

In this paper the authors perform a multi-model comparison in the regions surrounding the Iberian Peninsula. From global models using data-assimilation to local models nested into larger scale ones, the authors explore the differences in surface ocean properties for each approach and provides hypothesis and reasoning for the observed patterns. The paper is in general well written with only a few grammatical errors (see details below) and easily understandable.

Although I appreciate the approach and the effort to objectively analyze pros and cons of the different models I do have some concerns on the present version of the manuscript. Hopefully, such concerns could be solved through a revision of the text so the manuscript could be made acceptable for publication.

Many thanks to Dr. Macias for his thorough revision and the number of useful tips that unequivocally will help to strengthen the new version of the manuscript. Please find below a detailed point-by-point response to each issue raised during the revision process. Apologies in advance for the length of this document: a consistent review requires an equally consistent response!

Major concerns:

My first issue could be derived from my own lack of expertise with data-assimilation models but I do find it difficult to completely understand how the CMEMS global model works. It is stated (page 6, line 5) that the system provides 10-days forecasts updated daily. Does this mean that every-day the system assimilate all available information to update its status and then is run for 10 days? Then, the next day the cycle re-start, assimilating data for the new day and re-running the system for another 10 days? If I understand this correctly, the Global model is only left 'free' for one day at a time, am I right?

Not exactly, the product is updated as summarized in Figure 1:

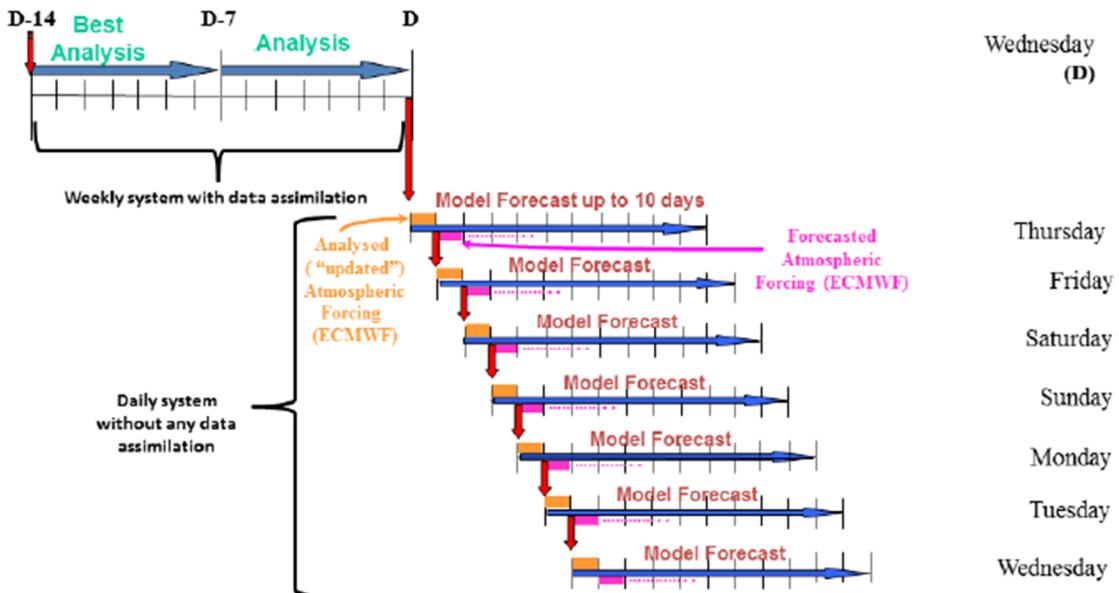


Figure 1: Scheme of the GLOBAL model operational chain.

In order to clarify how the GLOBAL operational chain works, the following paragraph has been added to section 3.1:

"Everyday, the daily configuration is run with updated atmospheric forcings, without assimilation, for days D-1 to D+9. The daily runs are initialized with the previous day's run, except on Thursdays, when they start from the weekly analysis run. Every week, on Wednesdays, the weekly configuration is run with assimilation for days D-14 to D-1. This run is separated in two parts: a best analysis for days D-14 to D-8 and an analysis for days D-7 to D-1. Therefore, every day, the time series is updated with new forecasts for days D-1 to D+9, erasing the previously available data for D-1 to D+8. In addition, on Thursdays, the analysis is also provided, replacing previously available files for days D-14 to D-1. The reader is referred to the GLOBAL Product User Manual -PUM- (Law Chune et al., 2019)."

And the following reference has been added to the reference list:

Law Chune, S., Nouel, L., Fernández, E. and Derval, C. PRODUCT USER MANUAL for the GLOBAL Ocean Sea Physical Analysis and Forecasting Products GLOBAL_ANALYSIS_FORECAST_PHY_001_024, Copernicus Marine Environment Monitoring Service, April 2019.

If the above is correct I wonder how it is possible for the model to present such relatively large deviations with respect to satellite in terms of SST (bias range -2/+2, figure 2). This is particularly shocking for me as satellite SST is part of the data assimilated by the model (as you state in page 6, line 26). I understand the calibration/validation is not a task to be performed by the authors but I would like to know your opinion about the system operation, do you have any thoughts on how to improve this issues? Or maybe I totally miss-understood how the system works?

1. As stated in the aforementioned PUM document, the satellite product assimilated by GLOBAL system is the CMEMS OSTIA SST product (the reference of this gap-free product in the CMEMS catalog is SST_GLO_SST_L4_NRT_OBSERVATIONS_010_001). By contrast, the satellite-derived data used in this work to intercompare the SST provided by GLOBAL and IBI models is another one (the reference of this L3 gappy product in the CMEMS catalog is SST_EUR_SST_L3S_NRT_OBSERVATIONS_010_009_a) since the validation against an independent and not-assimilated observational dataset constitutes a more consistent approach. This fact could explain a small portion of the SST differences mentioned by the reviewer, especially in open waters: the satellite data assimilated are different from the satellite data used to conduct the models validation.

2. Furthermore, satellite products are affected by intrinsic uncertainties. The Quality Information documents (QUIDS) focused on the accuracy assessment of OSTIA and L3 satellite products (freely available in CMEMS website) have reported a RMSD of 0.4° (0.2°) between OSTIA (L3) and drifting buoys observations for 2012 (2018). More specifically, in the case of OSTIA estimations the RMSD is 0.40° globally with regional values ranging from 0.28° in the South Pacific (suggesting highest accuracy) to higher values in the North Atlantic (0.47°) or in the Mediterranean (0.89°). By contrast, the merged multi-sensor L3S SST product over European Seas shows reasonably good difference statistics against drifting buoy measurements. This bias in satellite estimations should be taken into account when interpreting the results of the models validation.

Further detailed can be found in:

OSTIA: <http://cmems-resources.cls.fr/documents/QUID/CMEMS-OSI-QUID-010-001.pdf>

L3: <http://resources.marine.copernicus.eu/documents/QUID/CMEMS-SST-QUID-010-009-a.pdf>

3. In the same line, the higher SST discrepancies in GLOBAL performance (Figure 2: a, c, e, g, i, k in the first version of the manuscript) are mainly located in very coastal areas (African and Iberian upwelling systems, Strait of Gibraltar, the Irish Sea and the English Channel) where satellite remote sensing can be complicated by weather patterns and dissolved organic compounds of terrestrial origin that may attenuate signals and yield unreliable results (Thakur et al., 2018). As a consequence, many processed remote sensing products apply a land mask that excludes mixed pixels in nearshore areas and use temporal averaging to account for missing observations. In this context, the SST level 3 composite products had often missing information, likely due to poor satellite coverage, application of a land mask or cloud cover. Here we present the L3 SST availability (in percentage) for the entire 2017 and also for those months selected in Figure 2 of the first version of the manuscript. As shown, L3 data availability was lower (around 70%) in African and Iberian upwelling systems or the Strait of Gibraltar for the entire 2017 (Figure 2-a).

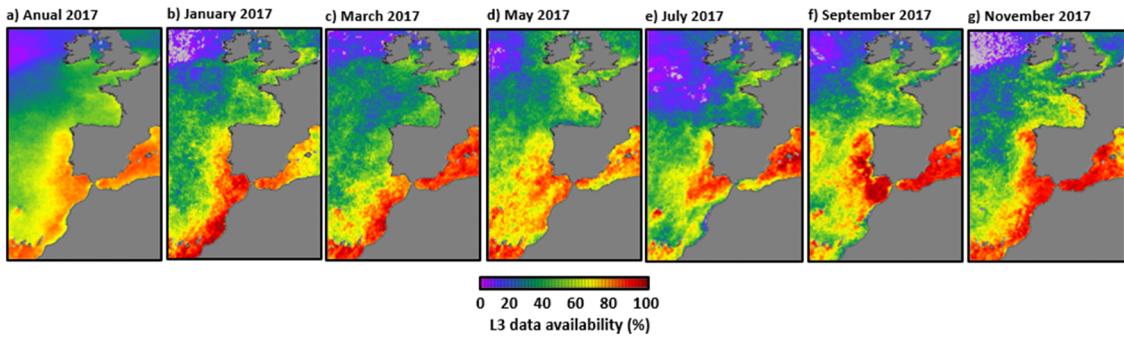


Figure 2: Temporal availability (in %) of L3 satellite-derived SST daily data

Reference:

Thakur, K.K., Varderstichel, R., Barrel, J., Stryhn, H., Patanasatienkul, T and Revie C.W. Comparison of remotely-sensed sea surface temperature and salinity products with in situ measurements from British Columbia, Canada. *Front. Mar. Sci.* April 2018, doi:10.3389/fmars.2018.00121.

