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Ocean Science Discussions

Interactive comment on "Daily scale winter-time sea surface temperature variability and the Iberian Poleward Current in the southern Bay of Biscay from 1981 to 2010" *by* G. Esnaola et al.

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Interactive Discussion

Discussion Paper

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9, C1736–C1768, 2013

Interactive Comment

Manuscript os-2012-118
Response to comments by reviewer 1: “Daily scale winter-time sea surface temperature variability and the Iberian Poleward Current in the southern Bay of Biscay from 1981 to 2010”

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26 April 2013

This document presents the explanations about the changes that we propose to the manuscript, including both concrete changes following specific tasks or questions proposed by each one of both anonymous reviewers and more general changes inspired by coincident comments by both reviewers. We judge that the document will improve its quality thanks to the constructive and instructive comments and suggestions made by the two anonymous reviewers. Some of the comments or suggestions by Rev2 require further explanation from our point of view.

This document is structured so that, first the general changes made to the document are explained and discussed, and second specific relevant tasks pointed by both reviewer Rev1 are answered.

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Interactive Discussion

Discussion Paper

1 GENERAL COMMENTS

This section deals with the general changes that are proposed for the reviewed document, as well as some general concepts discussed by the two reviewers. Two general points are discussed first, and then, the two major changes that will be introduced in the reviewed version are described and discussed. The first discussion point deals with the naming of the SST signal related to the process that has been studied in this paper, to which we referred as the surface signal of the IPC in the first version. This naming was widely discussed by Rev2 and the comments here aim to clarify the processes to which we were referring, also including some of the suggestions made by this reviewer. The second discussion point mainly follows the comments made by Rev1 regarding the spatial structure of the first EOF of the SST anomalies obtained from the reconstructed images, that was identified as the spatial structure of the surface signal of the IPC in the paper.

Then the two major changes proposed to be introduced on the revised document are described. Both reviewers made comments on those specific tasks. The first one is the one related to the analysis of the relation of the time series given in Figure 7 and the most prominent atmospheric teleconnection patterns of the Northern Hemisphere (3818-19, First 4 paragraphs in section 3.3 in the previous version of the document). We now propose two alternative new versions for this part of the document here, and give some additional results in order to reinforce our view on this specific topic. It is left to the criteria of the editor and the reviewers which of those two (a new one) options should be included in the revised document. The second major change introduced in the document is the one related to the temperature and salinity results for AREA V (see Figure 1 for the location of that area) given in Figures 8 and 9, and discussed in the document. This area is proposed to be removed from the analysis following the inconsistencies pointed out by both reviewers. The original reasons that motivated the inclusion of that area in the analysis and the ones that have led to its removal will be

OSD

9, C1736–C1768, 2013

discussed later in this section.

1.1 Nomenclature for the winter-time warm SST signal over the Iberian shelves and slopes

At this point we have to admit that in the first version of the document the were many incorrect uses of term and the concept of the IPC, as well pointed by Rev2. The surface SST structure analysed was incorrectly referred as the IPC in many cases, when it should have been named as the "surface signal of the IPC". In addition to naming errors, and specially in pages 3813 and 3814 of the first version of the document, the concepts of the IPC and its related surface SST signal were also incorrectly mixed, taking them as equivalent things, as well marked by Rev2. This was of especial relevance in the title (as pointed by Rev2 once again) of the manuscript and in the comparison of our time series from Figure 7 (which applies to the surface signal and not to the IPC itself) with the previous bibliography.

All through the document our intention was to deal with the surface SST signal related to the IPC, but as a consequence of a lack of rigor, we mixed the concepts of the IPC and the SST signal related to it. Following those suggestions by Rev2 we have carefully revised the document. On the one hand, the incorrect references to the IPC when talking about the surface SST signal will be corrected. On the other hand, in the cases when our series from Figure 7 (i.e surface signal) is compared with IPC (not surface SST) measurements, or estimations of its variability, the difference between both concepts will be made explicit.

Rev2 suggest the use of the Navidad term in order to refer to this SST signal, both through the text but also for the title of the document. Rev2 gives this definition for Navidad: "The extent of winter SST warming that results from the slope current in the Bay of Biscay along Northern Spain". In the revised document the terms used to refer to the analysed SST structure, depending on the context, will include "the surface signal

OSD
9, C1736–C1768, 2013

related to the IPC", "the SST signal related to the IPC", "Navidad" and "IPC-Navidad". For the western Iberian peninsula and NW Spain "the surface signal related to the IPC" will be used without reference to Navidad. This can be corrected if not appropriate. In the case of the title of the manuscript we will follow the suggestion made by Rev2, so that the changed title will read: "Daily Scale Winter-time Sea Surface Temperature and IPC-Navidad variability in the southern Bay of Biscay from 1981 to 2010". The running title will also be changed to "Bay of Biscay winter SST and IPC-Navidad". For the case of the results, and the discussion, of the PCA of the SST anomalies (Figures 6 and 7), the preferred term will be "IPC-Navidad". This was motivated by the fact that the EOF in Figure 6 shows a well developed SST anomaly signal in the inner Bay.

A final word on the nomenclature that has just been described: with the different terms that have been used, and listed in the previous paragraph, we are referring to a SURFACE TEMPERATURE signal or SST signal. We would like to make it explicit here that we are opened to suggestions by both reviewers and the editor in the sense of setting a preference for or disregarding any of the proposed terms.

1.2 Spatial structure of the first EOF of the SST anomalies

This issue got considerable attention by Rev1. Here additional results are given and discussed, and our view of the reasons that lead to the specific shape of the EOF is given.

In the original document we pointed that the spatial shape of the first EOF of the anomalies required some caution, the reason being the presence of part of the signal of the processes that we were studying in the climatology that was used to deduce the anomalies that yield the EOF. Here we will give some results that reinforce that view, but also some other possible causes that may have an influence in the spatial shape of that EOF. PCA analysis will be repeated but removing the SST signal over the shelf, i.e., retaining only SST pixels over the slope and in open ocean areas. This anal-

OSD
9, C1736–C1768, 2013

ysis will show that the results derived are in very good accordance with those shown in the first version of the document.

