It has never been clear to me, why one does not use salinity and salt content, which are straightforward and un-ambiguous quantities to describe the change in salinity in a given volume.

(5) I downloaded and superficially inspected the data. The salinity file contains many gaps in time, probably corresponding to the data availability in Fig3, but these gaps are not described in the text. From the text I would have expected annual mean of global mean fields with large error estimates instead (there are no error estimates for the gaps, either). This explain in part my issue (3) for salinity. I guess it makes little sense to fill the gaps where there are no salinity data available, but I think the text should clearly describe the gaps in the gridded data set.

(6) In the gridded salinity fields there are many unrealistically low numbers ($\sim$18 and even a large area of negative numbers down to -18 in timelevel 112 in the northeastern corner over the entire depth) that are not masked in the L1 and L2 versions of the fields (that have been masked according to the relative error thresholds). The temperature fields also contain many values near coastlines or in inlets that seem to be unrealistic, but naturally not as much as salinity, because (I guess) temperature values are generally closer to zero so that accidentally using a zero does not show up as badly as in a salinity field (which typical values around 33).

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The last issue is severe and may make the data set not very useful to the community. All of the other concerns are not major by themselves, but together they will require a major revision of the manuscript and maybe of some of the computations. Hence my recommendation.

Minor comments and suggestion. I am attaching an annotated PDF with the same, but unrevised comments for better context.

page 1 l1: "Due to its location between the Norwegian Sea and the Arctic Ocean, the Barents Sea is one of the main pathways of the Atlantic Meridional Overturning Circulation."

Not sure if this statement is accurate: Why the location between NS and AO the cause of this? Rephrase.

l4: according to my dictionary, "prospect" is not a verb, except for "searching" as in "prospecting for gold". You probably mean forecast/predict or similar?

l16: "the most to the reduction" -> most of the reduction

page 2 l33 and elsewhere: I learned that abbreviations like "e.g." or "i.e." are to be used only within parentheses, but that they should be spelled out in regular text ("for example", and "that is").

page 3 l37: (e.g. hydrographic sections) also an example of limited coverage in time? unless you are talking about repeat sections

l43: "freshwater" see major comment (4) and also discussion of "freshwater" in recent paper by Schauer and Losch (2019), JPO, doi:10.1175/JPO-D-19-0102.1 or similarly Treguier et al (2014), OS, doi:10.5194/os-10-243-2014

l46: post -> after

ll52: DIVA is not introduced properly. Which is the proper reference? Rixen et al? or Troupin et al? In general the algorithm is hard to follow. I would not be able not

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reproduce what you have done.

ll62: “then downgraded to a resolution of 1/8x1/8°”, how? (and improve format for 1/8 x 1/8)

ll64: “The remaining data availability”, is this per year? or per season?

l65: the plots 2 and 3 are difficult to read. Initially I even thought that some of the bars where stacked. Maybe fill the bars?

l66: “on” -> “for”?

l66: “four seasons”, do you bin the data into the four seasons per year? Not clear from the text (and the figures 2 and 3)

page 5 ll72: improve description to make clear that there is a reference field for each season, i.e. 4 per year, etc. what is a “simple data average”? a horizontally averaged value that is use as a horizontally constant reference field/first guess?

ll84: “clever poor man’s method, a good compromise between the computation time and the accuracy (see Beckers et al. (2014))” never heard of this, and unfortunately I don’t have access to Beckers et al. (2014), please explain this method. Reference scheme: (see Beckers et al., 2014)?

ll85: “This error field on the analysis is then compared to the error on the first guess” -> This analysis error is then compared to the first guess error

ll86: “namely the relative error field which thus consists in a score comprised between 0 and 1” unclear, if this refers to the first guess error or the ratio of the first guess to the analysis error or some scaled difference between the two. Please be more specific.

l88 would be the true field how can be know the true field?

l92: “The statistical parameters and the analysed fields masked when the relative error exceeds 0.3 or 0.5” awkward, please rephrase.

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l95: “gave” -> provided

l96: “from mid–2000s than previously” rephrase and fix grammar

page 6 l99: “uncertainties on the Atlas” uncertainties of the atlas data (not clear why you spell atlas suddenly with a capital A)

l99: “The BS has a varying data coverage” -> The data coverage in the BS varies from year to year.

l100: “relative” wouldn’t the absolute errors be more instructive? Now these are errors relative to very small temperature values (close to zero)

The entire error estimation is unclear to me.

l100: cut “BS”?

l102: “averaged on all layers” -> averaged over all layers

l103: minimum -> minimal

ll104: “For this reason, we decided to focus on the autumn only when considering the whole BS.” Make clear to which extent this is a limitation of your analysis.

l107: PSU there is not “PSU” and salinity has no units, e.g. absolute salinity has g/kg, but even “regular” salinity is unitless

l110: “here not taken into account” why not? Apparently a factor up to two is involved. Is that a problem? Having a converging lat-lon grid for such a small area is questionable to begin with. Why this choice?

ll113: “due to the cancelling effects from the increasing haline contraction and thermal expansion on density” -> due to the cancelling effects of increasing haline contraction and thermal expansion on density

l116: “at most” ???, the most?