4. Another reason that may explain some of the biases lies in the way the data are assimilated into GLOBAL system. As stated in Lellouche et al. (2013 and 2018), an adaptative tuning of observation error has been implemented for SST in the current operational version of GLOBAL system. In this context, taking into account the representativeness error is particularly important for assimilated OSTIA SST because the sky is clear only the 30% of the time on average (section 3.5 of Lellouche et al., 2018). The objective is to improve the error specification by tuning an adaptative weight coefficient acting on the error of each assimilated observation. The observation error variance was increased near the coast (within 50 km of the coast) for the assimilation of SST.

5. Finally, it is well known that tidally-driven vertical mixing over the continental shelf reduces the surface temperature due to mixing with the cooler water beneath the pycnocline (Graham et al., 2018). Since GLOBAL is a detided model solution, this fact could account for a portion of the discrepancies found between GLOBAL and satellite-derived SST estimations in energetic tidal areas such as the English Channel, the North Sea and the Irish Sea (these zones are precisely where the higher discrepancies are located and where no data assimilation is performed). In this context, several improvements in GLOBAL model are planned for the future, as listed in Lellouche et al. (2018): "the river runoff approximations, vertical mixing, the advection scheme or the efficiency of the assimilation scheme, including the assimilation of new types of observations (drifting buoys SST) to better constrain the modeled variables and overcome the deficiencies of the background errors, in particular for extrapolated and/or poorly observed variables".

To summarize and concisely answer reviewer's question ("*If the above is correct I wonder how it is possible for the model to present such relatively large deviations with respect to satellite in terms of SST (bias range -2/+2, figure 2). This is particularly shocking for me as satellite SST is part of the data assimilated by the model*"):

For the SST, higher discrepancies in model performance (compared against L3 satellite data) are located in coastal areas (within 50 km of the coast), precisely where there is lower availability of satellite data and observation errors are higher (and subsequently penalized by a weight coefficient acting on the error of each assimilated observation). So we consider that the results derived from the comparison of models against satellite observations should be mainly interpreted in open-waters, whereas in coastal areas higher attention should be paid to the results obtained from the models assessment against in situ observations from buoys (this is the approach we intended to adopt). As mentioned several times along this document, significant differences between satellite and in situ observations have been previously reported (see selected references below in this document). All these comments are reflected in the new version of the manuscript.

References:

Lellouche, J.-M., Greiner, E., Le Galloudec, O., Garric, G., Regnier, C., Drevillon, M., Benkiran, M., Testut, C.E., Bourdalle-Badie, R., Gasparin, F., Hernández, O., Levier, B., Drillet, Y., Remy, E. and Le Traon, P.Y.: Recent updates to the Copernicus marine Service global ocean monitoring and forecasting real-time 1/12° high resolution system, *Ocean Science*, 14, 1093-1126, 2018.

Lellouche, J.-M., Le Galloudec, O., Drévillon, M., Régner, C., Greiner, E., Garric, G., Ferry, N., Desportes, C., Testut, C.-E., Bricaud, C., Bourdallé-Badie, R., Tranchant, B., Benkiran, M., Drillet, Y., Daudin, A. and De Nicola, C.: Evaluation of global monitoring and forecasting systems at Mercator Océan, *Ocean Sci.*, 9, 57-81, 2013.

Graham et al.: AMM15: a new high-resolution NEMO configuration for operational simulation of the European North-West shelf, *Geosci. Model Dev.*, 11, 681-696, 2018.

My second issue comes from your interpretation of the results in the Strait of Gibraltar. The improvement in AJ direction and speed from the global to the regional model is clear, however the reason for such are not that obvious as you seem to propose. I fully agree with you that increasing spatial resolution (global<IBI<SAMPA) is one of the major reason why the direction and speed of the AJ is better reproduced in the regional model. However, the inversion events could not be related with this issue. In fact, a model with similar resolution to Global (see Macias et al., 2016) was able to reproduce the inversions of the jet. In that work, the remote barotropic effect of the meteorological forcing over the Mediterranean Sea was proposed as one of the major players in the regulation of the seasonal cycle of the AJ and of its occasional inversions. As far as I understood, only SAMPA include this type of effects (page 7, lines 43-48) and, in my opinion, this is the main reason why only this model is able to correctly reproduce the inversion events. I would suggest to make this difference clear in the text; increasing resolution helps with the simulation of direction/velocity of the jet; correct atmospheric forcing (remote) is essential to get the flow inversions.

The authors absolutely agree with this comment. Although it was stated in the conclusions, it is also true that we should have emphasized it more times along the entire document, specially in the discussion of results. As the reviewer will see below, several parts of the manuscript have been modified and enhanced in order to better clarify the relevance of the atmospheric (remote) forcing in the flow reversals and how sub-tidal barotropic lateral forcing for SAMPA, obtained from the NIVMAR storm surge model, ensures that the SAMPA model captures a realistic variability of inflow and outflow currents though the Strait.

I strongly suggest the authors to explore these caveats and to try to address them in a revised version of the manuscript.

Each caveat has been carefully addressed with the hope of improving the quality of the document and make it acceptable for publication.

Minor details:

Page 2, lines 12-17: I don't think global models are able to 'properly resolve' biogeochemical cycles, not even at large scale. Also, this phrase is too long, please consider breaking it up.

OK, authors' feeling was that biogeochemical models, at least within the frame of CMEMS, have been steadily improving in terms of consistency (despite there is still room for significant improvement). With the advent of new technologies such as BIO ARGO floats and BIO data assimilation, a combined use of in situ and satellite-derived observations will contribute positively to both more realistic simulations and more exhaustive model accuracy assessments.

Nevertheless, since this paper is focused on the physical component of IBI forecasting system, any mention to the biogeochemical cycle has been removed. Furthermore, the phrase has been split up into two sentences to improve the readability.

Page 3, line 24: consider changing 'lower' with 'less'

Done!

Page 3, line 36: what does 'poorly controlled information' exactly means?

What we wanted to mean is that one of the main challenges of the embedding between a parent large operational forecasting system and a child one consists in initializing and forcing the child system as best as possible. The child system has to take benefit of the parent solution and it has also to be forced by the best available information from the parent system. Within this context, the expression 'poorly

'controlled information' is directly linked to point iii) of the following paragraph in the introduction:

"The 'parent-son' model inter-comparison is mandatory during both implementation and operational stages since it aids to: i) verify the most adequate nesting strategy; ii) check the consistency of the nested model solution; and iii) identify any potential problem that might be inherited from the coarser system."

Therefore, special emphasis should be placed on the verification that the parent system is consistent enough and transferring coherent information into the son system. As previously stated by Kourafalou et al. (2015), "*if the boundaries try to impose unprecise information that is strongly in conflict with what the model is attempting to do in the interior, then over-specification error results which often leads to instability or spurious boundary re-circulations.*"

In order to avoid any confusion, the sentence:

"However, uncertainties in the downscaling process are hard to quantify since coastal solutions are still exchanging poorly controlled information with larger-scale at their boundaries (Hernández et al., 2018)."

...has been replaced by:

"However, uncertainties in the downscaling process must be evaluated since coastal models performance can be directly impacted by the propagation of any potential issue in the large-scale dynamics, inherited from the coarser system."

Page 3, line 44: 'researchers' should be 'research'

Done!

Page 4, line 28: please indicate in caption of Fig. 1c what the White square represents

Done!

Page 6, section 3.1: as indicated above, I don't fully understand how the assimilation/run/re-start cycle of this model works. Could you please provide a more detailed explanation?

As above indicated, a new paragraph has been inserted in section 3.1 in order to provide further insight into the GLOBAL operational chain and how the forecasting product is daily updated.

Page 6, section 3.2: similarly, the transfer of information from the Global to the IBI system is not fully clear. Does IBI have some data assimilation scheme? Or only

information from the parent system is transferred into the model domain? How often the nudging is done?

The current operational version of IBI (launched in April 2018) already includes a SAM2 data assimilation scheme. However, the previous version of IBI (which is used in this paper, focused on the entire 2017) did not include such assimilation scheme but only the spectral nudging approach. The nudging is applied on a weekly basis during the analysis cycle but the increment applied (described below) is the weekly mean of the daily increments computed between the parent model analysis and the child model forecast. Further details about this methodology have been provided in section 3.2 in order to clarify its impact on IBI forecast estimations:

After each forecast cycle of the child system (IBI), a corresponding analysis cycle is re-launched (Figure 3). The increment used to nudge IBI during the analysis cycle consists in a space and time low pass filter of the difference between the state variables XP (typically currents, temperature and salinity) of the parent model analysis and the variables XC of the child model forecast. As we want to keep the characteristic scales the parent GLOBAL system can resolve (mesoscale structures and the representation of these structures in terms of geostrophic turbulence are improved by the data assimilation of the sea level anomalies), the nested IBI system must be nudged by the parent resolved scales from large scale to mesoscale. So the space and time filter has to keep these scales and remove the smaller ones. Since a week is the typical characteristic time scale of the mesoscale structures, the chosen time smoothing consists in a simple weekly mean of the difference $XP - XC$, which is calculated in the parent system grid.

