Dear Editor.

I have read the manuscript entitled Ocean Heat Content in the Iberian-Biscay-Ireland regional seas by Pascual-Collar et.al

The manuscript makes use of different Copernicus products that provide, or allows computing, Ocean Heat Content in the IBI region. The work describes the spatio-temporal characteristics and also in terms of depth level integration of OHC series obtained in relationship to water masses in the region.

While I do not appreciate inconsistencies in the analysis, I feel that at this stage the manuscript does not present enough new and relevant research to merit a paper on its own. The analysis consists of computing linear trends from a number of available Copernicus products, and the discussion on the outcomes appears to me too descriptive and sketchy. I think that further work is needed in order to improve the manuscript. Some proposals are indicated below.

First, the authors would like to thank the reviewer for their valuable comments. Addressing his remarks and following his suggestions, the revised manuscript will be certainly improved.

The authors agree to a certain extent with the limitations pointed by the reviewer with respect to the original manuscript. Somehow the OSR structure (together with its limitation in extension and figures) conditions the contribution, and the paper may result a bit sketchy. The authors points the difficulties to compress the information needed to support a full paper in such a brief contribution. In our opinion, the reviewer has evaluated this contribution using considerations of a full paper (and we thank for that), but probably it should be evaluated keeping in mind the mentioned extent contribution limitation associated to the OSR guidelines.

The revised manuscript will be significantly improved following reviewer's suggestion and generalities, pointed by the reviewer, will be reformulated or suppressed.

The authors would like to remark here some scientific achievements of this contribution:

1. The main purpose of the contribution, focused on the analysis of Copernicus OHC OMI on regional scales and its sources of variability and uncertainty, is fully accomplished.
2. This work also provides information of the key role that the variability of subsurface water masses plays in the OHC trends in the IBI region
3. The work provides numeric estimations of warming/cooling for the whole IBI region as well as for specific subregions and water masses.

These estimations are computed using state-of-the-art methods having an intrinsic value by itself, moreover they can be used in future works to compare with other studies.

4. We agree that some discussions in this work left a path open for further investigations, but that is in fact a scientific result usually included in many scientific works.

After a carefully read of the reviewer's comments, the authors can partly agree with them, and we think they can be considered to improve the manuscript. Therefore, we propose to include the following mayor changes in the manuscript that mainly affect sections 4 and 5:

1. Include a better, and clearer, description of the main objective of the contribution: the evaluation of the proposed Ocean Monitoring Indicator in the IBI region.
2. Give a better discussion on the outcomes, considering and following the reviewer's suggestions. analyse the consistency of results and the relevance of interannual variability on OHC trends. To this aim, the authors will deeply modify the Section 4 (Analysis of regional trends) and 5 (Analysis of OHC trends across different water masses).
3. Highlight, as part of the conclusions, the scientific contribution of the work.

My first concern regards the purpose and value of using (and averaging) 5 different products. I understand that this work is not focused on product intercomparison and detailed documentation of each product is linked on table 1. I expect all products should yield similar outcomes since the bulk of baseline data comes from available ship-based hydrography and Argo floats, however further details on differences between products and especially on the causes of these differences will add value. If all products are very similar there is no point in averaging all five available, otherwise it would be interesting a discussion on which product may suit better. I am in particular confused about differences between the two reanalysis IBI-REA and GLO-REA since both have same resolution and coverage.

The methodology used in this contribution follows analogous methods to those previously used and discussed in the literature (specifically in the OSR: von Schuckmann et al., 2016; von Schuckmann et al., 2018; Lima et al., 2020; Mayer et al., 2021). Regarding the averaging of 5 Copernicus products, it should be mentioned that we are not as interested in the average itself than in the spread (the differences) of the products. Through the computation of the spread we obtain a proxy for the uncertainties of the indicator and thus, information on the indicator reliability (Lines 101-104). For example, the significant decrease identified in the spread after 2003 (Figure 1c) is a useful information for the user, and a warning on the existence of bigger uncertainties of this indicator in its earliest period 1993-2003.

The discussion of differences observed between products is mainly confronted in Section 3 (Lines 122-127), in this section the authors state than the main differences between products are due to the lack of observational data before the implementation of the Argo array. However, we are open to modify the text in case of the reviewer consider such explanation is not clear enough or some information is missing.

The discussion of which product may suit better is completely out of the purpose of this contribution (indeed, providing such direct product comparisons never was the spirit or goal of the OSR), anyway if that would be the objective, Figure 1 would show a different coloured line for each product. In this work, we assume the ensemble approach, so that, the use of an average of products (even if they are highly correlated) is always better than the use of just one product; and the spread of the members can be used as an indicator of uncertainties.

