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Interactive comment on "Water mass distributions and transports for the 2014 GEOVIDE cruise in the North Atlantic" *by* Maribel I. García-Ibáñez et al.

Anonymous Referee #2

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Good day,

The first reviewer has already provided a summary of the paper, so I will just go to straight to my points. However, everything else I say below solely reflects my opinion and view on the complex process of water mass formation and variability in the North Atlantic.

The issues the authors address in the paper are highly relevant and important for water mass analysis and prediction of their changes over time, and dissecting transformation and mixing of water mass is a big and nontrivial problem overall, so any novel solid approach and a study based on it would be much awaited here.

However, I cannot understand how a trans-Atlantic snapshot (not to mention that the

section does not end in St. John's, Newfoundland) and a simple model operating with only four members at once can be used to depict complex interaction and mixing of 14 water masses. I am not in position to judge the previously published paper of the same authors that is used as a basis for the current one, but if I had to review it, I would come with critical suggestions pretty much similar to those presented below.

Let me explain why I believe that a four-member approach does not work for this specific task:

(1) First of all - the case is not two-dimensional (2D distance along section vs depth). The water masses interact in over the entire subpolar North Atlantic. So, for example, any two waters appearing as neighbors on the OVIDE line may be separated by other waters elsewhere in the region. Therefore, the only way to solve this problem for the subpolar North Atlantic and its water masses is through solving a full system of equations where each end-member is carefully defined, and this creates another challenge.

(2) Now, a whole list of problems concerning the end members: a) The authors use end-member properties as they appear on a snapshot of an arbitrary section line (OVIDE or any) and not the properties of the studied water masses that these waters acquired at the times of their formation. Most critically here, both DSOW and ISOW should start from sub-zero temperatures. Both ISOW and DSOW are equally fresh the sills. However, ISOW gains its salt through mixing as it spread through the Iceland Basin. So taking the water that is already salty is not good for telling how it was formed from start – note that it has already been mixed with SPMW. Same is true about the other waters. (b) By no means, LSW remains undiluted between Labrador Sea and Iceland Basin. However, Figure 4 suggests 90% of original LSW in any other LSW all the way through the region. Well, the Labrador Sea is a very powerful engine, but can it pump so much water that stays unmixed for so far and so long? (c) The depth of LSW was not 2000 m in 2014, and there cannot 50% of LSW at 3000 m - at the depths where water is already as saline as ISOW modified through entrainment.

(d) Then, ISOW is fresher in Labrador Sea than in the Irminger Sea, because it is more diluted, but the corresponding fractions seem very much comparable in Figure 4. Does ISOW really reach 2000 m in the Labrador Sea adding about 50%? Or is it something else? How can we be so sure that another water mater contributing to the mid-depth exchanges and arriving from outside the Labrador Sea is not missed in this formulation? It must be something else rather than 50% of ISOW. . . (e) I totally agree that a more careful approach is needed for the two chemical variables used in the work. However, using a certain universal model for utilization may lead to overconsumption of oxygen at greater depth. I say this, because the oxygen section suggests weak biological utilization, whereas applying parameterizations used in biochemistry (I cannot expand further here, but any quick assessment would show a comparable result) would reduce dissolved oxygen more than what we see in the section. If we assume a strong bio-consumption, then how would we explain that dissolved oxygen closely follows salinity which in turn is not altered by living organisms?

So far I was talking about using static end members assuming the picture does not change with time. But there is another set of complications coming into play if we introduce temporal variability of water properties. Yes, the source waters change in time, but any static model assumes invariance of the source waters. How long does it take for LSW to cross the basin? Let's say N years? How would the authors introduce the temporal changes previously observed in the source or sources of LSW? Note that convection was not strong in 2010 and 2011, and that it was that water that had probably been seen in the Iceland Basin in 2014! LSW does change a lot in its source in 3-4 years. How would this knowledge be transpired into 3.00 and 34.87 with such narrow error bars? At the season of formation the waters are even more different. Oxygen saturation is probably >95%. Taking the transit time into consideration, the version of LSW seen on the OVIDE line in the southern Labrador Sea may not be directly related to that transferred to Iceland basin first through DWBC and then under NAC ... The properties of the original waters can be much greater than the error bars used through the work. I bring LSW only as an example but the same may true about

other waters brought into the equations.

Is it really true that DSOW has no LSW mixed into it? I find it strange because in the northern Imringer Sea DSOW is cascading down the slope entraining both NEADW (ex-ISOW) and LSW and warmer waters.

The Monte Carlo technique would only help if the errors were random respecting central tendency. I have no doubt that each of the linear 4-member solutions would converge even with larger seeded errors. However, the present case is subject to more systematic rather than random biases, raising a question like "How would each solution change if LSW was 0.3 warmer at time of formation?"

Saying that the task of unscrambling water mass composition in this highly dynamic and variable area is well worth pursuing, I, unfortunately, cannot agree that the presented method, data and results help much solving this task. There must be a solution, but based on a more extensive synthesis of three-dimensional (3D) data, on a proper definition of source waters and their changing properties, on accounting for multiple pathways, etc.

Concerning the transport part ... The water mass transport and transformation are two related problems. I don't think a simple geostrophy (note a coarse grid in the Irminger Sea and missing profiles in the western Labrador Sea - both are important for budgeting the fluxes) is sufficient for constraining the transports. Frankly, I would not even bring the transport part in the work discussing the contributions of source waters. I think the most important part for now is building a method adequate for the task and thoroughly investigating every aspect of interaction by taking into account a huge baggage of what is known and available to this date and developing something better than a static 2D approach for analysing a strongly time and space variant 3D dynamics and variability – essentially 4D.

To conclude my review I share my thinking of this problem as cookbook analogy – all we try to come up here with is a recipe. Think of real ingredients and not those appearing someplace somewhere – if you use the latter, the results are not going to tell much about your true ingredients. On the other hand, by weighting the real properties of the waters with the sought and found fractions, one should come to a section plot similar to that observed.

Considering the amount of data, effort and work needed to address the issues I raised in my review, I recommend rejection. This only reason why it is not revision is that by redoing the paper the authors would come with a totally new method, sets of results and visions. Sorry, but I cannot see it any simpler than that.

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