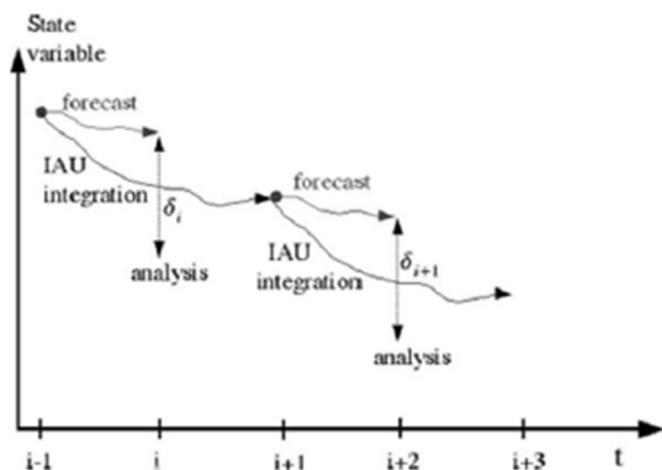


Figure 3: Spectral nudging. Delta is the increment of the i -cycle. IAU mean Incremental Analysis Update

The whole increment is applied during the analysis cycle at each time step, following the function $g(t)$ shown in Figure 4. In order to prevent a discontinuity between the increments of two successive analysis cycles $i-1$ and i and also so to ensure the transition between two cycles, a 1-day overlap is applied which corresponds to the linear decrease in the weight on the increment for cycle $i-1$ and the linear increase in the weight on the increment for cycle i .

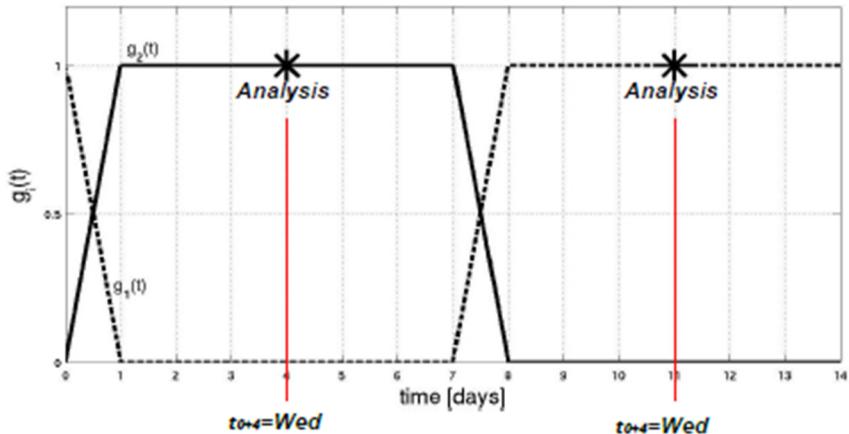


Figure 4: Function $g(t)$

Finally, here we present the nudging parameters applied (Table 1):

Simulation ID	Spectral nudging	Time window	Nudging Applying	Spatial window
1- Nudged run IBI-NUDG	Yes	7 days	1- Bottom > 200 m 2- 30km off coast 3- Depth > 3000m	45 km
2- Free run IBI-REF	No	---	---	---

Table 1: Nudging parameters applied to the 'nudged IBI simulation' (with applying of spectral nudging), in contrast with the 'free IBI simulation' (i.e. without applying of spectral nudging).

For further details, we refer the reviewer to Herbert et al. (2014).

Herbert, G., Garreau, P., Garnier, V., Dumas, F., Cailleau, S., Chanut, J., Levier, B., and Aznar, R.: Downscaling from oceanic global circulation model towards regional and coastal model using spectral nudging techniques: application to the Mediterranean Sea and IBI area models, Mercator Ocean Quarterly Newsletters, 49, 44, April 2014.

Page 9, lines 32 and 33: the symbol ‘◦’ is missing

True! Now this typo has been fixed.

Page 10, line 29: the transect used for evaluation is the black line/white square in Figure 1c?

Yes, it is. The sentence has been modified to clarify this point: “The data availability was significantly high: almost 100% in the selected transect (solid black longitudinal line, shown in Figure 1-c), decreasing in the easternmost sectors.”

Furthermore, at the end of Figure 1 caption, we have added: "The black line denotes the selected transect and the white square represents its midpoint."

Page 11, line 15: I can't see any clear benefit in using the IBI over the Global model in here.

Perhaps the tiny size of each map in Figure 2 of the first version of the manuscript does not help at all to notice this subtle but real difference between GLOBAL and IBI performances in the continental shelf break. Although such Figure 2 has being changed in the new version of the document (see below the next reviewer's comment), here we present a rather illustrative example focused on the annual (2017) SST comparison (Figure 5):

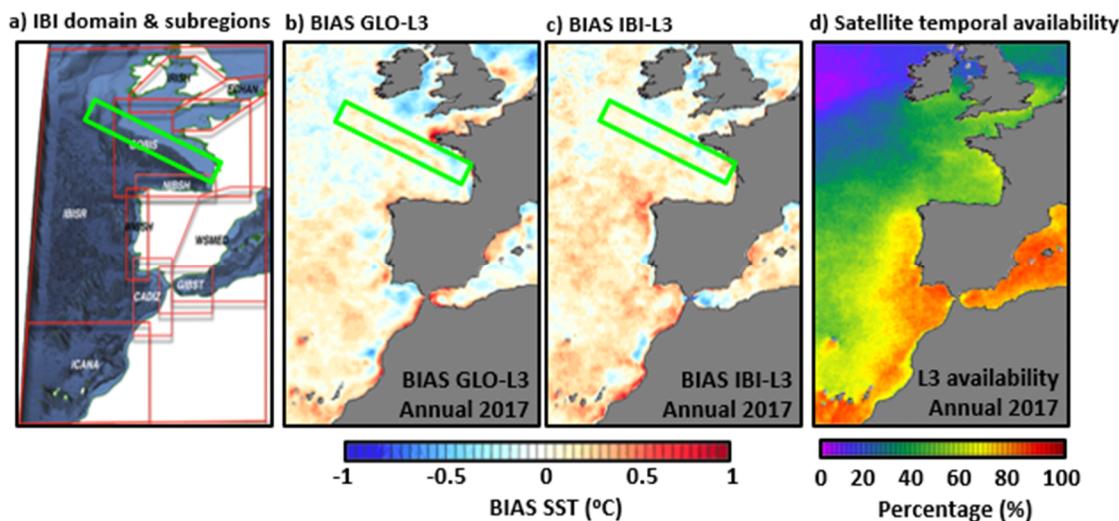


Figure 5. a) IBI domain and subregions. Annual comparison of SST between GLOBAL (b) and IBI (c) against satellite-derived L3 data. Availability of L3 data for 2017 is shown in d). Green rectangle indicated the location of the continental shelf break.

As it can be observed, for the entire 2017 there is a strip of warm bias (comprised between 0.4 and 0.5°C) in GLOBAL map of SST bias (green rectangle in Figure 5-b), coincident with location of the continental shelf break (green rectangle in Figure 5-a). In the case of IBI bias map (Figure 5-c), such strip is not evident. Of course, discrepancies are also detected in other regions, some of them (African upwelling system, Strait of Gibraltar or the North Sea) coincident with those zones where the temporal availability of satellite SST data (Figure 5-d) decreases due to several factors such as poor satellite coverage, application of a land mask or cloud cover.

Page 11, line 23: why the advantages of the SST assimilation into the Global model is not 'propagated' into the IBI? Is it related with the frequency of the nudging? The method?

We have thoroughly revised Figure 2 of the manuscript, which was directly extracted from NARVAL automatic validation web tool. We have detected that IBI panels for May 2017 (Figure 2-f) and July 2017 (Figure 2-h) are wrong. Since there was an issue

related to bad atmospheric forcings from April to July 2017 (the solar radiation flux presented anomalously high values), IBI free simulations during this period were seriously affected and thus the computation of increments (described here in Figure 3) to later perform the spectral nudging was inaccurate, giving rise to the SST overestimation observed in those two panels of Figure 2 of the manuscript (f and h).

Once the issue was detected in July 2017 and the atmospheric forcing was fixed, IBI simulations were re-launched and new consistent outputs were uploaded to the CMEMS online catalog, replacing the wrong files. End-users were timely informed about this change in the catalog through the CMEMS service desk (for further details, see attached document entitled "CMEMS_RFC_July_2017").

However, as NARVAL validation tool works automatically, monthly figures and skill metrics were routinely computed as soon as the wrong simulations ended. We should have launched manually the NARVAL toolbox after finishing the right simulations in July 2017 for those four previous months impacted by the corrupted atmospheric forcing in order to properly update NARVAL tool with the right monthly panels and metrics for the period April-July 2017, but we did not.

We apologize for any inconvenience derived from this lapse. Once NARVAL has been correctly updated, we have accordingly recomputed Figures 2 and 3 of the manuscript. Served as example, Figure 6 below exhibits the main changes introduced, where GLOBAL and IBI performances look rather alike in open waters (Figure 6-a and 6-c, respectively), highlighting the benefits of the spectral nudging technique.

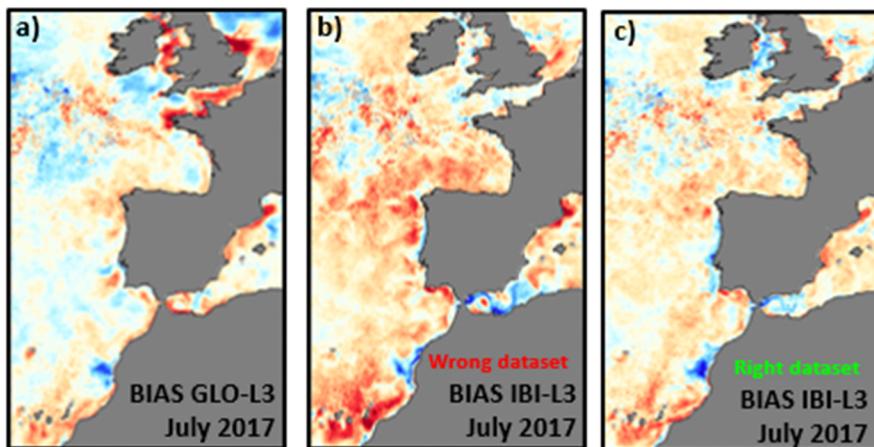


Figure 6: Monthly SST bias (model minus observation) directly extracted from NARVAL validation web tool. a) GLOBAL versus L3; b) IBI versus L3 (wrong dataset, generated with bad atmospheric forcings); c) corrected maps of IBI versus L3, after fixing atmospheric forcings: overall improvement in IBI performance over the entire domain.