Response Figure 1 shows the first EOF that is obtained if the PCA analysis in the paper is repeated removing the pixels over the shelf (depth $>$ 200 m). The shape of the EOF is very similar to the pattern shown in Figure 6, if only the pixels in this Figure are taken into account. In addition, the correlation between the PC related to this EOF and the one related to the Figure 6 in the paper (Figure 7) is 0.98 indicating that almost the same result (in terms of temporal variability) is obtained in the new case shown here. This result indicates that the shape of the EOF shown in the paper is not conditioned by the shelf wide nature of the area selected in that case. In the first version of the manuscript we argued that the shape was related to the presence of the IPC-Navidad signal in the climatology. Another possible cause of the shape of the EOF shown in the manuscript is the asymmetry of IPC-Navidad and non-IPC-Navidad conditions in terms of their SST. This possible cause of the shape of the EOF will be added to the revised manuscript. An evaluation of the impact of this asymmetry, as well as the refinement of the methodology used to deduce the climatology, are open questions that the authors will try to answer in future contributions.

Finally, Rev1 points that our affirmation (in several places of the original document) pointing the the shape of the EOF in Figure 6 is the one that one would expect for the IPC-Navidad is not correct. All those will be corrected introducing some of the concepts described in the previous paragraphs.

1.3 Relation of the time-series in Figure 7 and the Northern hemisphere teleconnection patterns

The first four paragraphs in Section 3.3 of the first version of the manuscript dealt with the relation of the monthly version of the PC shown in Figure 7 (grey bars) and the monthly time series of the most prominent teleconnection patterns of the Northern

OSD

9, C1736–C1768, 2013

Hemisphere. Our intention with the analysis shown in those paragraphs was to illustrate that our time series shows a set of relations with such patterns, but that none preferential relation was observed with any of them. This was used as an argument to justify the forthcoming composite analysis based on the extremes of the IPC-Navidad time series, in which the analysis is not restricted to the indices derived from such teleconnection patterns. The idea behind this is that those teleconnection patterns are useful when strong relations are found with a given process or phenomenon. Those teleconnection patterns characterize some given atmospheric 500 hPa level circulation anomalies (they are defined on that atmospheric level and not in the surface level), but an atmospheric circulation anomaly related to a given process (Navidad-IPC here) does not necessarily have to be related to one (or more) of them. This was the conclusion that was intended to be shown by the analysis in those paragraphs, a conclusion that reinforces the need of an analysis that is not limited to the use of those teleconnection patterns. This way, the results on the atmospheric states characteristic or preceding high/low phases of the IPC-Navidad are only conditioned by the values of the IPC-Navidad index alone, and not on additional restrictions (such as orthogonality of the patterns) that are not necessarily important from the point of view of Navidad-IPC.

For the revised version of the document those four paragraphs are proposed to be replaced with a new writing. Two alternative formulations for the text are proposed here. The first formulation explains that the analysis shown in the original document was conducted without showing its results and only giving the conclusion that no preferential relation was observed with any of the teleconnection patterns. A second alternative formulation gives the major results of the mentioned analysis in a new Table (called Table 3 here but not shown, it would include the correlations shown in 3818 23-26 in the original document) and also reaches the same conclusion. It is left to the criteria of the editor and the reviewers to decide the best formulation to be used in the revised document. The final part of this section shows an additional result using the daily time series of the NAO and EA teleconnection patterns to illustrate once again the lack of a preferential relationship.

OSD

9, C1736–C1768, 2013

The alternative versions for the text in the manuscript are:

OPTION 1 start here Previous studies on the interannual variability of the IPC and IPC-Navidad have mainly focused on the NAO teleconnection pattern (i.e. Garcia-Soto et al, 2002; Garcia-Soto, 2004; Llope et al, 2006; Le Cann and Serpette, 2009; Le Henaff et al, 2011), although some evidence for the relation with the EA/WR (East Atlantic/West Russia) pattern has also been found deCastro et al (2011). All these studies describe lagged integrated relations, among which the relation of January IPC and IPC-Navidad occurrences and the negative mean November-December NAO index is the most recurrent. The relations with the main monthly northern hemisphere teleconnection were studied here as well. A monthly version of the daily frequency IPC-Navidad time series (Figure 7, grey bars) was used for that purpose. Standardized monthly series related to the NAO, Eastern Atlantic (EA), EA/WR, Polar/Eurasia (POL) and Scandinavia (SCA) teleconnection patterns were obtained from the Climate Prediction Center (CPC at NCEP) (http://www.cpc.ncep.noaa.gov). Synchronous and lagged correlations were checked, allowing also time integrated combinations. Using a 99% significance level for meaningful correlations this analysis revealed relationships with NAO, EA, EA/WR and SCA teleconnection patterns, but no preferential relationship was found. Due to this fact the forthcoming analysis will not deal with relationship with the Northern Hemisphere teleconnection patterns. Instead, the extremal values of the daily frequency IPC-Navidad time series and a combination of several atmospheric and ocean surface variables will be used in a composite analysis on order to identify the patterns that drive the variability of the IPC-Navidad. OPTION 1 end here

OPTION 2 start here Previous studies on the interannual variability of the IPC and IPC-Navidad have mainly focused on the NAO teleconnection pattern (i.e. Garcia-Soto et al, 2002; Garcia-Soto, 2004; Llope et al, 2006; Le Cann and Serpette, 2009; Le Henaff et al, 2011), although some evidence for the relation with the EA/WR (East Atlantic/West Russia) pattern has also been found deCastro et al (2011). All these studies describe lagged integrated relations, among which the relation of January IPC and IPC-Navidad

OSD

9, C1736–C1768, 2013

occurrences and the negative mean November-December NAO index is the most recurrent (IPC-J NAO-ND). The relations with the main monthly northern hemisphere teleconnection were studied here as well. A monthly version of the daily frequency IPC-Navidad time series (Figure 7, grey bars) was used for that purpose. The relationship at daily time-scales will be later analysed. Standardized monthly series related to the NAO, Eastern Atlantic (EA), EA/WR, Polar/Eurasia (POL) and Scandinavia (SCA) teleconnection patterns were obtained from the Climate Prediction Center (CPC at NCEP) (http://www.cpc.ncep.noaa.gov). Synchronous and lagged correlations were checked, allowing also time integrated combinations. The significance level for meaningful correlations was set at 99% (estimated by a Monte Carlo test).

