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Interactive comment on "The GEOVIDE cruise in May–June 2014 reveals an intense Meridional Overturning Circulation over a cold and fresh subpolar North Atlantic" *by* Patricia Zunino et al.

Patricia Zunino et al.

pzuninor@ifremer.fr

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Dear editor and referees,

We were grateful to receive the very constructive reviews of our paper "The GEOVIDE cruise in May-June 2014 reveals an intense Meridional Overturning Circulation over a cold and fresh subpolar North Atlantic". Thank you very much to the 3 anonymous referees. We will incorporate in the manuscript the majority of their comments and we think the scientific results will be better exposed than in the first submission. Before dealing with the referee comments in detail, we wrote an answer to a concern that is common to the three reviews: the misunderstanding about the timescales dominating

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the cooling and freshening of the subpolar North Atlantic. Following, we answer point by point each comment of the three referees.

One result of our paper is the co-existence in May – June 2014 of the cooler and fresher eastern subpolar North Atlantic (SPNA) in relation to the mean 2002 – 2012, and the relative intense Meridional Overturning Circulation and heat transport across the OVIDE section. In the region delimited by 40°N – 60°N, 45°W – 10°W, the evolution of both (i) ocean heat and freshwater content in the upper 1000 m and (ii) air-sea fluxes of heat and freshwater since 2013 reveals that the atmospheric forcing is mostly responsible for the strong TS anomalies of 2014. However, as pointed by the three reviewers, we did not discuss the decadal context of our observations and missed to refer to Robson et al. (2016; 2017) who identified a new cooling period in the subpolar North Atlantic starting in mid-2000, a cooling period affecting at decadal time-scale.

In our revision, we will include a new figure showing the evolution of heat content in the upper 1000m of the region 40°N – 60°N, 45°W – 10°W (see Figure 1 in this document). Based on this figure, that shows a long term heat content decrease starting in the mid-2000s, we will be able to illustrate Robson et al. (2016; 2017). We also observe intensification in heat content decrease from 2013 to 2014, just the episode we discuss in our paper. Thus, the 2013-2014 cooling episode is inserted in the cooling at longer period of time detected by Robson et al. (2016; 2017). We show in our paper that the former was dominantly produced by the local atmospheric forcing over the 2013-2014 winter. Our results are in agreement with a recent paper, Frajka-Williams et al., (2017, in Scientific Reports): they exposed that the rapid cooling registered between 2013 and 2015 in the subpolar North Atlantic was explained by the atmospheric forcing since the effects of a MOC slow-down at 26°N is too slow to explain the observed rapid cooling.

Following this explanation, the third paragraph of the introduction will be restructured and the references to Robson et al. (2016-2017) added. The discussion 4.2 will also be expanded in order to place our observations in the context of a longer time scale. The figure 1 in this document will be inserted in the new ms.

We copied the referee comments in this document in blue font followed by our answers in black font.

Anonymous Referee 3

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In this manuscript the authors present the results from analysis of a 2014 CTD section taken along the OVIDE section. The cruise was a contribution to the GEOTRACES program and so the results of this hydrographic analysis will allow analysis of the extensive chemistry data also collected on the cruise. The authors describe the OVIDE section from the cruise data, place it in context of data along the OVIDE section from 2002 to 2012, and attempt to explain some of the differences.

The paper is a good, straightforward description of a cruise data set, and gives a highly valuable look at the MOC and gyre circulation of the eastern subpolar North Atlantic in summer 2014. The text is well written and the figures are relevant and mainly well presented. The weakness of the paper lies in the authors attempt to understand the reasons for the cooling and freshening they observe in the 2014 section eastern basins compared to the earlier data. The introduction is a little muddled on this topic, and the conclusions that they draw from their analysis are not as robust as they could be. Below are some comments that I hope the authors can use to improve that aspect of the paper, as well as some minor edits.

1. Timescales of change. The authors have missed a key aspect of the literature on changes in the subpolar North Atlantic, and that is about timescales. If you could rewrite the introduction considering the timescales of each of the papers that you cite it would help you focus your own analysis. In short, the arguments for ocean heat transport convergence being the primary control are all on long timescales – multiyear at least, certainly decadal. The "cold blob" analyses and the evidence for air-sea fluxes are all about short timescales - seasonal to a year or two. You should also consider the possibility that temperature anomalies and salinity anomalies may have some different

forcing mechanisms on different timescales. You may not have enough data to look at long-term changes, and a focus on the short term may be more appropriate with the analysis that you have done already.