With regards to the reviewer's enquiry ("why the advantages of the SST assimilation into the Global model is not 'propagated' into the IBI"), we have modified Figures 2 and 3 of the manuscript in order to better demonstrate how IBI benefits from the data assimilation into GLOBAL by means of the spectral nudging technique.

According to the Figure 5 (shown above), focused on the annual (2017) SST bias of GLOBAL and IBI predictions against L3 satellite observations, the benefits of SST assimilation into the GLOBAL are propagated to IBI nested system (in open-waters areas) thanks to the spectral nudging technique. Within this context, we must emphasize that a tapering function is used in IBI operational chain: this space weight function can be compared to a 3D mask with transition between the zones of the nested system which have to be nudged -typically in open waters, outside the continental shelf area- and which remain free -continental shelf, coastal areas and regions close to the open boundaries- (Figure 7). In those regions where the tapering function is zero (blue color in Figure 7), no spectral nudging is applied and IBI system runs freely. By contrast, in those regions where the tapering function is set to one (red color in Figure 7), the spectral nudging is applied and therefore IBI performance is rather similar to its parent system, the GLOBAL. Both model performances are not absolutely identical in open-waters because the aforementioned weekly increment is a weighted average where a time and space low pass filter is applied to the differences between the state variables XP and XC in order to keep the scales better resolved by GLOBAL and discard the smaller ones.

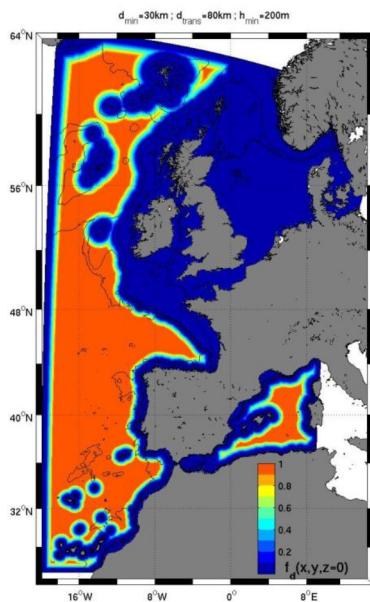


Figure 7: Tapering function at surface for IBI forecasting system

In order to provide further insight into the results derived from the spectral nudging technique, here we show the annual (2017) mean absolute difference (MAD) obtained for GLOBAL and IBI in open waters (where the spectral nudging is activated: Figure 8, a-b) and also for coastal waters (where there is no spectral nudging: Figure 8, c-d)

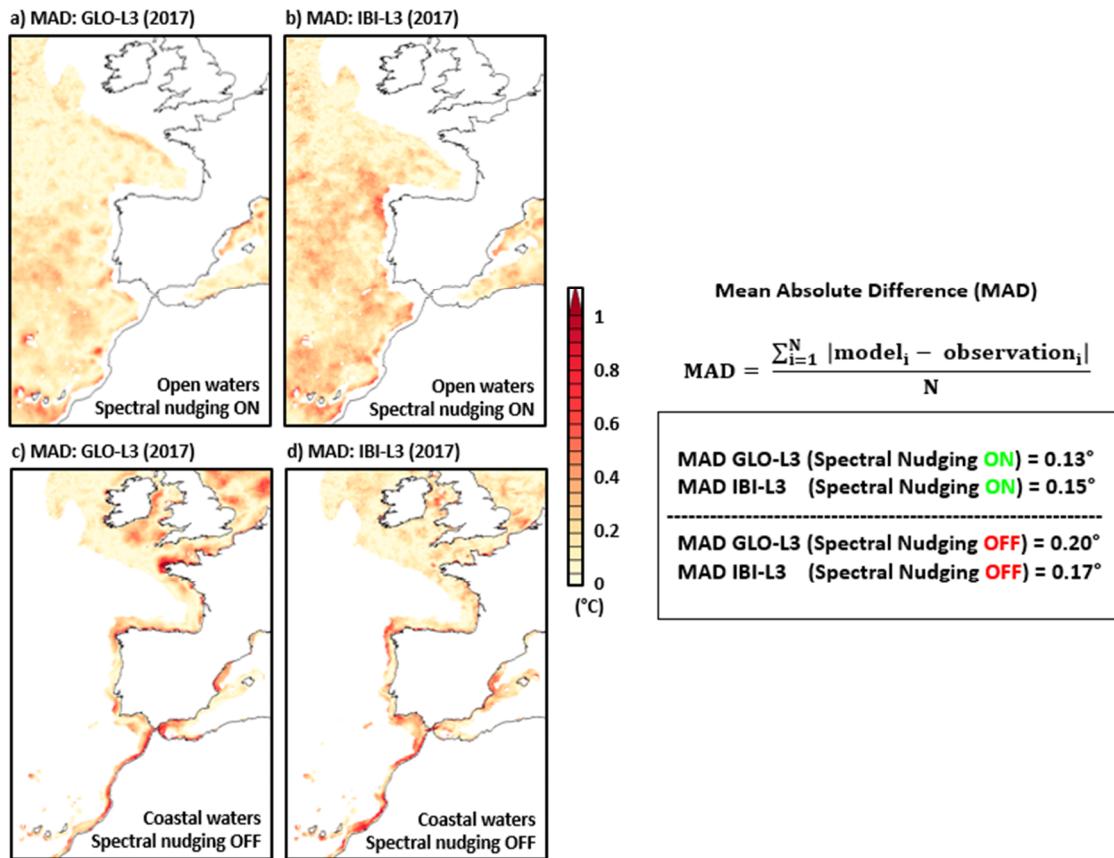


Figure 8: Annual (2017) Mean Absolute Difference (MAD) of SST derived from the comparison of GLOBAL (left) and IBI (right) against L3 satellite estimations. a-b) Open waters; c-d) Coastal waters. Skill metrics gathered in the right black box.

According to the skill metrics (right box in Figure 8), spatially-averaged over the entire open waters domain, GLOBAL performance is slightly better in open waters due to the direct data assimilation scheme. IBI benefits indirectly from the data assimilation implemented in the parent system through the spectral nudging ($MAD = 0.15^\circ$) and even outperforms GLOBAL locally in specific zones, delimited with blue rectangles in Figure 9-a, such as the continental shelf break, western Canary Islands or a portion of the African upwelling system. By contrast, GLOBAL outperforms IBI in the Gulf of Cadiz and the NW Iberian open waters (Figure 9-b).

According to the skill metrics (right box in Figure 8), spatially-averaged over the entire coastal waters domain, IBI performance is, on average, better than GLOBAL (0.17° versus 0.20°) in coastal areas thanks to several factors (among others, the higher horizontal resolution). IBI clearly outperforms GLOBAL in the continental shelf (English Channel, Irish and North Sea) and in the Strait of Gibraltar (Figure 9-c), although it is also true that GLOBAL is slightly more accurate in some parts of the Iberian and African upwelling systems (Figure 9-d).

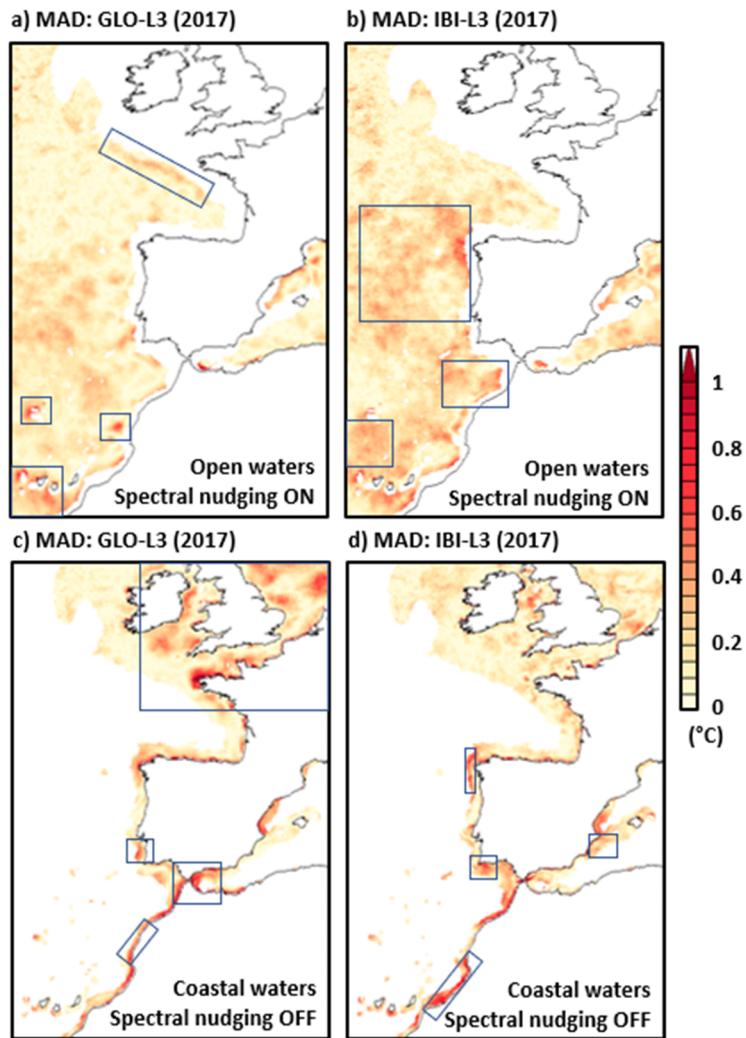


Figure 9: Idem to Figure 8, but zones of worse model performance (i.e., higher MAD) are delimited with blue rectangles.