Table 3 shows the significant correlations between the monthly IPC-Navidad time series and some of the teleconnection patterns of the Northern Hemisphere. Relationships are observed with four teleconnection patterns (NAO, EA, EA/WR, SCA), being most recurrent for the case of the NAO, , but no preferential relationship is found for any of them. Due to this fact the forthcoming analysis will not deal with relationship with the Northern Hemisphere teleconnection patterns. Instead, the extremal values of the daily frequency IPC-Navidad time series and a combination of several atmospheric and ocean surface variables will be used in a composite analysis on order to identify the patterns that drive the variability of the IPC-Navidad. OPTION 2 end here

To the end with this section an additional result to expand the ideas summarized in the previous paragraphs is given now. Response Figure 2 shows the monthly IPC-Navidad time series together with the one month, two months and three months accumulated index values of the daily NAO and EA time series. For a given day, the accumulated time series indicate the mean of the previous month, two months or three months of the given index. The Figure shows that there seems to be relationship between both indices and the IPC-Navidad time series for some given periods, while not for others. A detailed analysis of this relationships deserves a more detailed analysis that will not be conducted here, and hence is reserved for future research.

OSD

9, C1736–C1768, 2013

1.4 Removal of the AREA V in the analysis in the Section 3.2

The inclusion of the Area V in the first version of the manuscript (Figures 8 and 9) was motivated by the Figure 18 in Peliz et al (2005) that suggested that the flow could have it source in that area. As well pointed by both reviewers, the results in the first version of the manuscript do not support that hypothesis, and are also speculative. Accordingly, we propose to remove that area and the related results and considerations from the revised document. Rev2 also points that the source might be to the south instead of the west and suggests the inclusion of an area VI south of area I. Although this suggestion is interesting, we would not follow it, leaving it for future research as the paper is already long enough and contains enough results.

2 Reply to Reviewer 1 Comments

This manuscript exploits the long-term remote SST observation series from AVHRR Pathfinder data to reveal the variability of the Iberian Poleward Current (IPC) in the Bay of Biscay (BoB) and, with the help of reanalyses of atmospheric fields, to propose a suite of mechanisms explaining this variability. After a description of the various data sets used in the study, the authors briefly describe the methodology employed to extend the coverage of the winter SST maps, called DINEOF (Beckers and Rixen, 2003). The resulting dataset is evaluated in comparison with the non-extended dataset, and with independent in situ data. The satisfying comparison allows the authors to use the extended dataset for investigating the IPC signature in SST. They find that the first EOF mode of the decomposition of the winter SST anomalies with respect to a 15-day running climatology, over the shelf and shelf break along the Spanish and Portuguese coasts, represents the signature of the IPC. The authors thus consider the Principal Component associated with this mode as a proxy for the IPC index. The presence or absence of developed IPC based on this index show good agreement with estimates

OSD
9, C1736–C1768, 2013

from the literature. In situ data selected from events of marked presence and absence of IPC are used to investigate the respective signature in temperature and salinity of these events, which also compare well with previous studies. The authors then notice correlation (lagged or not) of their IPC index with various atmospheric indices. Finally, they use the same episodes of intense and absent IPC as previously, to derive anomaly indices of various atmospheric and oceanic fields. These anomalies show that intense IPC events are associated with changes in the 500hPa level geopotential height and associate surface fields, corresponding to a weakened anticyclonic gyre of the Northern Atlantic circulation, enhanced southwesterly winds over the Bay of Biscay, and enhanced heat gain over the European shelf. The authors suggest that both anomalies in wind stress and heat flux may explain the positive sea level anomalies over the European Shelf, which is associated with more intense IPC through geostrophy. The authors also found that intense IPC episodes are associated with a meridional sea level gradient west of the Iberian Peninsula, which is known to be the main driver of the IPC through JEBAR effect, and with enhanced local positive wind stress curl patterns along the Iberian Peninsula, which are known to also drive the IPC through topographic beta-effect. This manuscript provides a convincing tool for identifying IPC/Navidad (in the BoB) events. Its major quality, in my view, is that it is based on an objective index, derived by the authors based on exhaustive data set, and not on subjective appreciation of individual SST maps or isolated data, as is the case in most of the IPC/Navidad studies. In addition, the evaluation of the IPC index provides a useful synthesis, using previous studies, about the signature of the IPC/Navidad current, both from remote sensing and in situ data. These two aspects support the publication of the present manuscript. However, the interpretation of the atmospheric indices for explaining the intensification of the IPC is not very precise and requires some clarification. This is why I recommend publication after minor revisions. Detailed comments follow.

Detailed comments

Abstract The abstract is quite concise. I think the mechanism responsible for the vari-

OSD

9, C1736–C1768, 2013

ability of the IPC is not explained clearly enough. The origin of this variability in the 500hPa circulation anomaly, and not in all atmospheric fields, is not clear from the text. The end of the abstract suggests that both wind-stress and heat flux anomalies have major influence on the IPC variability, which is not clearly demonstrated in the text.

Reply: The abstract will be modified in order to more clearly explain the mechanism controlling the variability of the IPC-Navidad. Now, it is clearly stated that both anomalies (500 hPa and surface) are not independent. Surface level anomalies, which in turn force the variability of the IPC, can also be identified at 500 hPa. The comment about the relation of this mechanism with the main atmospheric teleconnection patterns of the Northern hemisphere will also be removed (see Section 1.3 in General Comments of this document). Regarding the role that the surface wind-stress and heat-flux anomalies play in the variability of the IPC, the reviewer accurately points out that the text does not give enough evidence to make such an statement, and hence it will be removed from the abstract.

Results Sea surface temperature reconstructions This part is robust and quite clear. Regarding Fig. 3, I don't see how the authors use the right column, since Table 1 already shows very similar BIAS and MAD for the reconstructed dataset in the 2002-2010 period and the Pathfinder-only over the same period. It seems that this column from Fig. 3 mostly shows that the BIAS and MAD of the 2002-2010 dataset are consistent with those from the long-term dataset at the same period. This suggests that the reconstruction using the 9-year series of SST maps is as efficient as the reconstruction using the 30-year dataset.