Yes, as indicated in the introduction of this document, we agree that the mechanisms controlling the heat and freshwater content changes in the ocean at different timescales were not well exposed in the manuscript. We will reorganize the last part of the introduction and the last part of the discussion to account for this. We will put the "inter-annual" signal of 2013-2014, which is an intensification of the cooling already started in mid-2000s as documented by Robson et al. (2016;2017), and shown in this manuscript to be dominantly caused by the air-sea flux.

2. Methods. I realize that the authors are using well-developed methods described in earlier papers from the group, but a little bit more information would help the reader understand their method. In particular I was not sure whether the SADCP data are used in the inversion. I had thought they were, so it is surely not a surprise that the main features in the SADCP data are also seen in the solution?

Yes, as it is indicated in the manuscript in line 144 – 145, the SADCP data are used to constrain the inverse model. To make it clearer, just after the first sentence, we will add the sentence "Before inversion, the S-ADCP data were averaged between stations in layers where the velocity vertical shear was consistent with the geostrophic velocity profiles". The referee is right: it is not a surprise that the main features in the SADCP data are also visible in the inversion solution. However, we wanted to show how the small-scale features visible in the SADCP data were averaged in the inversion because of the coarser resolution of the hydrographic profiles (lines 161-163).

I felt there should be more information about the reduced resolution of the CTD spacing in 2014. You say in lines 123-124 that you will later show that the features were "correctly sampled", but your evidence for this in lines 386-387 seems to be just that all the circulation features are identified (Table 1). I would like to see this explored more – is

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the higher uncertainty in Table 1 because of the resolution? If you subsample an earlier cruise at the 2014 resolution do you get the same results as the original resolution?

There are several points in your questions. First, we did sensitivity study on the 2010 data to determine the optimized sampling for the GEOVIDE cruise. The 2010 transport data showed the same results with the original high resolution and the GEOVIDE resolution, but indeed, errors on regional features increased when subsampled. However, in 2014, we used an OS38 (the latest generation of SADCP with a 1200m range), and could average the constrains in a deeper layer (more geostrophic) with less uncertainties that all the previous surveys where a NB75 was used. This is why, at the end, the errors in transports are quite similar in GEOVIDE and in the previous OVIDE surveys. This information will be synthetized in section 2.2 in the new version of the manuscript. Second, in table 1, the 2014 errors were calculated from the covariance matrix resulting from the box inverse model, while the errors given for the means (2002 - 2012) were standard deviations of the six estimates of the different currents. In fact, because the transport estimates of the currents are more or less stable during the 2002 – 2012 period, their standard deviations are low compared to the error given for each single OVIDE cruise. The information about how the errors were computed will be introduced in the table caption in the new version of the manuscript.

It would be useful to have more explanation about how you computed the gyre and overturning heat transport (lines 267-270)

We will add "(their equation (1))" line 269 after referring to Mercier et al. (2015). We prefer not to expand this topic because it is not a new result: one of the most important results of Mercier et al. was indeed that the MOC was the primary driver of the heat flux across the OVIDE section. We just wanted to complement their time series.

3. I struggled to see the importance or relevance of section 3.2 (fronts and eddies). This looks like a description that will be useful for colleagues who are writing papers on the GEOTRACES data, but it seems to sit a little uneasily in the context of the rest

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of the paper. The same can be said for section 3.4. I can imagine that these lines of text could be usefully transferred into a companion paper.

Yes, we agree that 3.2 and 3.4 sections are not so relevant as results. Nevertheless, one of the objectives of this paper was to define the physical background of the GEOVIDE cruise, which is very important for the interpretation of the TEIs distribution to be carried out by our GEOTRACES colleagues. In fact, this paper has been submitted to Biogeoscience Discussion as part of the GEOVIDE Special Issue, where all the GEOVIDE papers are going to be submitted. So, we consider that we can leave both sections in our physical paper, inside this Biogeoscience Special Issue. Nevertheless, we will introduce more information about the other GEOVIDE papers in section 3.4 in the new version of the manuscript.

4. Thermohaline anomalies. You need to state how you computed the anomalies - presumably on pressure surfaces.

It was indicated how the anomalies were estimated at the beginning of the first paragraph of section 3.3 as:" In the following, S and T anomalies were quantified as the mean values of the anomaly patches represented in Fig. 7." Following the remark of the reviewer, we will add in this paragraph that those anomalies were computed in pressure coordinates. Density coordinates are generally more appropriate, but it makes the interpretation much trickier and does not substantially change the conclusions of our study.