Page 11, lines 43-47: this explanation does not seem fully justified. The SST anomalies (even in IBI) do not only occur in the coasts, but also many km away where satellite images should not have any issues.

True. The explanation provided in the manuscript justifies a portion of the discrepancies detected in coastal areas (we have to keep in mind that model-observation differences decreased when using in situ SST observations from moored buoys). Regarding the SST discrepancies found in open-waters for both GLOBAL and IBI, they can be partially attributed to the fact that the satellite product assimilated by GLOBAL is the CMEMS OSTIA SST gap-free product, whereas a different and independent (not-assimilated) satellite product (L3 gappy product) was used as benchmark to comprehensively validate the models. A sentence has been added to the paper in order to reflect this.

Page 12, line 2: could you please explain better how the correlation spatial maps are computed?

The maps of temporal correlation are automatically computed through NARVAL validation tool by applying the CDO command “*timcor*”. As previous steps, i) the satellite field is bilinearly interpolated into the IBI grid resolution; ii) IBI outputs are masked taking into account that the spatial coverage of L3 satellite daily observations fluctuates due to the cloud cover and other factors already mentioned. Once the monthly map of temporal correlation is obtained for the entire IBI (Iberia Biscay Ireland) regional domain, the skill metric for each subregion is derived by spatially averaging the correlation values for all those grid points comprised within that sub-region. Since this paper has been submitted to an Special issue of CMEMS and the methodology has been commonly adopted by each CMEMS monitoring and forecasting center (and described in the Quality Information Documents -QUIDs-), we refer the reader to the QUID document for further details.

Notwithstanding, Figure 3 of the manuscript (where correlation values are shown) has been replaced by a new one.

Page 12, line 12: what do you mean with ‘in like fashion’?

Sorry, that was a typo. We wanted to say “in alike fashion”, which means “in the same way”.

Page 12, line 19: it is curious that high ‘r’ are coincident with high ‘RMSD’

Yes, but it is just a mere coincident (Figure 3-f of the first version of the manuscript). In the case of other sub-regions like ECHAN (Figure 3-g) or GIBST (Figure 3-j), lower values of correlation are coincident with higher values of RMSD. In other cases, such as IBISR (Figure 3-a) or GOBIS (Figure 3-b), the monthly correlation remained rather constant despite some fluctuations in the RMSD values. We have not added any additional comment in the manuscript since Figure 3 has being replaced by a new one, as previously indicated.

Page 12, lines 29 – 37: as you are mentioning this 3D comparison some numbers (statistics) should be provided (no figures might be needed though)

With the aim of complementing our statement about 3D comparisons, the EANs (Estimated Accuracy Numbers), provided in the QUID and derived from the comparison of full profiles of temperature and salinity provided by ARGO floats and IBI, are incorporated to the text:

“For the period 2012-2016 and the entire IBI domain, the averaged RMSD for full profiles of temperature and salinity are 0.51° and 0.13 PSU, respectively (for further details, we refer the reader to the QUID).”

Page 12, line 45: the point-wise comparisons you provide in Figure 4 seems to have lower biases than most of the maps shown above.. isn’t it a bit strange?

No, we are afraid that this is not so strange but rather common as the remote-sensed estimations are affected by intrinsic uncertainties, especially in coastal areas, already listed in the present document, among others: poor satellite coverage, application of a land mask or cloud cover.

As previously mentioned, the QUIDS focused on the accuracy assessment of OSTIA and L3 satellite products have reported a RMSD of 0.4° (0.2°) between OSTIA (L3) and drifting buoys observations for 2012 (2018).

Further detailed can be found in:

OSTIA: <http://cmems-resources.cls.fr/documents/QUID/CMEMS-OSI-QUID-010-001.pdf>

L3: <http://resources.marine.copernicus.eu/documents/QUID/CMEMS-SST-QUID-010-009-a.pdf>

Within this context, there is a variety of previously published studies focused on the comparison of in situ water temperature measurements against remote sensing SST products, suggesting significant differences across different geographical regions:

1. Thakur, K.K., Varderstichel, R., Barrel, J., Stryhn, H., Patanasatienkul, T and Revie C.W. Comparison of remotely-sensed sea surface temperature and salinity products with in situ measurements from British Columbia, Canada. Front. Mar. Sci. April 2018, doi:10.3389/fmars.2018.00121.
2. Wu, Y., Sheng, J., Senciali, D., and Tang, C. (2016). A comparative study of satellite-based operational analyses and ship-based in-situ observations of sea surface temperatures over the eastern Canadian shelf. Satell Oceanogr Meteorol. 1, 1–10. doi: 10.18063/SOM.2016.01.003
3. Stobart, B., Mayfield, S., Mundy, C., Hobday, A., and Hartog, J. (2015). Comparison of in situ and satellite sea surface-temperature data from South Australia and Tasmania: how reliable are satellite data as a proxy for coastal temperatures in temperate southern Australia? Mar. Freshw. Res. 67, 612–625. doi: 10.1071/MF14340
4. Williams, G. N., Dogliotti, A., Zaidman, P., Solis, M., Narvarte, M., Gonzalez, R., et al. (2013). Assessment of remotely-sensed sea-surface temperature and chlorophyll-a concentration in San Matías Gulf (Patagonia, Argentina). Cont. Shelf Res. 52, 159–171. doi: 10.1016/j.csr.2012.08.014.
5. Smit, A. J., Roberts, M., Anderson, R. J., Dufois, F., Dudley, S. F., Bornman, T. G., et al. (2013). A coastal seawater temperature dataset for biogeographical studies: large biases between in situ and remotely- sensed data sets around the coast of South Africa. PLoS ONE 8:e81944. doi: 10.1371/journal.pone.0081944

Therefore, the existing discrepancies between OSTIA (assimilated into GLOBAL) or L3 product (used as benchmark in this work to intercompare GLOBAL and IBI) and in situ SST observations from buoys used in this paper have been previously well-documented in the literature. In order to illustrate this, a paragraph has been added to section 5 of the manuscript.

Page 13, lines 10-12: how an intrusion of warmer waters could make the SST to drop?

Sorry, obviously that was another typo. We wanted to mean SST rise, as thoroughly described later on in Figure 12-c of the manuscript. We have replaced "SST drop" by "SST rise".

Page 13, line 27: to avoid this bias you could just extract model data from the closest depth to the buoys? Or make an interpolation to the specific depths?

We fully agree that the best strategy to conduct "offline" comparisons between model outputs and observations is by making interpolations to the specific depth (when comparing in the vertical dimension) and/or to the specific buoy location (when comparing in the horizontal dimension). In this case, the figure was directly extracted from NARVAL automatic validation web tool, which stores and retrieves models outputs only at the sea surface and only for the original model grid points. Up to now, no model outputs are ingested in our database for different depth levels due to obvious storage limitations. Since buoys spatial location can fluctuate and the buoy sensors can be periodically replaced (in terms of brand, nominal depth and characteristics), we thought that for consistency reasons we should feed always our database only with the original model surface outputs at the original grid. Complementarily, offline comparisons (out of NARVAL framework) might include the interpolation proposed by the reviewer.

To illustrate this, we have added at the end of the paragraph:

"Future validation exercises should include the interpolation of model outputs to both the exact buoy location and also to the specific depth level in order to more accurately assess the model skillfulness."

Page 13, lines 37 – 38: I am left wondering if NEMO vertical structure (stability) could be partially responsible for the observed differences. As mentioned above, a data-driven model running freely only for a very limited time should not show such large biases in SST. I know for a fact that NEMO has difficulties to simulate the vertical structure of the water column in the Mediterranean Sea and was wondering if something similar could be happening elsewhere?

The bias detected in the SST has been discussed in detail above. With regards to NEMO vertical structure, we must emphasize that NARVAL tool performs automatic monthly comparisons of IBI performance against quality-controlled daily profiles of salinity and temperature provided by ARGO floats, used as benchmark to infer the ability of IBI to reproduce the vertical structure of the water column.

Here we provide some results derived from the monthly validation exercises, focused on the Western Mediterranean Sea. As it can be seen in Figure 10 for May 2017, the qualitative resemblance between observed and modeled daily profiles is significant for both the temperature and the salinity (cyan and red dots, respectively). Monthly-averaged full profiles (solid black lines) are also rather alike, with similar values at different depth levels.

Before in this document we have mentioned the Estimated Accuracy Numbers (EANs), provided in the QUID and derived from comparisons against ARGO floats, where consistent skill metrics have been reported in the entire IBI domain. Therefore, our perception is that IBI performance is acceptable in the 3D column and within tolerance ranges not only in the Western Mediterranean but also in the rest of subregions. Further comments about NEMO vertical stability are provided at the end of this document.

A sentence has been added to the manuscript in order to clarify this point.