Reply: Our intention with the right column in Figure 3 was to give additional information to that given in Table 1. As the reviewer points out, both results indicate that the reconstruction covering 9 years is in fact as efficient as the one covering 30 years. The Table 1, on its own, could be enough to support this affirmation, but the Figure 3 was added to show the time evolution of the performance of the reconstructions.

OSD

9, C1736–C1768, 2013

We, however, understand the comment by the reviewer saying that the right column in Figure 3 could be interpreted as redundant information. We added it as a combined reconstruction of infra -red and micro-wave SST images using the DINEOF technique was proposed in this contribution for the first time for the Bay of Biscay area. From our point of view, this is enough reason to justify the inclusion of the right column in that Figure, but we would agree to replace it with a comment in the text stating the relative performances of different reconstructions if the reviewer or the editor judge that convenient.

Sea surface temperature variability and the surface signal of the IPC I have a few comments on this part. First, it appears that the reconstruction strategy leads to some smoothing of the data, which is to be expected, since EOF decompo- sition retains in priority large-scale patterns. In particular, I don't really agree when the authors say that the reconstructed SST fields of January 14, 2006 (Fig. 5) shows meanders as nice as the map from Le Cann and Serpette (2009, their Fig. 1b): the original map showed actual meanders with sharp fronts, whereas the reconstructed map only shows warmer waters extending off the shelf, without as sharp fronts. The smoothing is apparent also in Fig. 4, for the 1996 case: the reference data show a clear signature of the IPC as having warmer waters on the shelf break along the west- ern and northern coast of the Iberian Peninsula. In the reconstructed dataset, this shelf break signature has clearly decreased on the west coast, and vanished along the north coast, leaving only warm waters on the shelf compared to the deep ocean. Then, the SST anomaly approach also tends to affect the identification of the IPC, since the climatology used for the anomaly estimation includes part of the current signature, as noted by the authors. Based on Fig. 1, this signature includes a warm anomaly on the shelf break, especially along the north coast. Thus, in addition to the smoothing, the anomaly calculation also removes the shelf break signature of the IPC, which leads to favor, by contrast, its shelf-wide signature. This is clearly seen in the 1996 case of Fig. 4, where the SST anomaly is the most intense on the shelf and along the coast. As a result, the EOF

OSD

9, C1736–C1768, 2013

decomposition of the SST anomaly naturally favors the shelf-wide signature of the IPC, especially when the spatial extent is limited to the shelf and shelf slope. I think these aspects should be acknowledged, because the IPC is described in the literature as a slope current, and the authors describe it as such in their intro- duction. Thus, its main signature is expected to be along the shelf break, although the intense IPC episodes are also associated with warming of the whole shelf. Still, the approach used by the authors is perfectly valid and well justified, but the fact that the IPC shelf-wide signature is favored might explain some of the differences noted with some previous studies.

Reply: See Section 1.2 in General Comments of this document.

In the comparison with previous studies, I was confused by the two paragraphs (starting p. 3813, l. 10, and p. 3814, l. 12), since some studies are cited in both (esp. Garcia-Soto et al., 2002, Le Hénaff et al., 2011, and Le Cann and Serpette, 2009). I don't really see the point of these separate paragraphs, so this part could probably be clarified. At the end of the comparison about IPC occurrences detected by remote sensing, a sentence or two could be used to summarize its main outcome, and stress the good level of agreement. In that comparison also, the agreement and disagreement could sometimes be more explicitly described (see suggestions later in the review). This would help clarity.

Reply: The first paragraph mostly deals with the comparison with individual detections of the surface signal of the IPC (individual SST images), while the second paragraph deals with the comparison with estimations of its interannual variability. All this part of the manuscript will be completely rewritten following a suggestion by Rev2. Some sentences will be added making the difference between those paragraphs explicit. Also, as suggested, a sentence will be added by the end of the paragraph containing the comparison with remotely sensed images stressing the good level of agreement. With regard to the to levels of agreement and disagreement, a sentence will be added stating that the comparison is conducted comparing the value of the IP-Navidad time series

OSD

9, C1736–C1768, 2013

(Figure 7) with every reference, and effort will be made to more explicitly describe the discrepancies when they occur.

The following comparison, using in situ data, is robust and provides interesting insights on the different aspects of the IPC signature. I just don't understand why the authors say it is expected that area V shows enhanced proportion of ENAWT waters for the P80 cases (p. 3817). Favored proportion of that water mass is expected along the IPC pathway during intense IPC, but it is not clear why it should be the case off shore. The authors should provide some arguments.

Reply: See Section 1.4 in General Comments of this document..

Insights on the Iberian Poleward Current variability mechanisms This part is quite dense, and could be clarified. The authors present monthly corre- lation values with atmospheric teleconnection indices, but there is no interpretation of the results: is there any hierarchy in these correlation estimates? The authors then note various time lags between the IPC index and some atmospheric indices. This analysis leaves the reader confuse, because there is no clear conclusion about these indices and the lags, beyond the fact that there is no dominant atmospheric telecon- nection index driving the IPC. I regret that the authors do not explain how the patterns associated with these atmospheric indices relate with the patterns seen in the analysis of atmospheric fields associated with intense IPC events, which follows. Such relation is expected when reading the analysis of the atmospheric and oceanic field anomalies, or in the discussion part.

Reply: With regard to the analysis of the relation between the monthly IPC-Navidad time series and the monthly time series of the teleconnection patterns for the Northern Hemisphere, see Section 1.3 in General Comments of this document. In the case of the relation of the patterns obtained from the composite analysis and the teleconnection patterns, this is not is not an easy task to tackle with. One could, for example,

OSD

9, C1736–C1768, 2013

regress the patterns in Figure 10 on their corresponding anomalies of the original data sets (each one with its variable) to obtain a time series corresponding to each of the composites, and then compare that time series with those of the teleconnection patterns. The results yield by such an analysis, however, show a scheme similar to that obtained when comparing the monthly Navidad-IPC time series with the indices of the teleconnection patterns (that is, relationships with different teleconnection patterns, but not a preferred one with any of them).