Your description would be more easily followed if you related the anomaly patches to the circulation features that you have already described. For example, is the first anomaly (lines 333-334) in the Irminger Current?

Good idea. So we will transform the sentence as: "First, negative anomalies in surface waters were observed over the RR (in the IC and the ERRC), and east of 20°W (in the SNAC and its recirculation)". The deeper anomalies are associated with the variability of waters masses (LSW and ISOW) and not specifically to dynamical features. The anomalies in the Mediterranean Water were already associated with eddies.

If the anomalies are focussed in the main currents (IC, NAC) could that be evidence for ocean transport as a source of the anomalies?

Lines 355-358 suggest that the displacement of the SAF (i.e. central NAC) is preponderant in the fresh and cold anomalies at 23°W. However, we did not really interpret the anomalies in the ERRC and IC with the lateral advection, although, as you suggest, it is surely an advected signal. See below for a more precise answer.

An important point: I do not agree that the bottom of the anomalies is at the depth of the winter mixed layer - in most cases they extend deeper than the WMLD, which is surely significant and counter evidence for your hypothesis of air-sea fluxes being the key driver.

We agree, we did not put enough weight on the advective origin of some anomalies, and precisely the anomaly in the ERRC. Thank you for this interesting remark. So, to answer your remark and improve the manuscript, we will reformulate lines 333-336 by: "First, negative anomalies in surface-intermediate waters were observed above the WMLD over the Reykjanes Ridge (IC and ERRC) and east of 20° W (in the SNAC and its recirculation). In the former, the S and theta anomalies were quantified at -0.07 and -0.95°C, respectively. In the latter, the negative anomalies of S and theta amounted to -0.11 and -0.70°C. In the ERRC, negative S and theta anomalies also appeared below the WMLD, but concerns a water mass that is different from the one in the WMLD (Fig 2b); both water masses are separated by a maximum of potential vorticity (not shown)." Then, in the discussion, we will also reformulate lines 484-487: More evidence for the important role of air-sea fluxes is provided by the distribution of theta-S and oxygen anomalies in the water column. Indeed, the WMLD along the OVIDE section east of the 20 °W coincided with the deep limit of the anomalies (Fig. 7). In the ERRC, the WMLD crosses the anomaly separating SPMW and upper LSW (Fig. 2b), both being advected together by the ERRC, but probably issued from different ventilation regions.

According to Boisséson et al. (2012), the SPMW is formed by air-sea interactions on its way around the Iceland basin.”

line 360 and elsewhere - it is best to avoid subjective words like "remarkably" especially when you do not explain what is remarkable about that observation.

Ok, we checked the whole ms and we can and we will remove this adverb everywhere, and reformulate when necessary.

5. Discussion. This section needs some improvements because the writing becomes less clear and sometimes less focussed. Paragraph 2 (lines 392 onwards) is very unclear. I’m not always sure which data set or feature you are referring to when you quantify the transport, and how that relates to Table 1. You conclude that the Irminger Current is significantly strengthened in 2014, but from the numbers in Table 1 it looks as they are not significantly different within the error bars (the uncertainty on the 2014 estimates are large).

We will make the following changes to improve the clarity of the message. First, we will introduce the paragraph 2 by :“When defining the IC as in D2016, we saw an increase in the IC intensity in 2014, but within the observed variability (table 1). However, the such-defined IC encompasses a warm and salty northward transport and a cold and fresh southward transport. So, to go further [. . .]”. We will also add a specific column in Table 1 for the transport of the part of the IC that flows northward, which is the one that differs significantly from the mean of D2016.

para 4 (line 413 onwards). It is interesting to me that the SAF has shifted southeastward (by how much?).

It is indicated in the manuscript, approximately 100 km along the OVIDE section (line 414), and this is consistent with the SAF displacement observed in the ADT (see Figures 2 in this document and comments in the answer to Reviewer 1). To make it clearer, we will add to the ms the ADT figure (Figure 2 in this document).

Does this actually imply that the Bersch mechanism for freshening of the eastern basin might be at work, even though you are arguing for this not being the source of the freshening?