The authors do not understand where in the text the reviewer observes "differences between the two reanalysis IBI-REA and GLO-REA" because such comparison is not shown in the manuscript. We assume that this conclusion could be related with the results shown in Figure 2 and discussed in Section 4 (Lines 148-151). However, as it is explained in the text and Table 1, the resolution of GLO-OMI-trend is lower (0.25 degrees) than the resolution of our results (0.083 degrees). If this is the source of confusion, we are open to modify the Section 4 to provide a clearer understanding to readers.

The authors consider very fruitful the discussion of any result derived from our work, and we are fully open to include new related information in the resulting contribution. However, we would need further details about where in the manuscript the reviewer observes differences between IBI-REA and GLO-REA.

My second concern is that the discussion on OHC changes in relationship to water masses is too brief. Changes are interpreted in terms of boundaries advance/retreat, while no definition on boundaries is provided nor are insights on circulations changes that may cause such boundaries shits. I elaborate further on the specific comments.

Again, we crash here with the size limitations of this OSR paper. We would like to show in this work an analysis as detailed as in, for example, Pascual-Collar et al. (2019). But the issue is that whereas Pascual-Collar et al. (2019) has 17 pages (and 10 figures), the present contribution in review will barely have 7 pages (and 4 figures).

Therefore, this work assumes conclusions derived from other works such as Leadbetter et al. (2007), Bozec et al. (2011), and Pascual-Collar et al. (2019), being these works cited to avoid long explanations. The cited papers develop the hypothesis of the oscillatory processes of subsurface water masses in the Northeast Atlantic. Additionally, they provide a detailed definition of boundaries, discussion of circulation changes, and relationship with NAO index. This hypothesis could be revised (and improved) on the basis of new research; but as far as we know, currently there is no evidence (i.e. scientific publication) that supports other alternative hypothesis. Therefore, the scientific method supports the use of the current hypothesis to explain the current observations. The authors would be happy to revise the results in case of new scientific information appears on this topic.

According to our understanding, the scope of the OSR is to *"provide a comprehensive and state-of-the art assessment of the state of the global ocean and European regional seas for the ocean scientific community as well as for policy and decision-makers".* On this framework, the authors consider more adequate to present (i) an analysis of the proposed Ocean Monitoring Indicator, (ii) a discussion of the observed trends, and (iii) a discussion of how results are consistent (or not) with the ones seen in previous works; than a deep analysis of the oceanographic processes behind the oscillatory processes of subsurface waters in the Northeast Atlantic.

However, we understand that the discussion of OHC in relationship to water masses can be improved by addressing deep changes in section five. On this regard, we propose to analyse the OHC profiles in two different periods: 1993-2010 and 2010-2018. These two periods are proposed to represent two different behaviours of NAO index: negative trend in the period 1993-2010 and positive trend in the period 2010-2018. We consider this analysis can reinforce our conclusions highlighting the concordance of results with the hypothesis developed on Leadbetter et al. (2007), Bozec et al. (2011), and Pascual-Collar et al. (2019).

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Specific Comments

l.8 (abstract). The statement that OHC has increased not only globally but at regional scales is almost self-evident; OHC cannot increase globally if it does not also increase in [many/most] regions.

This assertion will be modified as suggested to avoid redundancies.

l.10 "observed derived products" sounds weird to me.

The sentence will be modified as follows:

*"...several Copernicus Marine reanalysis and **observational** products are used together to provide multi-product estimations of OHC anomalies over the water column..."*

l.15. There is no contradiction between (1) having significant warming and (2) having OHC variability dominated by thermohaline variability of subsurface waters. Indeed, it is neither 'thermohaline variability' dominating 'OHC variability' nor the other way around, both are equivalent. I guess authors are trying to convey that interannual variability due to advective patterns dominate the thermohaline variability/OHC. This should be made clearer across the ms. This is said again in the short summary.

The authors agree on this point, and the text will be modified to clarify the role of advective patterns and water mass distribution. This sentence is in a section that will be strongly changed in the revised manuscript.

l.100. It is indicated that the level 150m is chosen to 'analyse the OCH varibility in the mixing layer', however mixed layer depth varies strongly across the IBI domain from several hundredths of meters west of Ireland to tenths in the south (e.g. http://mixedlayer.ucsd.edu/). If authors wish to analyze OHC variability/trends in the mixing layer they should not use a fixed 150 m reference depth.

The authors are aware that in oceanography is quite usual to accept the depth around 100m or 150m as an average depth to represent the mixing layer. However, in order to prevent misunderstandings, it was avoided the use of "mixing layer" in relation with the OHC integrated from surface down to 150 m depth, being replaced by "surface layer" or "upper layer".

l.128. ss. the authors highlight the increase of OHC for the whole period 1993-2020 but in the next section it is decided to compute trends for the period 2005-2019 since the global Argo array become dense enough, and this period shows a cooling.