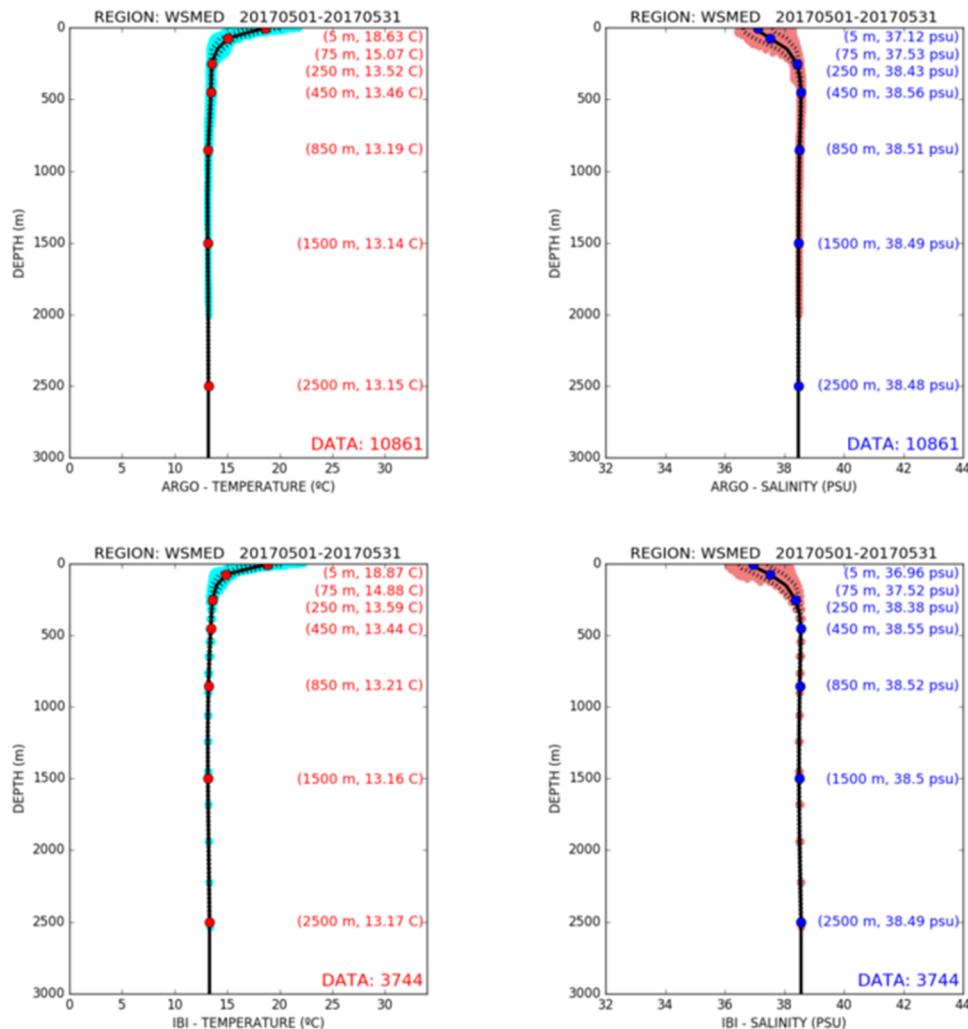


Figure 10: Monthly (May 2017) qualitative comparison between daily IBI outputs (lower panel) and in situ observations from ARGO floats (upper panel) in the Western Mediterranean for temperature (left) and salinity (right).

Page 14, line 18: 'accurate' seems a bit subjective... why not use 'rather accurate' instead?

Done!

Page 14, line 24: the cooling in the IBI simulation does not only occur along the river plume but also on the NW Iberian coast. Could it be also related with some other process happening at a more regional scale? Such as locally-induced upwellings?

In order to infer if the cooling could be related to the NW Iberian upwelling system, we have computed the monthly-averaged maps of SST (from OSTIA satellite-derived observations) and surface currents (from the HF radar deployed in Galicia) along with wind roses from Silleiro buoy (denoted by a black dot in the figure below) for March 2018 (when the impulsive-type freshwater discharge took place). According to the results for this month (Figure 11, a-c), there was no coastal cooling (no longitudinal gradient of SST can be inferred in Figure 11-a) since the main current flow was directed NE (Figure 11-b) under predominant westerlies (Figure 11-c).

By contrast, we could clearly observe the well-documented Iberian coastal upwelling during summertime (August 2018) when northerly winds were predominant (Figure 11-f) and induced the movement of surface waters away from the coast towards the SW (Figure 11-e), which were replaced by cooler water that welled up from below, as reflected by the SST gradient in the Galician coast (Figure 11-d).

According to these results, we might conclude that the cooling in IBI simulation for the entire NW Iberian coast is not related to a regional upwelling event but likely to the climatology used as forcing to take into account the freshwater river runoff at coastal scales. In order to clarify this point, a brief sentence has been added to the paper.

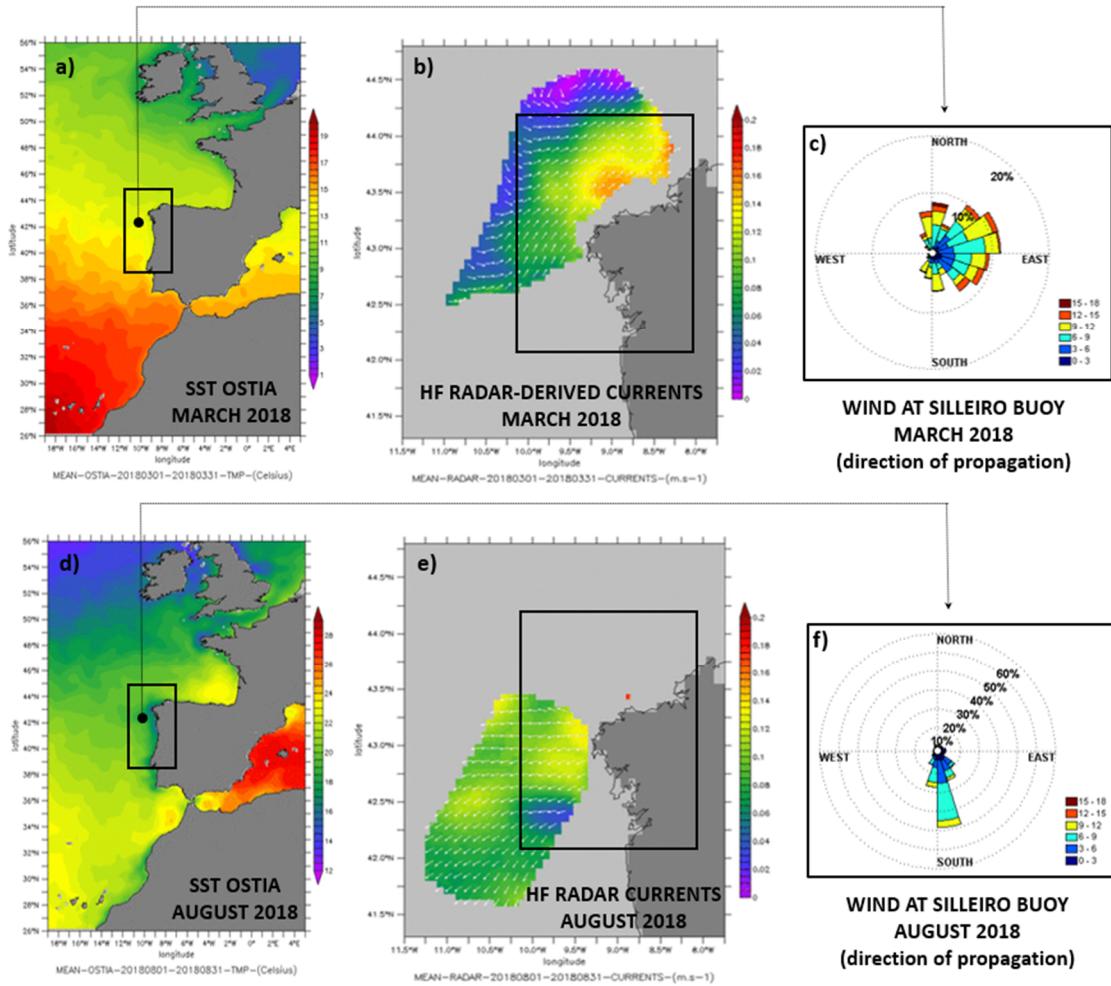


Figure 11: (a-c) Monthly map of OSTIA SST, monthly map of HF radar-derived surface currents, monthly wind rose at Silleiro buoy for March 2018 (propagation direction); (d-f) Idem, but for August 2018. HF radar spatial coverage fluctuated due to a station break down.

Page 16, line 34: red line in Figure 6c is quite difficult to interpret because of the continuous changes associated to the tidal cycle. If you use dots (as with the buoy data) it might be more easy to read the figure.

Figure 6-c and 6-d of the manuscript have been recomputed taking into account reviewer's suggestion: red lined were replaced by red triangles. Accordingly, the caption has been also modified: "Figure 6. (a-d) Monthly inter-comparison (March 2018) between GLOBAL (green line), IBI (red line / red triangles) ..."

Page 15, line 4: it is true that SST decrease on the river plume but, as commented above, in IBI there are other processes bringing up cold waters nearby the coast.

As exposed above, we could not find any relevant evidence that could justify that the drop in SST is partially due to an upwelling process.

Page 16, line 21: the differences between the different models are not just on the downscaling (increasing resolution) but also on the imposition of lateral conditions at the boundaries!

Completely true! In order to emphasize this fact, the paragraph has been extended accordingly by enumerating other relevant factors:

"Overall, a steady improvement in the AJ characterization is evidenced in model performance when zooming from global to coastal configurations, highlighting the benefits of the dynamical downscaling approach along with other relevant factors such as a more detailed bathymetry, a higher spatio-temporal resolution of the atmospheric forcing or the inclusion of accurate tidal and meteorologically-driven barotropic velocities prescribed across the open boundaries."