At the end of this section, I was confused by the use of the Absolute Dynamic Topography (ADT) value from AVISO to calculate the meridional steric sea level gradient, which drives the JEBAR effect that is thought to be the main driver of the IPC (Huthnance, 1984, Frouin et al., 1990). The ADT is the sum of the Mean Dynamic Topography (MDT) and the Sea Level Anomaly (SLA). The MDT is the temporal mean of the ADT, which is thus constant. So, the time tendency of the ADT gradient is only due to the SLA part of the signal. But, since the authors calculate the CP composite alone, and not the CP-N, they need the full ADT to access the sea level gradient with the correct sign and amplitude. This choice of composite leads to another question: why did the authors calculate the CP composite for the sea level gradient, and not the CP-N as for other variables? If the CP-N composite shows that the sea level gradient is somewhat the same during intense episodes or absence of the IPC, it means that the JEBAR effect is not responsible for the intensification of the IPC. This would be a major result.

Reply: The ADT was used so that the results were more intuitive to understand. Qualitatively, without the use of ADT, i.e. using only SLA, and according to the results, the CP would indicate an approximately null anomaly (slope due to seasonal signal) while CN shows a negative anomaly (approximately compensating the seasonal signal). This indicates the presence of a negative anomaly somehow compensating the seasonal signal for non-IPC-Navidad cases, the signal can be attributed to the seasonal slope for IPC-Navidad conditions.

OSD

9, C1736–C1768, 2013

Discussion This part is interesting, but could be clarified, mostly in relation with the analysis of the atmospheric and oceanic field anomalies. In the part discussing the in situ T/S data in extreme IPC cases, the authors mention results by Perez et al. (1995) and Pollard et al (1996), who found a temporal trend in their in situ data (p. 3827, l. 6-9). The authors say it is not clear on their Fig. 9, which gathers data from over 30 years. The authors could probably perform a time analysis with their data, to see whether such a trend is seen in the long-term dataset. Later in this discussion, the authors suggest that a salinification noted in area I in P20 cases might be due to "enhanced evaporation combined with vertical mixing, as suggested by Fig. 10." This is not very convincing, because the corresponding patterns on Fig. 10 are large scale, and would thus affect all five areas quite similarly, rather than only one.

Reply: With regard to the analysis of the trends in the temperature and salinity suggested by the reviewer, a preliminary checking confirmed the presence of trends in both variables. This analysis will be extended including the determination of the statistical significance of the trends detected in that preliminary checking. The results, whether statistically significant or not, will be shown in the discussion section of the revised document.. In the case of the salinification for Area I in P20 cases, the argument given by the reviewer is very convincing, and thus, that affirmation will be removed because it is incorrect and speculative.

In the discussion about the atmospheric and oceanic field anomalies associated with IPC extremes, the authors say that the "heat-flux anomaly patterns shown on Fig. 10 are the consequence of the meridional anomalous advection induced by the already mentioned surface pressure and wind stress anomalies" (p. 3828, l. 27-30). In that case, the heat flux is affected by the anomalous advection, but also the anomalous SST and wind stress. I think the authors could provide more details on how the heat-flux anomalies relate to other anomalies.

Reply: This is also pointed by Rev2, so it will be clarified in the next version of the

OSD

9, C1736–C1768, 2013

document and additional references will be added, including Esnaola et al. (2012) that deals with this topic for the Bay of Biscay area. The advection refers to the atmospheric low level advection of heat and humidity by meridional transports. Those meridional transports generate surface sensible and latent heat-flux anomalies which are, together with wind-stress related transports, the major drivers of the SST anomalies over the area, as shown in Esnaola et al. (2012).

Esnaola, G., J. Sáenz, E. Zorita, P. Lazure, U. Ganzedo, A. Fontán, G. Ibarra-Berastegi, and A. Ezcurra, Coupled air-sea interaction patterns and surface heat-flux feedback in the Bay of Biscay, Journal of Geophysical Research-Oceans, 117, 2012.

Finally, I found the part discussing the causes of the SLA anomalies over the Eastern North Atlantic not very conclusive. The authors suggest that both the heat-flux and the wind-stress anomalies play a role. The similitude in the signature of the geostrophic current anomalies over the shelf around the Bay of Biscay with Pingree and Le Cann (1989), noted by the authors, support the role of the wind-stress anomalies. Maybe the authors could use the data they already have (ERA reanalysis and AVISO) to evaluate the seasonal SLA and surface heat flux over the continental shelf: the seasonal SLA cycle should be dominated by the steric effect, associated with seasonal changes in surface heat flux. Then, the authors should be able to estimate whether the heat flux anomaly contributes significantly to the SLA anomaly related to the IPC variability.

Reply: As it will be done in the abstract, the affirmations claiming a link between the surface heat flux anomaly patterns and those of the the SLA anomalies will be relaxed. As already stated, the defense of such relationship is not strongly enough demonstrated in the manuscript, and hence it is speculative. Once that affirmation is removed, the computation proposed by the reviewer, although interesting, is not necessary anymore and it is left for future research.

Conclusions The conclusion is concise. In the third paragraph, I don't see why the

OSD

9, C1736–C1768, 2013

authors mention that the ocean and surface atmospheric processes have their origin in the 500 hPa circulation anomaly (p. 3832, l. 8): the diagnostics are all related, and there is no argument to favor the origin in the 500 hPa field rather than at the surface, at least in the manuscript. Finally, I regret that the conclusion ends on an abrupt note, and not on possible consequences of the results, or future directions of research, but this was partly done in the discussion.

Reply: This is simply a problem of the writing of a sentence of the paper. Figure 10 shows the maps using the geopotential height of the 500 hPa surface but also the information obtained using mean sea level pressure data, and both results are consistent (with the difference that one of the plots shows Z fields whilst the other shows SLP anomalies). We have to consider that atmospheric fields at 500 hPa and sea level are not fully independent.There were several reasons that lead us to use Z500 in Figure 10 and related computations:

The first one is that, in general, previous literature shows that teleconnection patterns and atmospheric low-frequency variability of the kind that can be expected to appear on long time scales as the ones that we were searching for shows an equivalent barotropic vertical structure (Hsu and Wallace ,1985; Thomson and Wallace, 1998). Given this kind of vertical structure, we could expect to find a similar signature (in terms of average anomalies using P20 and P80) at the surface of the ocean and at a middle tropospheric level, albeit with perhaps a stronger signal at high levels above the surface, because of a lower influence of land surface in winter.

