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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.

G. Esnaola et al.

gesnaola001@ikasle.ehu.es

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Interactive Comment

Manuscript os-2012-118 Response to comments by reviewer 2: “Daily scale winter-time sea surface temperature variability and the Iberian Poleward Current in the southern Bay of Biscay from 1981 to 2010”

G. Esnaola, J. Sáenz, E. Zorita, A. Fontán, V. Valencia, P. Lazure

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 Rev2 are answered.

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

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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

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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-

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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

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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.

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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

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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.

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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 2 Comments

The is an excellent paper on the poleward ïňĆow on the slopes (and shelf) around Northern Spain. It captures a reconstructed SST structure of the winter ïňĆow as an anomaly time series from 1981 to 2010 and derives the meteorlogical variables that force years with enhanced ïňĆow. The main driver for the SST positive anomalies is shown to be a low atmospheric pressure anomaly in the N Atlantic to the west of Europe with a centre near 25W, 50N. The associated wind stress and heat ïňĆux patterns are given. The wind stress will force a ïňĆow pattern directly or indirectly (through density re- adjustment) and altimeter data are used to show the slope currents off Portugal and in the southern Bay of Biscay that correspond with the SST winter warming pattern. Excellent, I strongly recommend publication of Esnaola et al, subject to reference to some papers outlined

below, some inserts in Table 2, a few minor oceanography corrections, a few incorrect sentences removed, and that the authors show that they know the difference between

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SST / PIC (particular at page 3813). The paper is very well written. In terms of balance, although I liked the reconstruction analysis and the SST result, ïňĄg 7, I would have preferred relatively more words and appraisal of the scientiïňĄc ïňĄndings or deductions and have made some suggestions under CONCLUSIONS. Overall rating, very good. Many points are listed below. Most can be taken care of with an appropiately placed reference. Some are just information which may help interpretation of data. These are largely put under Figures below as that is where the results can be seen and appraise more critically. The comments are intended to be helpful, showing sometimes an alternative view or giving a bit of extra information that is not easy to glean from the literature, or something that is clearly incorrect.

Title. The deïňĄnition of the Navidad is ‘’the extent of winter SST warming that results from the slope current in the Bay of Biscay along Northern Spain”. This SST anomaly comes near New Year and ‘belongs’ (is attached to) to Spain, hence the convenient shortened name ‘Navidad’. A shortened title might read .. ‘’Navidad variability from 1981-2010”. Making everthing Iberian (PC) can leads to some misinterpretations (and some don’t know where it is). For example, Navidad refers to SST and this is not quite the same ‘thing’ as the slope current. The temperature can result from displacement, or current integration, so SST maximum will not be coincident with ïňĆow maximum. In general the SST will lag the poleward current and near maximum temperatures can be sometimes observed when the ïňĆow has stopped. If it is IPC along Spain at what point does it stop being IPC in French Waters on the Armorican slopes? Isn’t a lot of the ‘real’ IPC already lost off NW Spain?

Reply: See Section 1.1 in General Comments of this document where this point is discussed.

Give an estimate for the IPC slope transport (and ref if already in literature), it is not difïňĄcult. See satellite images (ïňĄg 17) shown in Pingree and Le Cann 1989 showing loss off NW Spain for example (put in Table 2). These authors also gave the increasing

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(collected with latitude, with wind stress and dynamic height) poleward slope transport off northern France as 2Sv and 7Sv as far north as 60N as early as 1989. Inertial Overshoot occurs off other promontories, Goban Spur, Cape St Vincent, as well as NW Spain.

Reply: For the slope transport related to the IPC see comment by the end of the document. The SST images (Figs 17a, 17b) in Pingree and Le Cann (1989) will be taken into account in the section that deals with the comparison of our IPC-Navidad time-series in Figure 7 and the detections of that SST signal in the bibliography. Accordingly, that reference will be added to Table 2 in the manuscript. The slope transports estimated by those authors will also be added to the INTRODUCTION section. With regard to the Inertial Overshoot, we kindly request from the reviewer additional clues so that we can elucidate which is the purpose of that suggestion.

Abstract. An oceanographer likes to think the surface conditions drive the ocean, cf 500hPa.