At the end of the discussion of the manuscript we briefly discussed about the Bersch mechanism. Inspired by your remark, we pushed further and answer no, our observations lead us to conclude that the freshening at short timescales is not associated with more advection of subpolar water into the eastern SPNA. So we will remove the last paragraph of the discussion (lines 520-527) that was related to the observations of Bersch et al., but add this information to the paragraph about the southeastward displacement of the SAF (line 413-418), as follows: "The SAF, that bears the central branch of the NAC, shows also a remarkable southeastward displacement in 2014 in relation to the mean circulation pattern (station 26 in Fig. 1), of about 100 km. A careful study of the ADT streamlines showed that this displacement was not due to a peculiar meandering of the front (Fig 8). Bersch et al. (2007) linked the displacement of the SAF with the NAO. After a decade of neutral winter NAO index, it turned positive in 2011 and continued positive in 2013 and very high in 2014 (Hurrell et al., 2017). Therefore, symmetrically with Bersch et al. (2007), the southeastwards displacement of the SAF is consistent with their observations. However, following the mechanisms proposed by Bersch et al. (2007), an enhanced eastward advection of cold and fresh water in the eastern SPNA would also be expected, and conversely, we found an increase in the heat transport across the section even if we also found cold and fresh anomalies. This observation will be further discussed later in the light of air-sea fluxes".

You need to explain why the Marsh and Grist papers are relevant to this work, since they refer to a different branch of the NAC that does not come here - what is the connection?

Yes, the referee is right, they referred to a different branch of the NAC. The connection here is just to compare our result about the southeastward displacement of the SAF with other works, but obviously, it blurs the picture since it is further south, so we will

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delete these 2 sentences about Grist et al. . .

Finally, I come back to my point about timescales of forcing. I think your result that the heat transport is high even though the upper ocean is cooler is interesting, but I don't agree that it is necessarily contrasting with the results of Desbruyeres et al. The air-sea fluxes that you present are a great result - but they are only for 1 year, and many of the papers that you refer to are talking about ocean transport convergence as the primary factor over longer timescales.

Yes, we agree with the referee, the explanation of the changes at different time scales and the mechanisms generating them is the first point to improve in this manuscript. In relation to the Desbruyères et al., we want to keep the contrast with Desbruyères et al. (2015), making it clearer, so we propose in line 449-453 this sentence instead: "This result might be the effect of a short term variability since it contrasts with the study of Desbruyères et al. (2015), who argued that the long-term variability of the ocean heat transport at the OVIDE section is dominated by the advection by the mean velocity field of temperature anomalies formed upstream rather than the velocity anomalies acting on temperature".

It is not correct to say that the 2014 anomaly comes after 18 years of warming and salinification - papers by Robson, Holliday, and the ICES Report on Ocean Climate show observed declining temperatures and salinity in these eastern basins since the late 2000s, part of multi-year variability.

Yes, totally right, the sentence "The 2014 anomaly was the first detected, after approximately 18 years of warming and salinification" is going to be deleted in the new version of the manuscript because it is wrong.

That said, the obs also show a sharp drop in salinity and temperature in recent years, so it seems likely that the longer term changes in circulation are being reinforced by enhance air-sea fluxes. It might help if you focused the discussion on a short term atmospheric influence, superimposed on a longer term trend.

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Exactly, as it has been previously exposed in this document, this is what we are going to do in the new version of the manuscript. We will even add “at short time scale” in the last sentence of the abstract: “We concluded that, at short time scale, these changes were mainly driven by air-sea heat and freshwater fluxes rather than by ocean circulation”.

Minor edits

- Lines 88-89, Holliday et al does not look at the Irminger Sea. Their hypothesis was about a shift of the subpolar front (the Bersch mechanism) not advection.

Right only Iceland basin, we will remove Irminger in this line. About their hypothesis, actually, we were referring to the sentence “The decrease in potential temperature and salinity after 2010 in all basins provides the first new evidence that the eastern subpolar Atlantic is once again influenced by cold, fresh western subpolar water.” (paragraph 5, section 5 of Holliday et al. (2015)”. So we will explicitly quote Holliday et al. to clarify.

- line 144 (and elsewhere) "hydrological" should be replaced with "hydrographic"

Yes, we will change it everywhere.

- line 219 use "Fig. 3b" rather than "Fig. 3, lower panel"

Yes, right.

- line 239 what do you mean by barotropic streamfunction here? You are referring I think to the plot of accumulated transport - is that the same thing?

Yes, the barotropic streamfunction is the volume transport vertically accumulated and horizontally accumulated from Greenland to each station. We will indicate it in the new version of the manuscript.

- line 246 and Fig. 5, I find the green dots hard to see - can you use a color that stands out more clearly? Yes, we will change it to white dots.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2017-292, 2017.