Page 16, line 26: positive bias with respect what?

Here we used the wrong expression. We should have used "skew" instead of "bias" since we were not comparing against a reference dataset but only analyzing the statistical distribution of values. For this reason, the paragraph has been slightly modified:

"Both datasets show similar positive skew and variability, with the standard deviation around 56-57 cm·s⁻¹ for 2017 (Figure 9, a). IBI and GLOBAL presented narrowed histograms, with distributions positively shifted and constrained to zonal velocities above 0 and 40 cm·s⁻¹, respectively. In the case of meridional currents, each distribution exhibits a nearly symmetrical Gaussian-like shape but shifted towards different values (Figure 9, b)."

Page 17, line 44: the two ways current system you describe in here is not clear from the graphs in Figure 10.

The sentence has been modified in order to properly describe what is shown in Figure 10 (s-t) of the manuscript: the alternation of weaker eastward and westward currents as a result of changes in the predominant wind regime. In the figure attached below (Figure 12 of this document) we highlighted in orange those periods when weaker outflows were observed in the northernmost sector.

Now the sentence in the manuscript is:

"By contrast, in August and December, the classical AJ intense inflow (above 100 cm·s⁻¹) into the Mediterranean was only observed in the southern part of the transect, whereas in the northern sector some fluctuations between weaker eastward and westward currents were evidenced, mainly associated with changes in the prevalent wind regime (Figure 10, s-t). Under persistent easterlies, a weaker coastal counter current was detected flowing westwards and bordering the Spanish shoreline (Figure 10, s)."

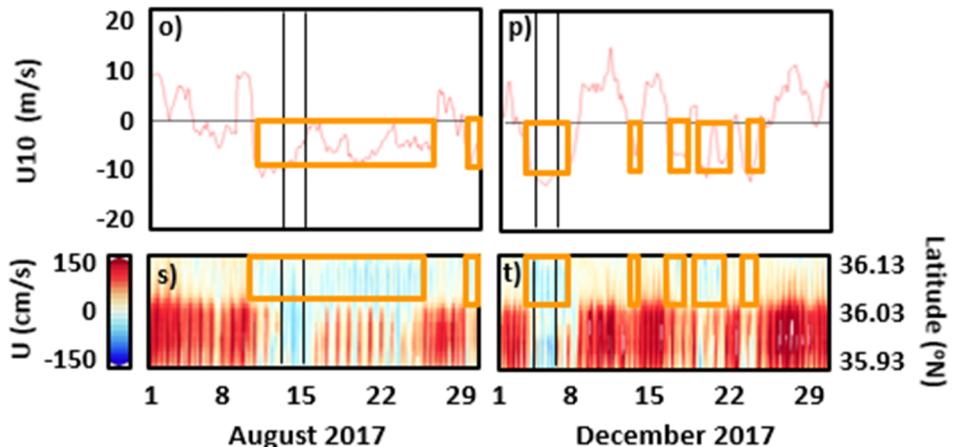


Figure 12: (upper panels) Monthly evolution of ECMWF zonal wind at 10 m height, averaged over GIBST subregion, for August (left) and December (right) 2017. (lower panels) Monthly Hovmöller diagrams of zonal currents derived from HF radar, for August (left) and December (right) 2017.

Page 19, line 27: the fact that SAMPA and IBI represents the tidal dynamics is not because of the nesting, but because you include this forcing in both models (and not in Global).

We fully agree. Perhaps the original sentence could mislead the reader about the reasons that explain the better metrics obtained when moving from global to coastal systems. To avoid this, the paragraph has been rewritten to underline the primary role played by the tidal forcing and also the fact that GLOBAL is a detided model solution:

"The monthly inter-comparison of the zonal currents at the midpoint of the selected transect (represented by a black square in Figure 12-a) confirmed the progressive improvement in the skill metrics obtained (Figure 12-b, right box) thanks to both the multi-nesting strategy and the inclusion of accurate tidal forcing. SAMPA and IBI were able to accurately reproduce the wide tidal oscillations, although only the former could properly capture the flow inversions represented by negative zonal velocities that took place between the 14th-15th and between 21st-24th of August (Figure 12, b), as SAMPA properly resolves the meteorologically-driven (barotropic) currents through the Strait, imported from NIVMAR storm surge model. GLOBAL detided outputs only reproduced basic features of the surface flow, showing always smoothed eastward velocities."

Page 19, line 35: increasing resolution is not the only reason why SAMPA outperforms the other two models (see general comment above).

Fully agree. For this reason, we have emphasized along the entire document that the higher horizontal resolution of SAMPA along with other relevant factors jointly explain the improvements detected in model performance. Such factors have been enumerated in several sections, not only in the final conclusions. Besides, it is directly connected to point iii) in the Introduction ("identify any potential problem

that might be inherited from the coarser system"), in this case, the need to improve barotropic velocities in IBI, something that has been successfully addressed in SAMPA.

Page 19, lines 36 -42: where are the metrics you refer here to?

They are presented in Figure 12-b, black box in the right side. In order to avoid any confusion, we have added: "The complex correlation coefficient and the related phase were 0.85 and -7.37° , (*Figure 12-b, black box in the right side*) respectively [...]"

Page 19, line 43: the warming in Fig. 12c is less than 7.5 degrees, I would say 5?

We are sorry to say that from the 11th to the 17th of August there was a warming from 17.5 to 25 degrees (orange circles in Figure 13, attached below), as shown by in situ observations registered at B7 buoy (blue dots in Figure 10 below). We guess that the reviewer refers to either the SST increment of 5°C (from 20°C to 25°C) that was observed from the 14th (when the full reversal episode started) to 17th of August or the SST increment of 5°C (from 17.5°C to 22.5°C) that was observed from the 11th to the 15th of August (when the full reversal episode ended). In order to clarify this point, the sentence has been slightly modified in the manuscript:

"From the 11th *to the 17th of August*, a progressive warming of 7.5°C at the upper ocean layer of the northern shoreline was observed (Figure 12, c), according to the in situ estimations provided by B7 buoy."

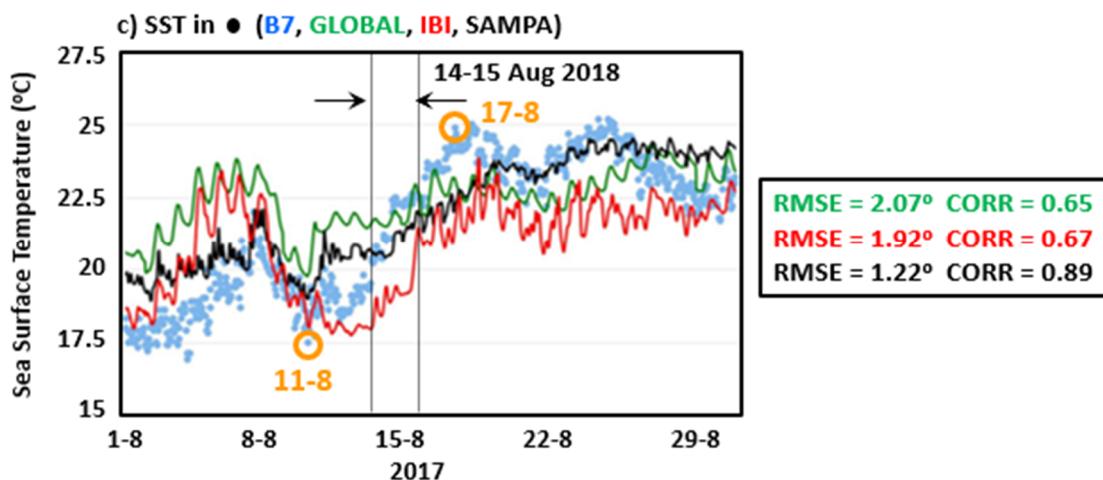


Figure 13: Monthly time series of SST at B7 buoy (blue dots), SAMPA (black line), IBI (red line) and GLOBAL (green line). Monthly skill metrics derived from observation-model comparison are gathered in black boxes on the right.

Page 20, line 15: the situation of the WAG you describe here is not the typical one. The AJ is entering in a rather meridional direction and the WAG seems to be slightly

detached from the NW Alboran. I would say this is already an evolving situation into the inversion episode

We agree, this is not the most typical picture of the AJ-WAG circulation but an early stage of the full inversion episode. The sentence has been modified as follows: "Prelude: the AJ was observed flowing vigorously (with velocities clearly above 80 cm·s⁻¹) into the Alboran Sea with a rather zonal direction (Figure 13-a), heading northeast later on, surrounding and feeding the WAG which appeared to be slightly detached from its traditional position in the western Alboran Sea (Figure 13, b).

Page 20, line 20: the coastal eddy in the NW Alboran is almost always there (see situation in previous snapshot and plenty of reports elsewhere), the only difference is how big this structure is (which is linked to the AJ migration and WAG displacement).

OK, that paragraph has been modified accordingly:

"Onset: as westerly wind lost strength, the AJ speed became progressively weaker and tended to flow more southwardly, giving rise to a weakening and subsequent decoupling of the AJ-WAG system along with the reinforcement of an already existing small-scale coastal eddy that coexisted with the WAG (Figure 13, c-d). Circulation snapshots with three gyres (including the EAG, out of the pictures) have been previously reported in the literature (Flexas et al., 2006; Viúdez et al., 1998). This coastal eddy could be either cyclonic and confined northeast of Algeciras Bay (February 2017, not shown) or be anticyclonic, starting to grow, detach from the coast and migrate eastwards as a result of both the change in AJ orientation and the WAG displacement (Figure 13, e-f). Meanwhile, the WAG presented different configurations: from an almost-symmetric aspect (August 2017, not shown) to a more elongated shape in the cross-shore direction (December 2017: Figure 13-f) or in the along-shore direction (March 2017, not shown)."'

