Reply to the comments by Reviewer 1, Matthias Tomczek

The paper uses the most comprehensive ocean database to objectively define source water types for the Atlantic Ocean.
The paper is put sloppily together and will require a careful reading by someone not too close to the original manuscript to iron out the many grammatical errors and other inconsistencies.

We have now rewritten the manuscript and did careful reading to iron out the many language errors.

I do not consider it the role of the reviewer to do that, but here are a few pointers as to what has to be done:
a) The reference "Schaffer, A.J., JACOBSEN, A.W., Mikulicz's syndrome: a report of ten cases. American Journal of Diseases of Children 34, 327-346, 1927" has no place in an oceanography paper. I assume that, in an effort to be as comprehensive on the history of water mass analysis as possible, the author meant "Jacobsen, J P. (1927) Eine graphische Methode zur Bestimmung des Vermischungskoeffizienten im Meer. Gerlands Beiträge zur Geophysik, 16, 404-412" and went astray in his Google search.

Thanks for pointing this error out. Reference now removed.

b) "The ocean is thus composed a large number of water masses" misses the word "of", and similar with many sentences.

Corrected (line 41)

c) "Source water type" is sometimes abbreviated as SWT, sometimes as STW.

We went through the manuscript and corrected to it now is always "SWT".

d) "Based on the work of (Pollard and Pu, 1985)" should be "Based on the work of Pollard and Pu (1985)", and similar with many citations.

We went through the manuscript and corrected the citations. (line 276)

Turning to the scientific content of the paper, it may be noted that there are basically two approaches to derive objective definitions of water masses and their source water types. Assuming no knowledge of the situation under study one might try cluster analysis and discover the relevant source water types from that. If, on the other hand, sufficient information is available to predict the source water regions with certainty one might proceed with an analysis of the parameter distributions in the preselected source water regions and derive the source water types from that. Given the large amount of information available for the Atlantic Ocean the paper rightly proceeds along the second path.

We agree with this assessment, and the intention of our work was not to redefine the water masses in the Atlantic Ocean, but rather to use the information already available and base the work on that. In several instances, water masses has in the past been differently defined and/or named so we tried to put this information in the paper as well.

Unfortunately, the paper does not fully grasp one of the basics of water mass theory, namely the distinction between water masses formed by subduction and water masses formed by winter-time deep convection. The water masses of the deep ocean are mostly formed by winter-time deep convection.

Source water types of the latter variety represent the conditions in their source regions during winter and can therefore be mathematically described by a single point in parameter space, with an associated variance representing interanual changes during water mass formation. The variable values given in Table 3 for the lower three layers can thus bee seen as a listing of the SWTs for the corresponding water masses, which are defined through single points in parameter space.
The water masses of the upper ocean (known as Central Waters), on the other hand, are formed by subduction over extensive ocean regions of the subtropics and therefore characterized not by points in parameter space but by usually nearly linear parameter relationships. This is clearly seen in Figure 3, where the TS-relationship of ENACW stretches from 10 to 15_C and 35.2 to 36.2 in salinity, or in Figures 5 and 6, which show similar TS-relationships for WSACW and ESACW. (This is even occasionally acknowledged in the text, for example in section 3.1.1.) To define these Central Waters through source water types requires two SWTs for each Central Water to define a linear parameter relationship, one at the lower end of the parameter relationship and one at the upper end. The principle is demonstrated in detail in Figure 2 of Tomczak (1999). The paper states "In this paper we use the concepts and definitions of water masses as given by Tomczak (1999)" but it does not appear to follow this through in full logic. It states when introducing the water masses of the upper layers: "The Upper Layer is occupied by four SWTs called central waters that are known to be formed by subducted [sic] into the thermocline (Sprintall and Tomczak, 1993; Tomczak and Godfrey, 2013) into the interior of the ocean (Pollard et al., 1996). Figure 2 illustrate [sic] a schematic of the main currents in this layer and the main formation regions of the central waters in the Atlantic Ocean. Water masses or SWTs in this layer can be easily recognized by their linear T-S relationship."