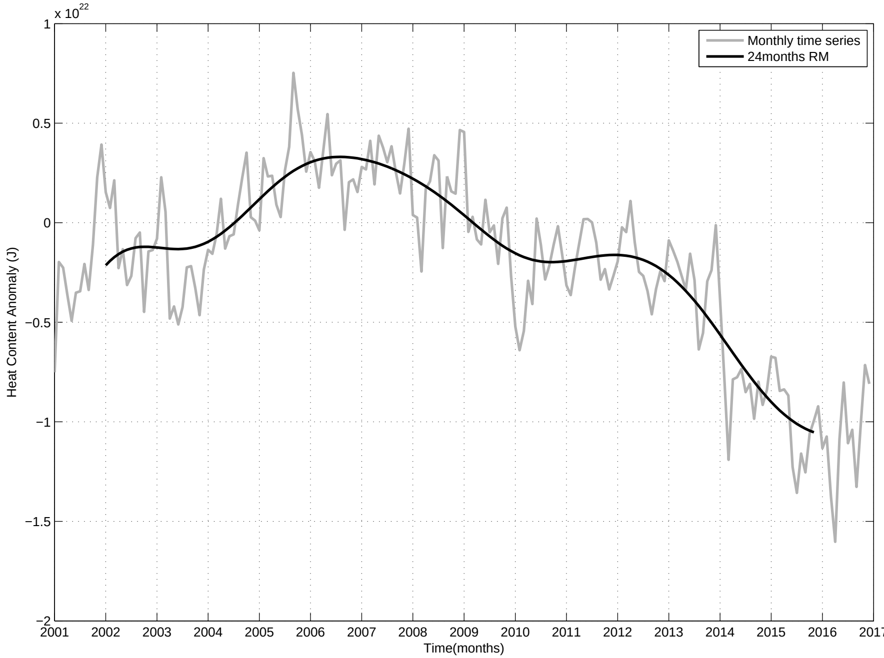

Figure 1: Heat content anomalies in relation to the mean heat content for the period 2002 - 2012 in the upper 1000m of the region 40°N-60°N and 45°W-10°W. Grey line is the monthly time series; black line is the 2-year running mean of the monthly time series. Data source: EN4 database (Good et al. 2013).

Fig. 1.

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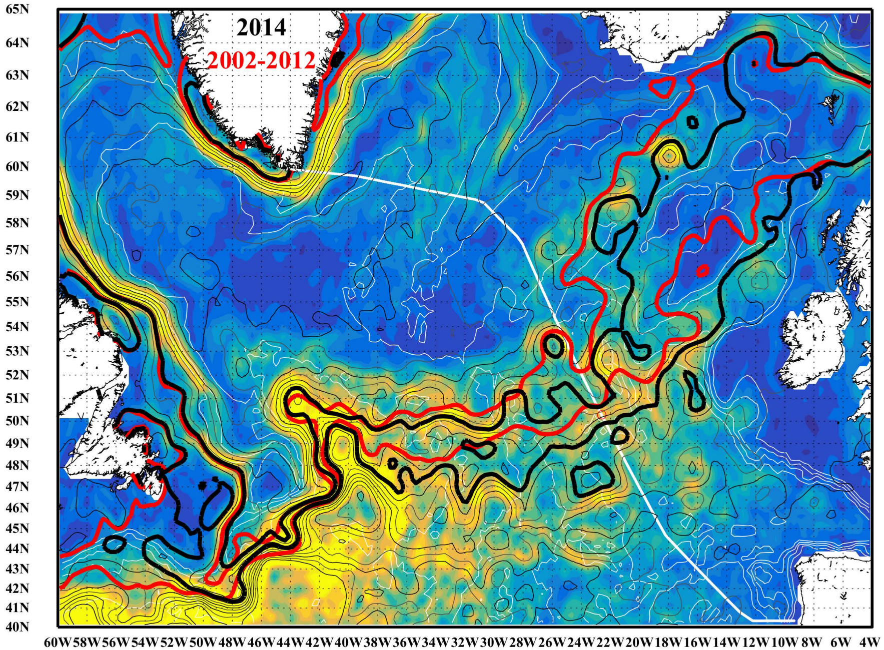

Figure 2: Contours of the Absolute Dynamical Topography averaged over 2014 (in thin lines). Contours are every 0.05m. Thick contours correspond to the levels encompassing the SAF front during OVIDE cruises: bold red lines for the mean 2002 – 2012 and bold black lines for 2014. Note that the temporal trend on the mean ADT over the whole box (2.8mm/yr) was removed. Bathymetry (1000m step contours) and the OVIDE section are plotted in white. Colors represent the absolute velocity of the current (yellow for velocities stronger than 0.3m/s). This figure will be added to the new ms.

Fig. 2.

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