Most of the studies in terms of teleconnection patterns have been conducted at the 500 hPa (Wallace and Gutzler, 1981) or 700 hPa (Barnston and Livezey, 1987) and, additionally, teleconnection indices from CPC are also computed at 500 hPa. According to this, we could expect those results would be consistent with the ones from the analysis of teleconnection indices.

Additionally, since we were also searching the impact of baroclinic perturbations by

OSD

9, C1736–C1768, 2013

band-pass filtering of geopotential data, it seemed also natural to try to find them at the same mid-tropospheric level used to plot the rest of the Figure. The 500 hPa level has usually been selected to that end in several studies, some of them close to the area (Ulbrich et al., 1999). This was particularly relevant considering the vertical structure of baroclinic perturbations. Using a vertical level different from 500 hPa would lead to difficulties interpreting the results from synoptic perturbations and Z anomalies, since the spatial structure of the geopotential variance associated to baroclinic perturbations tilts with height (Wallace et al, 1988).

Barnston, Anthony G., Robert E. Livezey, 1987: Classification, Seasonality and Persistence of Low-Frequency Atmospheric Circulation Patterns. Mon. Wea. Rev., 115, 1083–1126.

Hsu, Huang-Hsiung, John M. Wallace, 1985: Vertical Structure of Wintertime Teleconnection Patterns. J. Atmos. Sci., 42, 1693–1710.

Thompson, D.W.J., and J.M. Wallace, 1998: The Arctic Oscillation signature in the wintertime geopotential height and temperature fields.Geophys. Res. Lett., 25, 1297-1300.

Ulbrich, U., Christoph, M., Pinto, J.G. and Corte-Real, J. (1999), Dependence of winter precipitation over Portugal on NAO and baroclinic wave activity. Int. J. Climatol., 19: 379–390.

Wallace, John M., David S. Gutzler, 1981: Teleconnections in the Geopotential Height Field during the Northern Hemisphere Winter. Mon. Wea. Rev., 109, 784–812.

Wallace, John M., Gyu-Ho Lim, Maurice L. Blackmon, 1988: Relationship between Cyclone Tracks, Anticyclone Tracks and Baroclinic Waveguides. J. Atmos. Sci., 45, 439–462.

However, in the case of the final version of the paper, if requested by the editor, we could add a panel showing MSLP composites (not just MSLP anomalies) and the text

OSD

9, C1736–C1768, 2013

would be rephrased giving more relevance to sea level pressure. However, part of the information is already included in Figure 10, since we can compare the difference of Z means (top row, black contours) with the difference of SLP anomalies (second row, white contours). Despite the westward displaced position of the center of activity of the negative mean with respect to SLP anomaly center (typical of a cyclonic perturbation as shown in the left panel of the map), the horizontal structure of both is similar. Additionally, those changes will also be reflected when the future directions of research already introduced in the discussion will be summarized.

Suggestions 3796, l. 14: “doesn’t have”, instead of “has not”.

Reply: This will be corrected.

3800, l. 19: the acronym PCA should be introduced here.

Reply: The acronym will be introduced here as suggested.

3801, l. 7: “The variety of the datasets used. . .”

Reply: This will be corrected.

3803, l. 12: the authors can add that, although the satellite data are not totally independent from the ICOADS in situ data, there is no other way to provide an estimate of the satellite performance.

Reply: This is already implicit in the sentence, so we will prefer to retain the sentence in the original form.

3803, l. 16: the authors should introduce the acronym WOD here.

Reply: The acronym will be introduced here as suggested.

3803, l. 18: what do the acronyms OSD, CTD and PFL mean? Otherwise, the authors

OSD

9, C1736–C1768, 2013

could simply mention “various instruments.”

Reply: The meaning of the acronyms will be explained in the sentence: OSD (Ocean Station Data: Bottle, low resolution CTD/XCTD), CTD (High Resolution CTD/XCTD), PFL (Profiling Floats).

3804, l. 7: although AVISO maps are produced daily, they don’t really provide information at the daily scale, but rather at the 5 to 10 day scale, due to sparse along- track measurements and long revisit periods. This time scale is still adapted to the authors’ study.

Reply: A comment saying that the daily products have their origin in 7 daily products will be added.

3807-3808, sentence in parentheses in between the 2 pages: I don’t understand that sentence.

Reply: The DINEOF technique has been applied to different configurations regarding the treatment of the originally non-missing values. One can force the technique to retain the originally non-missing values unchanged, or otherwise allow those values to change in the iterative process. The second option was used in our case in order to avoid sharp changes from reconstructed to originally non-missing nearby pixels. The impact of this choice is evaluated in the Figure 2 where originally non-missing values and their reconstructed counterparts are compared. That Figure then tells that the impact is small in terms of the BIAS and the MAD, as is explained in the document.

3808, l. 6-7: the authors should use left/right, and top/middle/bottom, instead of first, second, third etc. Otherwise, they could use letters to point to each subplot. This is the case for all figures.

Reply: Changes will be done in the next version of the document to refer to subplots as suggested by the reviewer. With regard to adding identification letters, this will be

OSD

9, C1736–C1768, 2013

done for the case of figure 10, in which this will most help the understanding (also following the suggestion by Rev2), but we do not think this is necessary for the rest of the Figures.

3808, l. 6-11: this is the figure caption. It shouldn't be in the text.

Reply: This will be removed from the text as suggested by the reviewer.

3808, l. 20: instead of "again", the authors could say "as in Table 1," and end the sentence with "evolution of the error parameters."

Reply: The sentence will be changed according to the suggestion.

3809, l. 6: "discrepancies between of satellite and in situ. . ."

Reply: "of" will be replaced with "between".

3810, l. 1-2: the authors could remove the end of the sentence about the 2002- 2010 period, it is understood with the use of "mainly" in the first part.

Reply: the sentence will be shortened following the suggestion.

3810, l. 15-16: "daily climatology" is used twice, in reference to Fig. 1, whereas the figure shows bi-monthly climatology (if I'm correct).

Reply: the Figure 1 shows the December-January mean of the 15-day smoothed daily climatology, as explained in the previous sentence (it is called the composed mean there).

3810, l. 19: Western Iberian Buoyant Plume

Reply: "Bouyant" will be corrected to "Buoyant".

3811, l. 19: what exactly are the previous patterns from Fig. 4 seen on Fig. 5?