Page 21, point iii): you acknowledge here the potential effects of barotropic flows on AJ inversions but is not clear in your discusión above. As suggested in the general comments, I would recommend to make a stronger case for this difference between models, resolution is important for the Strait dynamics but is not the only element to consider.

We fully agree that further stress should be made in the discussion section about the role of barotropic flows. To this aim, the following paragraph has been added to section 6:

"The reversal of the surface inflow is caused by meteorological-driven flows through the Strait associated with the passage of high pressure areas over the Mediterranean (García-Lafuente et al. 2002). Because these flows originate in the far field and not in the Strait itself, the different grid resolution of IBI and SAMPA do not appear the likely explanation for these events to do not show up in the IBI model. Instead, their

different skill in capturing such extreme events seems to be associated to their different forcing."

Page 22, lines 14 – 19: I would also recommend to keep on improving the mechanics of the models. Data assimilation is a nice tool but should be developed on parallel with model improvements. Otherwise models would only become very sophisticated data-interpolation tools, losing their potential to fill gaps by doing free-simulations.

Absolutely true! For this reason, just after that paragraph (page 22, lines 19-40) we described a number of expected improvements in IBI configuration (increased vertical and horizontal resolutions, a more detailed bathymetry, better meteorological and riverine forcings, etc.) that should indirectly impact on SAMPA performance in a positive way and that besides might be subject of analysis to be also directly implemented in SAMPA.

In this context, a sentence has been added to the manuscript: "Although data assimilation is a powerful technique, advances in coastal ocean modelling should also encompass an improved understanding of high frequency small-scale physical processes, the accurate model parameterization of the effects triggered by such sub-grid phenomena and the integration of air-sea, wave-current and biophysical interactions by means of coupled forecasting systems. Complementarily, as part of possible next improvements of SAMPA system, we will explore the possibility of a more direct nesting strategy into IBI since the current operational version of IBI includes the delivery of 3D hourly outputs."

Page 22, lines 38 – 39: see my comment above about NEMO and vertical stability problems in the Mediterranean. We should think about which model is best to be used depending on its applications.

We have already provided an answer to the previous reviewer's comment about NEMO performance and his concerns about both the vertical stability. Of course, we agree that a wise decision about the model to be used is mandatory depending on its practical application, but also the imposed model configuration (resolution, parametrization, forcings, open boundary conditions, etc.). Based on the reviewer's field of expertise, here we hypothesize that his major concern regards the coupling of NEMO and PISCES biogeochemical model and their joint ability to properly simulate and reproduce the (sub)surface chlorophyll concentration and the related spatio-temporal variability, especially in the Western Mediterranean Sea. Although this interesting topic is beyond the scope of the present paper, we would like to emphasize several aspects:

- i) The present NEMO model configuration, with a rather self-explanatory name "IBI" (Iberia-Biscay-Ireland), was implemented with a primary focus on the Atlantic façade, not in the Mediterranean Basin. In this context, NEMO has proved to be a reliable modeling tool in the North Atlantic, with a strong community of developers and end-users. There is now a vast literature that supports the previous statement. Therefore, the selection of NEMO in the IBI area seems to be appropriate.

- ii) Part of the Western Mediterranean was included in the model service domain in order to: 1) better characterize the inflow/outflow through the Strait of Gibraltar (a key region); 2) provide open boundary conditions to intermediate users (Spanish harbors and Academia), fostering thereby downstream services (for instance, SAR operations in collaboration with SASEMAR) and/or the implementation of high resolution coastal models nested to IBI.
- iii) To better simulate 3D physical and biogeochemical parameters in the Western Mediterranean, it seems clear that the entire Mediterranean Basin should be included in the numerical model configuration (which is not our case) in order to include the aforementioned remote barotropic effect of the meteorological forcing over the Mediterranean (major player in the regulation of the seasonal cycle of the AJ).
- iv) On the other hand, to our knowledge, it is unclear to which extent the bias reported elsewhere in NEMO simulations of chlorophyll (respect satellite-derived observations) could be due to the physical model itself or to the biogeochemical parameterizations (or in which percentage each factor explains those discrepancies).
- v) Finally, we suggest reading Salon et al., paper also submitted to the ongoing CMEMS Special Issue (<https://www.ocean-sci-discuss.net/os-2018-145/>). This work focuses on the skill assessment of a CMEMS biogeochemical system for the entire Mediterranean Basin, coupled to NEMO (grid resolution of 1/24°, 141 vertical z-levels), and run with an operational assimilation scheme (3DVarBio). Results depicted in Figure 14 reveal that model performance (for CHL concentration) significantly improved after implementing the data assimilation scheme, increasing the horizontal resolution (from 1/16° to 1/24°) and evolving the physical component (with new boundary conditions at Gibraltar). So we guess that, despite there is still room for significant improvement, NEMO is a consolidated option in the Mediterranean and the model configuration (for both the physical and bio components) might play a key role to optimize the results.

Copernicus Marine Environment Monitoring Service Request For Change

<u>Change</u>	<u>Notification</u>	<u>Your Reference Number</u> MYO-1137
<u>Risk Level</u> 1		<u>Change Type</u> Standard
<u>Summary of the change</u> Update of the IBI-MFC PHY & BIO historical timeseries in order to substitute files affected by a temporal bug occurred during the first fortnight of April in the atmospheric forcing files. This opportunity will be also used to include two attributes in the header of the files of the BIO product that are necessary for their right display with the GODIVA viewer. Affected products: IBI_ANALYSIS_FORECAST_PHYS_005_005 IBI_ANALYSIS_FORECAST_BIO_005_004		<u>Submission Date</u> 20170725 <u>Requester (Name)</u> Arancha Amo Baladrón <u>Requester (CMS Element)</u> IBI-MFC/Puertos del Estado PC <u>Requester (Email)</u> aab@puertos.es
<u>Justification for change</u> (continue on additional sheet or attach document as necessary)		
TASK 1: Due to a temporal bug that affected the preprocess of the atmospheric forcing files the first fortnight of April, the solar radiation flux presented slightly higher values than expected during that period. The IBI-MFC PHY and BIO hindcast runs (the ones executed once a week to generate the historical timeseries) were forced with this solar radiation flux data. Therefore, in order to supply the best possible solution to the IBI-MFC users, the affected historical timeseries have been generated again from 5 th of April 2017 using the right solar radiation flux forcing files. Products delivered from August 2 nd onwards will not be affected by this issue.		
TASK 2: As the whole BIO historical timeserie needs to be updated for this task, a minor bug that only affects to the display through the GODIVA viewer of two variables of the IBI_ANALYSIS_FORECAST_BIO_005_004 product (the silicate and the dissolved molecular oxygen concentrations) will be fixed. This display tool requires the presence of the "scale_factor" and "offset" attributes in the netcdf header for every variable. This information is usually stored in the headers of the netcdf files in the IB-MFC operational suites, however, when these attributes take values 1 and 0 for "scale_factor" and "offset", respectively, they are not saved. This was the case for the aforementioned variables. In order to fix this minor bug, only an update of the code to force the creation of the missing attributes for both variables in the header of the netcdf files is needed.		
<u>Quality Likely Impact</u> (continue on additional sheet or attach document as necessary – <u>Reminder</u> : if product quality impacted, the related Quid should be updated and delivered)		<u>Technical Likely Impact</u> (continue on additional sheet or attach document as necessary) Warning : Please ensure the DU for the related product is informed of the change TASK 1: No change in the IBI product data content, only in its quality. TASK 2: No change in the IBI product data content, only in the header of two BIO variables. No other CMEMS systems/serviced will be affected. A maximum outage of 2 hours in the access to the user interfaces might be produced.

	Affected product: IBI_ANALYSIS_FORECAST_PHYS_005_005 IBI_ANALYSIS_FORECAST_BIO_005_004	
Description of risks and mitigation activities (continue on additional sheet or attach document as necessary)		
There are no big risks associated to this update. However, in case of detecting any failure due to the modifications introduced in the code or in the update of the historical data, a backup of the old operational suite and the netcdf files of the historical timeseries is available and can be used to recover them.		
<u>Implementation plan</u> (continue on additional sheet or attach document as necessary) The implementation plan of this update is coordinated with the IBI IT-Team. Both historical timeseries, PHY & BIO, have been already re-generated. The date proposed for the update of the PHY & BIO data is the Wednesday 2nd of August between 11:00-13:00UTC. A PSO will be sent to the CMEMS Service Desk for the possible outages during the implementation of this RFC.	<u>Backout plan</u> (continue on additional sheet or attach document as necessary) In case of detecting any failure due to the modifications introduced in the code or during the update of the historical data, a backup of the old operational suite and the netcdf files of the historical timeseries is available and can be used to recover them.	
<u>Test plan</u> (continue on additional sheet or attach document as necessary) The updated data have been validated against observational sources resulting in an improvement of the quality for these products. On the other hand, the right display of all the variables of the BIO product affected by the missing attributes has been checked using the GODIVA viewer.	<u>Communication Plan</u> (continue on additional sheet or attach document as necessary) CMEMS Service Desk is informed about this RFC.	
<u>Related tickets</u>	<u>Scheduled Start Date / Time (UTC)</u> 2017-08-02 / 11:00	<u>Scheduled Finish Date / Time (UTC)</u> 2017-08-02 / 13:00
<u>Decision:</u> Accepted/Refused	<u>Decision on implementation schedule:</u>	