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page 7 Caption Fig6: “Average relative error on the Barents Sea for temperature”

-> “Average relative temperature error in the Barents Sea” (and similar for salinity)

fig6 caption: “seasons” it is a function of time, not of seasons (the labels are years)

page 8 Fig7a caption “Average volumetric T-S diagrams during 1994–1998 and 2006–2010” is unclear, rephrase (the version in the text is clear)

l118: “it is clear the error” insert “that”

l119: “This strengthens the reliability of the observed T-S changes.” This is not clear to me, large uncertainties mean few data points, changes cannot be detected with few data points, so many changes may have gone unnoticed?

page 9 l128: we focus on the periods

l131: “One way of studying changes in temperature and salinity in the MRA is to look at the vertical dimension.”

I would remove this sentence. No additional information and the phrasing is not very “scientific” (e.g. you can “look at a piece of art”, or “look at me, when I am talking to you”, but I would study/inspect/analyse/take into account the vertical dimension).

l131: “Temporal . . .” The temporal evolution . . . is shown . . .

Fig. 9, 10, 11: consider a different presentation of the data, e.g. a Hovmøller-like plot as in Fig 12 and 13 (except depth on the y-axis), the current plots are difficult to read. Maybe you can find a good way of combining Fig 9 to 13 in two or three panels. Now they take up a lot of space for limited information.

page 9 l134: raise -> increase

l134: here and elsewhere: I am not a friend of abbreviations and I would consider spelling out Barents Sea every time you use “BS”.

page 10 Section 6.3 It is not clear that the volumetric changes in T/S and density

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provide new information over the profiles (it get's warmer, salinity is ambiguous and density doesn't change very much), so the use of this section is not clear (and this has implications for the title of the paper, so I would ask for a better explanation of the volumetric t-s diagrams, etc.)

eq(1) can only be a Ocean Heat Content (OHC) change, because deltaT is the change of temperature relative to a reference.

page 13 l150, 153: SI units are not supposed to be in italics

l153: "significant to the 95% level" not sure if this is the appropriate formulation

eq(2) [and to some extend eq(1)] what is delta s : s_ref-s? if so, then delta s/ (s+delta s) = s_ref-s/s_ref?

It is not clear what this EFWC is supposed to be. The proper (e.g. TEOS10) definition of freshwater is ocean-water minus salt (i.e. 1-s). In this sense, eq(2) can only be some fractional freshwater content (and just because it has been called freshwater before doesn't make it right). Because eq(2) depends on a reference salinity (the value of which is not even provided here), it is impossible to related the calculated numbers to anything else. Also the choice of reference (be it the mean as in your case or some arbitrary value) makes a difference in the time series. See Schauer and Losch (2019), their Figs3+4 for a simple illustration, also the discussion in Treguier et al (2014)

Similarly the OHC in eq(1) depends on the reference (and the units, do you use degC or Kelvin?). In the OHC case one can argue that everyone in oceanography uses degC and a reference of 0degC to compute OHC so that the ambiguity problem goes away (see McDougall, 2003, doi:10.1175/1520-0485(2003)033<0945:PEACOV>2.0.CO;2). Here the reference appears to be the mean temperature resurrecting the same problem as for the salinity anomaly/fractional freshwater.

161: SI units not in italics

l163: "For both OHC and EFWC trends significance, we followed the Fisher–Snedecor

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test described in Chouquet (2009) and Montgomery et al. (2012) augmented by a penalization of autocorrelation (Wilks, 1995)"

this information should have come earlier, also: "For both OHC and EFWC the significance of the trends was determined following . . ."

page 14 l165: any idea or comment why the salinity trend at BSO is opposite to "EFWC"? Maybe because of the minus sign in the definition? Wouldn't it make more sense to reverse the sign in the plot to illustrate the correlation?

This also goes back to my point of eq(2): Using salt content (integral over salinity) would be a less ambiguous measure and would yield itself much more easily to physical interpretation.

page 14 Conclusions

the conclusions are weak, but since this is a data product, there may not have to be strong conclusions about the physical interpretation. I would focus on the presentation of the data in the conclusion.

l169: on this part -> in this part

l169: "much" replace by "some" or remove

l170: "provided a variational method minimising the expected errors on the resulting fields is used" I don't think that this research shows that this method is required for the analysis. To be able to draw this conclusion I would like to see why it is impossible to extract physical information from sparse data without this interpolation method. Please rephrase.

Please also note the supplement to this comment:
https://www.earth-syst-sci-data-discuss.net/essd-2020-70/essd-2020-70-RC2-supplement.pdf

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