OSD

9, C1736–C1768, 2013

Reply: that refers to the SST anomaly patterns shown in the right column of Figure 4. The sentence will be modified to make this clear.

3811, l. 22: can the authors provide examples of previous studies using monthly averages?

Reply: these are listed in the last column of Table 2, so a reference to that Table will be added to the sentence.

3812, l. 13: "to the North in along the French coast. . ."

Reply: the sentence will be modified following the suggestion.

3812, l. 20: the authors assume that the 1st EOF is related to a well developed IPC, not "at least partially". This is the main work hypothesis in the manuscript, it has to stand strong, and the authors have arguments to support this.

Reply: "at least partially" will be removed from the sentence as suggested.

3813, l. 5: by "that" data, do the authors mean infra-red SST, or in situ data?

Reply: it refers to both types of data, so this will be made explicit in the sentence.

3813, l. 27-28: it is not clear whether "the rest of the figures" refers to results by Herbert et al. (2011) or the authors'results.

Reply: it refers to the results by Herbert et al. (2011). The sentence will be modified to make this clear.

3814, l. 4: I disagree with the authors' statement that January 1997 SST map from Le Hénaff et al. (2011) doesn't show IPC-like signal. Fig. 5 from Le Hénaff et al. (2011) for that year shows a tongue of warmer water along the shelf break, very similar to Fig. 3a from deCastro et al. (2011) in 1989, which is considered a year with marked IPC.

OSD

9, C1736–C1768, 2013

This specific disagreement with the authors' results might be due to their methodology, which favors shelf-wide signature of the IPC.

Reply: The affirmation saying that there is no SST IPC-Navidad signal in the Figure by Le Hénaff et al. (2011) will be changed. From our point of view the disagreement comes from the fact that the SST signal is localized around the north-western corner of the Iberian Peninsula (also in the case of the Figure by deCastro et al., 2011), not entering the inner Bay of Biscay, and hence in not the case represented by the EOF in Figure 6. The reason of this discrepancy, according to our view, will be added to the revised manuscript.

3814, l. 10-11: it is not clear why the authors mention again Torres and Barton (2006), since that study was already mentioned before. Then, the authors should precise what the agreement with Le Cann and Serpette (2009) is,; in my view it is an early development of the IPC in the 2006 episode, well seen in the authors' time series.

Reply: Torres and Barton (2006) is referenced twice because, first the SST images from that work are compared with our time-series (Figure 7), and then in-situ measurements from that reference are compared with our time-series. A comment following the agreement in relation to the early development will also be added.

3814, l. 21-24: the authors should precise what the disagreements are, esp. for the 1999 case. Otherwise this part is quite useless to the reader.

Reply: A sentence will be added by the beginning of the comparison with previous works stating that the comparison is made on the base of visually comparing the results in the references (i.e. presence of SST signal or warm water) with the values of our time series fro the date being compared. This of coarse is not ideal but given the number of references that are taken into account, and the availability of the data shown in those references, this is the only way to do it. So, this will be made explicit by the beginning of the text dealing with the comparison. Take also into account that the entire comparison

OSD

9, C1736–C1768, 2013

will be rewritten following a suggestion by Rev1.

3815, l. 5-8: what are the acceptable level of agreement, and the disagreement, mentioned by the authors?

Reply: This will be reworded; see previous reply.

3815, l. 8-10: what is the point of the sentence saying that no comparison is needed with Le Cann and Serpette (2009), since it doesn't say anything about the results, and in addition that paper has already be mentioned in the previous paragraph? The comparison with previous studies is a bit confusing sometimes.

Reply: That part of the document deals with the comparison on interannual T/S variability estimations. Le Cann and Serpette (2009) contains one of such estimations but as it is based on the previous works mentioned in that sentence, and as those have been already compared with our time series, the points that no comparison is necessary is made explicit. We will remove that statement in the revised version of the document.

3815, l. 20: I suggest to use "most developed", instead of "best developed".

Reply: Suggested correction will be applied.

3815, l. 27-29: I don't understand the sentence starting with "as Torres and Barton. . .".

Reply: The sentence will be modified this way in order to make in easier to understand: "Torres and Barton (2006) found that the major poleward signal was located off the slope (characterized by the 200m isobath), so only vertical profiles located off the slope will be considered."

3816, l. 14: "for area I" instead of "for the I area". In general, the authors should use

OSD

9, C1736–C1768, 2013

“area I”, “area II” etc. without “the” in front.

Reply: All occurrences will be corrected.

3816, l. 29: “, and” instead of “but”.

Reply: The sentence will be modified following the suggestion.

3817, l. 7: in general, the authors should use “shelf” rather than “platform”.

Reply: All occurrences will be corrected.

3817, l. 20: the authors could indicate that the corner of the BoB is in area III.

Reply: this will be added to the sentence.

3819, l. 23-25: the beginning of the sentence is awkward, with the use of “analysis” twice. Then, the meaning and purpose of the sentence is not clear.

Reply: A major change for the first four paragraphs in the Section 3.3 is proposed in Section 1.3 in General Comments of this document.

3820, l. 3: the authors should precise here that the term “each variable” refers to atmospheric fields from the reanalysis.

Reply: As pointed by the reviewer the term “each variable” refers to the ERAInterim variables, but also to the AVISO variables. In order to avoid possible confusions “(from ERAInterim or AVISO datasets)” will be added after the “each variable”.

3820, l. 14-15: I don’t think that the wind-stress anomaly curl comes from AVISO.

Reply: the sentence will be modified in order to avoid confusions like the one pointed by the reviewer.

OSD

9, C1736–C1768, 2013

3820, l. 21-29: this is the figure caption; it shouldn't be in the text.

Reply: part of this paragraph may be redundant to some degree but it offers additional information that is not given in the caption of Figure 10 due to length limitations (the caption is already very long). So, although we agree with the reviewer that these lines should not be in the text, They will be retained. Anyway, if the reviewer or the editor judges we should place them in the caption instead of the text we would have no problem to do so.

3821, l. 5: "west-southward" is not very clear: is it southwestward, or westward then southward?

Reply: it is "southwestward", so the sentence will be modified including this term.

3821, l. 21: probably 35âŮę N, instead of 25âŮę N.

Reply: The was an error there as it should have said "25W−−50N" instead of "25N−50N". This will be corrected in the revised document.