There is complete confusion between water masses and SWTs here. The correct description would read: "The Upper Layer is occupied by four water masses called Central Waters that are known to be formed by subduction into the thermocline (Sprintall and Tomczak, 1993; Tomczak and Godfrey, 2013). Figure 2 illustrates a schematic of the main currents in this layer and the main formation regions of the Central Waters in the Atlantic Ocean. Water masses in this layer can be easily recognized by their linear T-S relationship, and each water mass requires two SWTs to represent this relationship for a complete description."

Thank you for helping us out here with a clear path forward.

In the revised version, we made amendments to this aspect. **Central Waters** are redefined by their **upper** and **lower boundaries** by following the suggestions:

In the Section 3.1, distinctions between Central Waters and other deeper SWTs are pointed out. The most significant feature of the Central Water is that the characteristics cover a relative larger range and cannot be consider as a point value. As a result, upper and lower boundaries are set for each property of Central Waters and the values between them are all considered as SWTs of Central Waters in the further OMP analysis.
Take the potential temperature (θ) of ENACW as the example. After confirming the formation area (15°W—25°W, 39°N—48°N and 100 — 500 db), and the criteria (σ_θ between 26.50 and 27.30 kg/m^3), a relationship between potential temperature and density (potential density as the reference/independent parameter, detail in section 3.0) is plotted (blue dots) in Fig. 3 with linear fit (red line). The upper and lower boundaries in potential temperature of ENACW are finally determined by the choice of our own fit ranges (https://omp.geomar.de/), 14.60 °C as the upper boundary and 9.80 °C as the lower boundary. All the other properties are done with the same method and the boundaries are listed in Table 3.

In the figure plots for other deeper water masses (for example Fig. 9 for AAIW) statistics are done for all the data in the selected regions with selected criteria. For the Central Waters, statistics are done, considering all **the data between upper and lower boundaries**, and then, all the data between the boundaries are considered as one Central Water Mass and plotted in the following figures (for example Fig. 4 for ENACW). In the figure plots, Central Water, for example ENACW, is still plotted as **one water mass**. The reason we plot figures in this way is hoping to show the reader that one Central Water, although the properties cover certain ranges and have upper and lower boundaries, is still **one water mass** instead of two.

The problem of the Central Waters continues right through the paper. If the criteria used to define ENACW pick up all data points along the linear parameter relationship of Figure 2, what is the meaning of the variable values given for ENACW in Table 3?
Those values do not describe a parameter relationship but a single parameter point in space not representative of Central Water. The representation of the Central Waters in Figure 22 also does not correspond to the definition ranges shown in Figures 2, 5 and 6. Central Waters should show up in Figure 22 as TS-lines, not TS-points. (As an aside, I assume that "F ISOW" in the figure caption should read "K ISOW".)

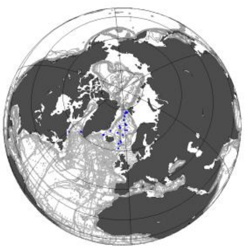
However, in the further OMP analysis, one Central Water **occupies two positions** of SWTs (upper and lower boundary respectively). In other words, upper and lower ENACW for example are considered two independent SWTs in the calculation of OMP analysis, but the final output in the figure plots (for example, in Fig. 4 for ENACW) is still **one water mass**, which means that all the data within the range are considered to be this central water mass.

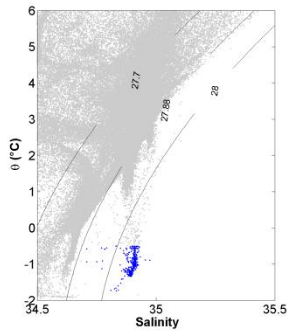
To summarize the review up to this point, the paper has to address the description of Central Waters afresh and make a clear distinction between subducted water masses and water masses formed by deep convection.

We hope that the above modifications we have made can avoid the previous mistakes and also avoid the misunderstandings or confusions to the readers, and also conform to our original intention of quoting the literature for example, Tomczak (1999).

There are other points of a minor nature that nevertheless require attention. Figure 22 shows a sequence of data points near 34.8 salinity reaching down to temperatures as low as -1.5_C. Such data points cannot be produced by mixing between any of the SWTs shown, so they represent an additional SWT. At least a remark is required what these data points stand for and why they are not considered further.