The authors fully agree with the reviewer on this point, we consider this contradiction as something that blurs the conclusions.

We propose to modify section 4 showing the regional trends not only for the period 2005-2019 but also for the whole period 1993-2020. We consider the difference of results computed in these two periods an important result worthy to be discussed in the text.

Additionally, Figure 1 is modified accordingly, thus it will show trends computed in the two periods (2005-2019 and 1993-2020). The text will be consequently adapted to discuss this result.

l.184. the cooling trends in Fig.3 are computed for the period 2005-2019, so the statement that this is consistent with changes in thermocline thickness described in

2007 by Leadbetter et.al. (right at the start of the series or even before) deserves further explanation. Is it that the process described by Leadbetter initiated cooling trends in the IBI region? Is it suggested that this mechanism continued operating the following decade?. The authors should notice the large scale North Atlantic freshening/cooling well documented for the 2010s (e.g. https://doi.org/10.1038/s41467-020-14474-y https://doi.org/10.17895/ices.pub.7537)

We understand the comment of the reviewer. This sentence is too short and summarized, and a clearer discussion of results regarding Leadbetter et al. is due. As explained in the respond to the general comments, deep changes will be done in this section to provide a better interpretation of results.

Additionally, we thank the reviewer's recommendation to enhance the bibliography by means of the inclusion of the ICES report. The detailed information of such report on the North Atlantic helps in the discussion of results.

l.189. the discussion on the displacement of the MOW boundaries in the Horseshoe basin requires further explanation. I do not see clear relationship between displacements of the 'MOW boundaries' westward and warming/salt increase in the region (besides, boundaries are not defined). As long as source waters properties do not vary, a westward shift of the boundary should only cause warming west of the original boundary placement. The warming/salt-increase in this sub-basin makes me feel that the waters are slowed/retained. If so, possible reasons should be discussed.

As we explained in the general comments, this contribution assumes the conclusions derived from previous works, especially Leadbetter et al. (2007), Bozec et al. (2011), and Pascual-Collar et al. (2019), considering these works provide solid results that allows to accept their hypothesis. The discussion of such hypothesis is out of the scope of this contribution and, from our perspective, also out of the scope of the Ocean State Report goal.

Therefore, as can be seen in the sentence in L189:

"This result is consistent with Pascual-Collar et al. (2019) that described a displacement of the MOW boundaries towards the west in the Horseshoe basin in the period 2006-2017."

This work only tries to check whether the obtained results are consistent with the available knowledge of the region. Any discussion regarding the validity of previous hypothesis, would be only applicable in case of find contradictory results, which is not the case.

However, as explained in General Comments, we propose to modify the section providing a more detailed explanation of results and its links with Leadbetter et al. (2007), Bozec et al. (2011), and Pascual-Collar et al. (2019).

l.194. Again about the limits of the MOW, I disagree that the warming in the 43N box (on the northwards pathway of the MOW vein) indicates a westward movement of the MOW tongue.

We can agree that, in this sentence, there is no causal relationship between warming/cooling and displacement of the MOW tongue. Therefore, we will reword the sentence as follows:

"The warming of the westward limits of the MOW waters (boxes 34N and 43N) and the cooling of the northward boundary of MOW (subregion 49N) in the period 2005-2020 are consistent with a westward movement of the MOW tongue described by Bozec et al. (2011)."

l.202 Section 6. Data availability. The products used have been already described; I do not think this 2-line section is necessary.

We may agree on this, but again it is a requirement related to the Ocean State Report structure. Further discussion about this point should be done with the OSR editors.

l.216 OHC changes expressed in W/m3 (power density), should read W/m2?. Also in Table 2.

OHC is usually presented as an integration from ocean's surface down to a static depth (f.e. 0-150 m, 0-700 m, and 0-2000m), therefore results are expressed in $J{\cdot}m^{-2}$. However, Table 2 shows OHC changes (usually in $W{\cdot}m^{-2}$) for three layers with different thickness so, to make them comparable, results have been divided by the layer thickness resulting $W{\cdot}m^{-3}$.

We will include this information in the text.

Figure 3. The procedure to obtain the averaged dots (markers) in the TS diagrams is not explained.

Markers in figure 3b, 3c, and 3d show the spatial average (computed on the corresponding region) of θ and S at each vertical level. Since these spatial averages are computed for timeseries in annual basis, a mean value (θ and S) is obtained for each depth, and year.

We agree with the reviewer that this information is not clearly included in the text, so we commit to solve this issue in the next version of the manuscript.