

Interactive comment on “Atlantic meridional overturning circulation at 14.5° N and 24.5° N during 1989/1992 and 2013/2015: volume, heat and freshwater fluxes” by Yao Fu et al.

Anonymous Referee #1

Received and published: 17 December 2017

I have looked at the work with thoroughness and consider that the authors have made an enormous effort to carry out a series of analyzes and generate significant knowledge about the Atlantic Meridional Overturning Circulation (AMOC). Despite these remarkable efforts, I can not recommend this article to be published in its current state. I believe that it is convenient to encourage the authors to review their work and introduce, if they deem it convenient, a series of modifications to make it more robust and ready to be finally published in your journal.

The authors essentially compare volume transports from four zonal hydrographic sections in the North Atlantic, performed with a time difference of 23-24 years, trying to

C1

check if in that period the AMOC has intensified or weakened. To do this, they first analyze the distribution of water masses present in the study area and finally use an inverse model to estimate mass transport.

The main problem I see in the analysis presented is that the authors use for the 1989 section, made at 14.5°N, a reference level different than that used for the other sections. In particular, the reference level chosen is the neutral density (γ_n) of 27.82 kg/m³, located approximately at 1500 m depth. For the other three sections, they place the reference level at a much greater value ($\gamma_n = 28.1295$ kg/m³), located around 4000 m depth. The problem of placing a level of no-motion in the middle of the water column is that the values around that level of no-motion will also be close to zero, reducing notably the mass transports at those levels but allowing the values located further in depth to increase to unrealistically high values. It can be justified that different reference levels are used for each section, of course, but in this case in which the authors want to compare two sections made at different times, it would have been desirable for the reference level to be the same, so that the differences observed are due to the natural variability in the system and not to an artificiality introduced by the authors.

The inverse model is applied and it could introduce velocities in the reference level, but it actually doesn't. As it can be seen in the Fig. 8a, the reference level at the stations located in the interior ocean is practically 0. And in Fig. 10a, the no-motion level is clearly located at roughly 1500 m depth. So the initial choice of the reference level is relevant in the remainder of the paper, despite the usage of the inverse model.

If the authors insist on maintaining the current reference levels, it would be convenient for them to carry out some additional analysis in which they show that their results are not altered by the choice of the reference level. In particular, I would like to see what would have happened if the reference level of the section at 14.5°N of 1989 had been made with the deep reference level ($\gamma_n = 28.1295$ kg/m³) or if the section at 14.5°N of 2013 would have been carried out with the shallow reference level ($\gamma_n = 27.82$ kg/m³).

C2

Comments in detail I would like to make some additional comments: 1. page 1, line 28. The authors' description of the formation of NADW is perhaps too simple, since not all the heat from the south goes to the formation of NADW but much of it is aimed at heating the climate of Europe. In any case, a reference is needed.

2. page 2, line 22. What type of changes? Changes in the formation rate? Changes in its thermohaline properties?

3. page 3, line 11. Why could it be problematic? Because of the different technology employed?

4. page 3, line 15. Please, before describing the structure of the paper, its main goal should be more explicitly indicated.

5. page 5, line 8. The authors describe a treatment performed on the dataset to interpolate and smooth the data for the sections at 14.5N. Could they provide a sensitivity analysis related to this treatment?

6. page 5, line 18. Could the authors also give more transparency to the analysis performed on the oxygen data?

7. page 6, line 33. There is a huge dispersion in the observations at those depths. Could the authors show if the differences plotted are significant?

8. page 7, line 8. At the eastern part of the North Atlantic, the gyre is not so deep to force the variability observed for the water masses at intermediate levels. The authors should consider rewriting this sentence.

9. page 9, line 1. Δx_j is presented outside the integral. That would be right if the distance between stations would be regular, which is not the case. The authors should consider writing that element within the integral, as they did in Eq. 12.

10. page 9, line 15. The authors state that they have 51 unknowns. As far as I understood, the inverse model adds 17 unknowns as dianeutral velocities, which are

C3

those later shown in Figure 9. The authors should consider rewriting this sentence.

11. page 10, line 5. The authors argue that they must modify meridionally the value of the transport at the Bering Strait (0.8 Sv) that has widely being used to constrain the net transports in zonal sections of the Atlantic Ocean. They state that, otherwise, they would be ignoring the freshwater flux from/to the atmosphere. So, if the freshwater flux between 14.5N and 24.5N is a positive value of 0.3 Sv to the atmosphere and the net balance at the zonal section at 24.5°N is 0.8 Sv, then the transport at 14.5°N should be 0.5 Sv. In their argument, the authors are somehow ignoring the freshwater flux in the basin north of the section at 24.5°N, which is unlikely 0. Not only Hernandez-Guerra et al. (2014) but also Ganachaud (2003) consider that the Bering Strait transport must be used in all the sections of the Atlantic Ocean. The authors should consider reviewing their approach in this point.

12. page 10, line 25. The sentence 'Given the much larger number of unknowns...' could be expanded with '...and the linear dependency between equations...'.

13. page 11, line 28. Please, consider producing a picture to explain the method to reference the velocities to those retrieved by the LADCP.

14. page 12, line 30. The authors should justify their choice for those uncertainties.

15. page 15, line 20. In Figure 10, the authors present the transports integrated along the layers. A third curve is given, which seems to be the imbalance between the other two curves. However, if we take a close look at the curves, we can see that the imbalance presented is a much smoother curve than the one that should appear after finding the difference between the other two curves. I would ask the authors why the imbalance curve is so smoothed, and why it doesn't seem to be related to the other two curves. In the left panel of this same figure, please, consider shifting the layer number so it is finally in the layer instead in its interface with the next layer.

16. page 16, line 5. The authors consider here that a difference of 1.4 Sv is '...very

C4

close to each other', but in the previous page, on line 27, they consider that a difference of 1.6 Sv '...is considerably weaker'. They probably should rewrite those sentences.

17. page 17, line 12. I would suggest the authors to include a figure showing the velocities in the FC, as they did with the Figure 12.

A few typos 1. page 2, line 12: '...in detail...' 2. page 2, line 23: '... were occupied' 3. page 3, line 20: '... are given...' 4. page 3, line 24: please, expand here MAR. It's done later, on page 7, but this is the first time that MAR is used. 5. page 5, line 13: '(Figs. 3e-f and 4e-f)'. 6. page 7, line 4: please, expand SAM. 7. page 7, line 20: '...originated...' 8. page 15, line 1: 'Note the western...' 9. page 15, line 17: '...only exists...'

Interactive comment on Ocean Sci. Discuss., <https://doi.org/10.5194/os-2017-87>, 2017.