Thanks for pointing out this detail that we overlooked in previous version. This water mass indeed cannot be explained by the mixing of any above listed original water masses. So a new SWT should be defined. By analyzing of their distributions (blue dots in the left figure) on the map and their depth (between 2000 and 4000m), and also combining with temperature and salinity data (right plot). The properties are closer to Eurasian Basin Deep Water (e.g. Bönisch and Schlosser, 1995; Schauer et al., 2014). As a result, we preliminarily determined this water mass as from the deep Arctic (Eurasian Basin). Since this water mass originate from the Arctic Ocean, and the amount of data is too small (only 600 samples), and further there is not much interaction with other listed water masses, we did not list it as an independent water mass in this paper and also no SWT is defined so far. Thanks again for pointing out this point and also provides guidance for our next work. We explain this point in the new manuscript.





The paper states "Mode Waters, on the other âˇA´lhand, are considered as the precursor or the prototype of the central waters." This is not necessarily so. The quasi-linear parameter relationship of Central Water does not relate linearly to depth. Some depth ranges within the Central Waters display rapid parameter changes with depth, others show particularly uniform properties. Mode Waters are sub-regions of Central Water; they describe such layers within the Central Water range that display particularly uniform properties.

Finally, in a combined TS-diagram of data from a region SWTs are normally identified as the extreme TS-points that cannot be produced by mixing with other water masses. The definition points for AAIW in Figure 8 clearly include TS-combinations influenced by admixtures from NADW and even Central Water. A definition of AAIW based on its source region will have to concentrate on the low salinity points in the 0 to 2_C range. Some commentary is necessary to justify the selection of definition points in Figure 8.

In the previous version, Mode Water is considered as the precursor or the prototype of the Central Water. Such a statement is indeed not rigorous. Thanks for pointing out this key point and in the new version, such description is used. Mode Waters are described as **sub-regions** of Central Waters and also report the most significant characteristic of Central Waters: the linear T-S relationship.

Reply to the comments by Reviewer 2, van Heuven

Liu and Tanhua present an enumeration of statistical properties of selected sets of seawater samples, thereby defining ’Source Water Types’, likely for further use in Optimal Multiparameter analysis (OMPa) in a separate manuscript. Although of laudable intentions (having ’true’ or ’universal’ SWT definitions would make many a PhD student’s life easier), I do not see why this paper should be published in its current shape – or at all.
My main criticism is, in increasing order of importance:
A) The ms. has not been carefully proofread, and glaring oversights remain.
B) Fundamental concepts in water mass analysis appear lost on the authors, rendering the findings of reduced usefulness for use by other investigators, who may work with adifferent conceptual framework of water mass analysis / OMPa.
C) The findings are trivial, and possibly not application-appropriate. (although I did not read the companion manuscript).
The large and rather thorough central portion of the manuscript may have merit as a review of literature on Atlantic oceanography – however I do not consider myself qualified to judge whether it may hold value over existing work in this regard.
I can imagine this entire paper to constitute, in severely condensed form, the first three paragraphs of a/the application paper. Almost all figures may then be moved to supmat.
Below, I’ll list for each of the above categories — non-exhaustively —- some illustration to my criticism stated above, and provide further general commentary.

*** A) "The ms. has not been carefully proofread, and glaring oversights remain".
Writing is worryingly sloppy. References are made to obviously wrong papers.

We examined the references carefully and corrected the misquoted references.

Abbreviations are jumbled.

We went through the manuscript carefully and corrected all the abbreviations to the right way of expression.

Example: – For GLODAP, reference is made to Lauvset et al (of the excellent mapped product). Given that the (bias-minimized) original bottle observations are used, reference should be to Olsen et al. That paper contains a fully self-contained explanation of the employed QC methods. This makes reference to Key et al., 2010 superfluous. Given the second authors exceedingly heavy involvement in these earlier publications, this slipup is surprising.

We read the methods section of Olsen et al., (2016 and 2019) carefully, and make changes in the description of QC part in the new manuscript (section 2.1).

Phrasing is occasionally imprecise and terminology (e.g., "variable", "value", "definition" etc.) is used inaccurately.

We unify the relevant expressions in the whole manuscript. For example: the word "**characteristics**" is used to describe the overall water mass (for example: characteristics of water masses); "**property**" is used to describe one special features such as oxygen (for example: six key properties); the word "**variable**" is not used in the relevant description in order to avoid confusion; "**value**" is used to show specific mathematical value; "**definition**" is used to show concrete concept.