3821, l. 25-26: what is the "surface centre of action"? It sounds exciting.

Reply: the surface centre of action term refers to the maximum negative anomaly area, for the SLP in the case of the surface level, while for the geopotential height in the case of the 500 hPa level. This will be added to the sentence to make it clearer.

3822, l. 5: SLA instead of SLP.

Reply: this error will be corrected.

3822, l. 15: if mentioned, Cabo Peñas should be marked on a map. But that precision is probably not necessary.

Reply: "Cabo Peñas" will be replaced with "Aviles canyon" (see comment by Rev2).

OSD

9, C1736–C1768, 2013

Instead of adding the location on a map, its coordinates will be added just after it is mentioned: Aviles Canyon (6.32 °W-43.92 °N).

3822, l. 17: ". . .coast of Britanny coast"

Reply: the sentence will be corrected.

3822, l. 20: the anomalies of atmospheric surface fields were significant until the 60-75 day lag (previous paragraph), so it is not exactly true to say that SLA is significant until 75-90 days "like in the case of previous variables".

Reply: this is right so "like in the case of previous variables" will be removed.

3822, l. 23-24: "which is related to related . . .": there is one too many.

Reply: redundant "related" will be removed.

3823, l. 2: what is the unit of the ADT gradient?

Reply: The values of the gradient are dimensionless. They represent the division of the difference of the height between the the two bands used in the computation by the distance between the two bands (m/m). If those should better be given in radians (angle) we would make the change in the revised document (will be the same values as $arctan\theta \approx \theta$ in that case).

3823, l. 23-24: I don't think that the SST pattern is very consistent with those from SLA and heat flux. The negative patch in the middle of the Atlantic is quite consistent, but the signature in the BoB and on the European shelf shows marked local differences. In particular, SLA and heat flux already have different patterns in the NE Atlantic, with heat-flux positive patch being large scale while the SLA one being specifically located on the European shelf. Still, I agree there is a general agreement, especially at the 0-15 day lag.

OSD

9, C1736–C1768, 2013

Reply: The disagreement pointed by the reviewer will be explicitly stated in the document, stressing at the same time that the overall agreement is good.

3825, l. 16: what do the authors mean by the “instability of the flow over this area”? This is quite vague.

Reply: It referss to the baroclinicity over the slope area, so this will be more precisely pointed in the new sentence.

3825, l. 22: the authors should use “not as good” instead of “worse”; the agreement with previous studies is generally good, not bad (!).

Reply: Following the suggestion by the reviewer “worse” was replaced with “not as good”.

3825, l. 24-28: the disagreement with Garcia-Soto (2004) for the winter 2001/2002 could be tempered by the fact that there is no indication, in that paper, about the number of images the author used to derive his estimate.

Reply: This comment will be added to the next version of the document.

3825, l. 29: “. . . compared to the rest other years”

Reply: “of the years” will be added by the end of the sentence to clarify it.

3826, l. 21-22: regarding the early development of the IPC, the authors could mention Le Cann and Serpette (2009), who studied the 2006 case.

Reply: A reference to the onset process for the 2006/2007 will be added .

3826, l. 27-29: it is not clear how the present study supports thorough studies of early IPC development. Do the authors mean to say it has been observed in several occurrences in their series, and not much studied before?

OSD

9, C1736–C1768, 2013

Reply: The present study does not support such studies. What the sentence intended to say was that Figure 7 points that such studies would the interesting. The sentence will be modified to avoid possible confusions.

3828, l. 4-5: what is exactly the authors' "working hypothesis"? Is it the ability to derive the IPC time series from long-term reconstructed SST maps?

Reply: that is correct. In order to make the sentence clearer the term "working hypothesis" will be removed and the sentence modified.

3829, l. 3-4: again, I don't think the correspondence between the heat-flux and SLA anomaly patterns is very good, because the SLA positive patch is much more marked on the European shelf, compared to the heat flux one, which extends over the whole NE Atlantic.

Reply: This will be answered in a previous comment (reply to last general comment about the discussion).

3829, l. 15-16: again, I find differences on and along the shelf between the SST and the patterns from Fig. 10. The shelf area is the key area for the IPC signature. The agreement is not very good, although there is a general agreement at the large scale.

Reply: This point will added to the manuscript, also pointing that the cause could be the use of a time series (Figure 7) that is deduced from a limited area (area in Figure 6).

3832, l. 17-19: the authors should precise that the PCA analysis was derived on the shelf and shelf break area.

Reply: (p 3831, l. 17-19) following the suggestion "restraining the SST to shelf and shelf-break areas in both cases" will be added by the end of the sentence.

OSD

9, C1736–C1768, 2013

3832, l. 19-10: again, I don't think the 1st EOF necessarily corresponds to the IPC signature one would expect; the approach followed by the authors favor an IPC signature over the whole shelf.

Reply: That sentence will be removed. See, also, Section 1.2 in General Comments of this document.

3843, Fig. 4: can the authors provide the number of days with observations used in each year? And also the definition of the flag for the Pathfinder data (if more than one image is used)?

Reply: Following this suggestion, and another one by Rev2, a new version of Figure 7 is given in this document. This new Response Figure 3 should not be included in the revised version, as we think that the original version is clearer ,but as it is already made it could easily be introduced in the final version of the manuscript. This new Figure is very similar to the original one but in this case the monthly standard deviations and the number of observations in each month have been added to the Figure. Finally, the caption of the Figure 7 will be modified to indicate the meaning of the flag 7 quality data.

3846, Fig. 7: the meaning of the horizontal dashed lines should be in the caption.

Reply: The meaning of the horizontal dashed lines will be added added to the caption.

3849, Fig. 10: on the "Z" figures, the values on blue and red contours cannot be read. One cannot see the 200m isobath on SLA & V figure (bottom). Some information is missing in the figure caption, whereas it is in the text. It should be the other way around.

Reply: Figure 10 will be modified so that the values on blue and contours and 200 m isobath can be clearly seen in top an bottom rows respectively. In the case of this Figure, and as the cation is already very large, we would prefer not to add additional information there (a reference is already present in the caption noting additional infor-

OSD

9, C1736–C1768, 2013

mation id given in the text). However, if the editor or the reviewers think this should be changed we would not any inconvenience.

OSD

9, C1736–C1768, 2013