Please carefully re-read. As an example: the caption to Table 1 now reads: "Table 1: Table of all the water masses and the four main layers as defined in this study. The variables defined are used to select water samples that defines water masses in the formation regions." but perhaps, much less ambiguously, should read: "Table 1: Summary of the criteria used to select (from GLODPAv2) the water samples considered to represent the source water types discerned in this study. For convenience, they are grouped into four depth layers."

Thank you for helping us out here with a clear linguistic organization. In the new manuscript, we try to use clearer expressions to make the reader understand more clearly.

*** B) "Fundamental concepts in water mass analysis appear lost on the authors, rendering the findings of reduced usefulness for use by other investigators, who may work with a different conceptual framework of water mass analysis / OMPa."

For instance, already the initial review section betrays a lack of understanding of the fundamentals of OMP. Line 67: "SWTs describe the original properties of water masses in their formation area, and can thus be considered as the original form of water masses (Tomczak, 1999)" is incomplete. For water masses defined as originating from a single SWT, this is correct. However, for WMs defined as being on the mixing line between two distinct SWTs (i.e., central waters), the statement is incorrect.

Thanks for pointing out the mistakes in our previous work. We read the reference (Tomczak, 1999) again, and indeed, Central Waters, which are formed by subduction through the thermocline and into the interior of the ocean, need to be treated differently from other deep water masses due to their different formation ways. In Section 3.0, this difference is pointed out.

Throughout the text, the terminology "SWT" and "Water Mass" is mixed up. The authors appear to not be aware of (or to subscribe to) the very specific, and non-identical, definitions of SWT and WM as provided by Tomczak 1999 (although that paper is cited).

A new section (Section 2.2) is added to distinguish the differences between "Water Mass" and "SWT". In general, a "Water Mass" is an objective entity with temporal and spatial distribution; while a "SWT" is a set of mathematical values that describes the original properties of a "Water Mass".

Rather, the terminology is used loosely, often incorrectly, and certainly confusingly. Tomczak states (paraphrasing) that "a water mass may be defined as either a point in parameter space, or as a line between two such points". All water masses discussed in the manuscript as treated as point sources, while in wider literature many (notably the Central waters) are generally considered to be "line sources". Please re-read Tomczak 1999 and follow its protocol. For highly relevant examples of what a hierarchy of water masses and their constituent water types could look like for the Atlantic, again, consider [Middag et al., ESPL 2018] or the chapter 7 of the thesis of Van Heuven, 2012.

Thanks for the guidance in distinguishing Central Waters and other deep masses.

In the revised version, we made amendments to this aspect. Central Waters are redefined as a line between two points, which are the upper and lower boundaries. In the Section 3.1, such distinction between Central Waters and other deeper SWTs is pointed out, and all the 6 key properties of Central Water, which cover relative larger ranges and cannot be consider as point values, are redefined by upper and lower boundaries.

In the figures (Fig. 4-7), all the data between upper and lower boundaries are plotted and the Central Water is still presented to the readers as one whole water mass. In the form of figure plots, Central Water has no difference to other deep water masses. The main difference is mainly reflected in the

OMP analysis, the central water masses occupy two "positions", which are the upper and lower boundaries and the values between them all considered as SWTs of this Central Water.

For example, in Section 3.1.1, the potential temperature (θ) of ENACW has clearly two boundaries. The upper and lower boundaries in potential temperature of ENACW are finally determined by the choice of our own fit ranges (https://omp.geomar.de/), 14.60 °C as the upper boundary and 9.80 °C as the lower boundary. All the other properties are done with the same method and the boundaries are listed in Table 3.

*** C) "The findings are trivial, and possibly not application-appropriate."
Effectively, the paper is an enumeration of means and standard deviations of properties of seawater samples encountered in (slightly arbitrarily drawn) multidimensional boxes in the ocean.

We agree with this assessment, and the intention of our work was not to redefine the water masses in the Atlantic Ocean, but rather to use the information already available and base the work on that. In several instances, water masses has in the past been differently defined and/or named so we tried to put this information in the paper as well.

Such defining of SWT properties would likely be re-performed by any investigator of Atlantic water masses. Presenting them here thus has little added value for the community. (exceptions may be long-term tracing of changing Atlantic water mass distributions, always employing the same SWT definitions. Such a ’climate change’ application though, would require a less subjective or circular approach to the defining).

The exact SWT definition used in a particular study may be very much application-dependent. E.g., where this study employs ’formations region’ SWT definitions, other studies (e.g., Middag et al., 2018) employ ’edge-of-section’ definitions for the water masses that have not been sampled at their formation regions. Other ’ad hoc’ definitions may be envisaged, and may be equally valid for the application at hand. That is, the definitions are subjective. Likely, for high-detail application, the results presented in this manuscript are too general. Conversely, for largest-scale application (i.e., basin-wide OMP), much more coarse approximations of the SWT’s may suffice (see for example Middag et al., EPSL 2018).

We disagree with this statement, at least partly. It is true that several investigators will go through the trouble to define their source water properties themselves, and possibly use “edge-of-section” data. We think that most do that since finding a stringent water mass definition is cumbersome. We provide in this work a comprehensive characterization of water mass properties that can be used by investigators. Obviously, the investigators focusing on a particular water mass might want to be more precise, and possibly look at temporal evolution etc. This paper is not intended for those, but rather for the chemical/biological oceanographers that would like to understand (roughly) the formation history of the water they sampled.

We explicitly aimed for being general and course in the WM characterization, realizing that “sub-water-masses” can be defined, and that spatiotemporal variability do exists.

*** Assorted commentary, in no specific order

– Although I'm a great fan of GLODAPv2, I do not see demonstrated that that data product is "uniquely ideal for use for SWT definition" (paraphrased from LINE 99), given its limited physical oceanographical detail. While GLODAPv2 is a very good biogeochemical data product, it features limited vertical resolution (vs. CTD), rather lax accuracy constraints for the exceedingly precise measurements of S and T. Without further corroboration of this statement, it is not evident why Gv2 should serve this purpose better than any other dataproduct that features S, T and O. Please elaborate. (Evidently, there may be additional value in having colocated values for N, P and Si, but I'd wager that will prove of little discriminating value for OMPa).

Point taken. I guess other products with biogeochemical data would do the work. The main advantages of GLODAPv2 is the internal consistency of the data, so that we can use a large set of cruises and have some confidence that the data are consistent, which might be useful for water mass analysis. We do agree that high-resolution CTD profiles would be useful, for instance one can use potential vorticity as a parameter. Since our intended main audiences are chemists and biologist with sparse vertical resolution of their data (mostly), this work corresponds to that need and the expected data availability.

– The CDW in the Weddell Sea, I believe is more commonly locally referred to as the "Warm Deep Water". It evidently does not include freezing waters. However, such samples are visible at 34.65/-1.9 in Figure 20 (likely located on the continental shelf of the Antarctic Peninsula). Please select more carefully – these skew your averages...

The selection criteria have been adjusted and data with low temperature (below 0 °C) are removed.

– Table 3: What is the use of stating "potential density" statistics (particularly for deep and bottom waters)? These would not likely be used for OMPa, because no information is contained additional to what is already contained in S and T. Also, they are formany SWT's pre-described through the sample selection criteria, so this is constitutes a rather circular result.

We agree, potential density is calculated from T and S, and is not an independent property of a water mass. So column of "potential density" is removed and Table 3 shows only 6 key properties of the water masses.

– Line 34: change "sea water type" to "source water type".

The words "sea (source) water type" have been removed, since in this paragraph the concept of SWT is not referred yet and we are still talking about Water Mass.

– On conceptual grounds I have some trouble with referring to CDW as an SWT. Rather, it may be considered to be an aged mixture of the other SWTs presented. Large-scale (extended-)OMP-analysis that considers CDW will prove unusably under-constrained for samples from the CDW (i.e., such samples might be found to consist of 100% CDW, or of 33% of each of the other SWT, or any possible combination between). (However, OMP users may obviously choose to not include CDW an a candidate SWT). Same goes for "North East Atlantic Bottom Water". That water mass is not "formed" (F19 even mentions its "formation region"). It is merely AABW that flowed there, aging, and with admixture from (already defined) deep water. This is an intermediate to other more extreme STWs, that would already be accounted for in OMPa. Obviously, for a local study of, say, NE Atl. water masses, the NEABW SWT may be used, but please refrain from using "formation region" in its context.

Indeed, some water masses are not original water masses, but products of spreading and mixing, for example, NEABW. Because of their special properties and may be useful for region-specific studies, we hope to still keep such water masses and consider their SWTs. In terms of expression, we distinguish such water masses from ''original'' water masses, such as AAIW. We only show the key properties of such water masses in specific region, while the definition "formation area" is only for the ‘‘original’’ water masses.

– Line 82: "our analysis is relatively course" ==> "coarse"

Changed into "coarse", new version in line 87

– I do not clearly see what the role is of the 4 density intervals discerned in this study? Are they merely to steer the reader’s eye? Or are they used as additional boundaries to the vertical extent of selections of samples? Whatever the case, I believe that the two conceptually different ways of separating water masses by means of (i) OMP and (ii) density intervals are not necessarily compatible, and that these two methods should ideally not be mixed within a single paper, to avoid confusion.

The OMP method is the only criterion for distinguishing water masses in this study. However, due to the excessive number of water masses, our current OMP analysis can only calculate no more than 6 water masses within one OMP run, so the ocean is divided into 4 layers according to the density (in total 13 OMP runs, divided by density and latitude as shown in the following table), so as to ensure no more than 6 water masses in each OMP run. Therefore, density can also be considered as an additional boundary, which will be further clarified in this article so as not to confuse the readers. (Table 1 in the manuscript)

– Line 120: "for some SWTs, key properties such as salinity, oxygen or silicate are also necessary". It may warrant some discussion as to why this does not constitute circular reasoning. For example, if one pre-defines the S-range for the samples, then the resultant average S is of little intrinsic value! This is pertinent for example to the definition of MOW ("36.35-36.65"), which in reality has salinities well beyond the statedrange.

The criteria in Table 1 are listed according to historical literatures. Based on these criteria, we selected eligible data from the GLODAP dataset, and then make statistics on these eligible data to obtain a result (Gaussian distribution), and these results (or values) are considered as the basis for our definition of SWT. Therefore, besides the area distribution (latitude, longitude and depth), it is also necessary to list some criteria in Table1, because there may be other water masses in the designated area, and we need to use these criteria to eliminate the interference of external water masses.

– Most (panels of most) figures should be moved into supmat. Please maintain only an interesting subset of figures for the main ms.

This is a good point. We do think that the figures are an important part of the paper. Now, particularly since we have merged the two manuscripts into one, we do have an exceedingly large number of Figures. We will carefully make a selection to which figures will be moved to supmat. Thanks for the suggestion.

– Figure 1 – this cruise is not drawn in on the map. Consider plotting it in F2. You can’t expect readers to google the cruise track themselves.

A small figure (map) is added to show the cruise.

– Figure 22: no characters in circles in legend. I believe it to be a shame that the paper does NOT present alternative property-property presentations of the definitions derived. Possibly, nothing beats a set of theta_vs_x plots (or similar) for visualizing the multidimensional separation of the various SWTs. For inspiration, please refer to, for instance, figure 7.5 in the thesis of van Heuven (2012). Also, from F22 I recon that there’s some extremes of the samples in the S-T diagram that are not accounted for by any watertype. For example, in F22, panel B, the few hundred (?) samples at -1/34.9 have no closely associated SWT. These samples – while indeed part of the Atlantic Gv2 – are located in the Norwegian Sea, which is not covered in your work. Remove these from the figures to improve legibility and aid understanding. Also, many hundreds of samples are located at salinities well below that of AAIW (several standard deviation of the AAIW SWT definition). Are these surface samples? If not, how would these ever be represented accurately in an OMPa? For an example fix, please consider the SWT definitions in figure 7.5 of van Heuven (2012).

We have made corresponding modifications and adjustments to the figure as suggested.

– Line 128 "[...] the standard deviation of the distribution (the amplitude of the curve defined as 2/3 of the highest bar)". This does not ring a bell as being a definition of standard deviation. Please rephrase.

Description is reorganized, in new manuscript line 228.