Reply: The abstract will be clarified in order to better explain the proposed mechanism for the variability of the IPC-Navidad. This regard to the possible confusions that the inclusion of the atmospheric 500 hPa level in the analysis, the reason is explained in the following. We agree with the view that the surface atmospheric conditions drive the the ocean, but these atmospheric surface conditions are not independent of the atmospheric circulation anomalies on higher levels. In fact, as it is shown in the document, the anomalies of the surface atmospheric variables are related to a circulation anomalies on higher levels, like the 500 hPa level selected here. We have made an explicit use of the 500 hPa level anomalies in our analysis, but it has to be stressed that such anomalies have also implicitly and widely been used in the bibliography on the IPC-Navidad. Many contributions in the bibliography on this topic have paid considerable attention to the problem of the relation of the IPC-Navidad and the atmospheric teleconnection patterns of the North Atlantic, specially the North Atlantic Oscillation (NAO).

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Most of those contributions make used of the teleconnection patterns and related standardized time series obtained from the NOAA-CPC (Climate Prediction Center) for the mentioned analyses. Such teleconnection patterns are deduced from the atmospheric 500 hPa geopotential height anomalies using a Rotated PCA analysis. In our case we have preferred to directly use such anomalies instead of the indices corresponding to such teleconnection patterns, as they did not show to be of practical use in our case (see Section 1.3 in General Comments of this document). So, taking into account the previous lines, it can be concluded that although our analysis based on the anomalies of the atmospheric 500 hPa level could seem confusing, it is not essentially very different from what can be found in the bibliography on this topic, since CPC teleconnection indices are actually derived from geopotential anomalies at the 500 hPa isobaric surface.

Introduction. -Page 3798 Line 29. These references, Garcia-Soto & Pingree (2012) (which extends the AVHRR satellite observations of the IPC to 2010) and the earlier one by Garcia-Soto, Pingree & Valdés (20002) (covering 30 years of high resolution AVHRR satellite observations (1979-2010)) should be mentioned early in the INTRODUCTION. They cover and analyse the same oceanographic structure (SST/IPC) for the same period of time (1981-2010) as in this Esnaola manuscript. I suggest with a sentence added after line 15 page 3797 saying . . . This paper (Esnaola) examines the meteorological forcing for the SST variability for the period . . . analysed earlier . . . insert refs 2002 and 2012.

Reply: Garcia-Soto & Pingree (2012) and Garcia-Soto, Pingree & Valdés (2002) references will be added to the INTRODUCTION section. A similar sentence to that suggested by the reviewer will also be added to the same section.

3796 25 after oceanic area give ref for anticyclonic from direct current measurement (eg Deep-Sea Research 1993, vol 40 369-388)

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Reply: This reference will be added as suggested by the reviewer.

3797 1 after wind-induced currents give Prog Oceanogr 1989 vol 23 303-338 ref otherwise it looks like everything was done by Koutsikopoulos and Le Cann in 1996.

Reply: This reference will be added as suggested by the reviewer.

3797 5 bay >Bay

Reply: The sentence will be corrected as suggested by the reviewer.

3797 10 the poleward ïňĆow in the Bay of Biscay (called here IPC) and Navidad (this paper) was ïňĄrst measured by Pingree and Le Cann 1990

Reply: “, and was and first measured by Pingree and Le Cann (1990)” will be added by the end of this sentence.

3797 24 but the wind stress causes the ocean density structure change, the balance is upset (a weakened Gyre for example, see your ïňĄg 10). Also the local wind at a point does not produce the current at that point. So say how this 1/5 quantitative current value of IPC was scientiïňĄcally derived or remove this sentence.

Reply: The sentence will be removed as suggested.

3798 23 the decay is derived analytically in Pingree and Le Cann 1990 and even more thoroughly in Cont Shelf Research 1999 19 929-975 (insert a ref with Peliz). This paper also discusses SOMA, the seasonalty response shown with year long measurements eg Pingree and Le Cann 1990. Give a ref for IPC seasonality off Portugal from current measurement.

Reply: Those two references, Pingree and LeCann (1990) and Pingree et al (1999), will be added to the part of the sentence dealing with the decay phase. For the reference to the SOMA see comments latter. Finally, we kindly request the reference pointed by

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the reviewer in order to introduce it in the manuscript.

3798 28 see also JMBAUK 1994 74 107-128 shows Navidad time series from 1967-1993.

Reply: Reference will be added to the sentence, and “AVHRR” replaced with “satellite” in order to retain consistency after adding the new reference.

3799 19-21 remove this sentence, not correct.

Reply: The sentence will bee removed as suggested.

3800 5 satellite images of complete Navidad ïňĄrst shown in Pingree and Le Cann 1989?

Reply: Pingree and Le Cann (1989) will be added to that list of references in relation to their Figures 17a and 17b.

3800 20 you are analysing the SST or Navidad not the IPC which is not the same if you examine lonterm records. Give or a ref for longterm IPC mooring showing that SST and IPC are completely in phase, what did the ïňĄrst IPC deïňĄning observations say about this?

Reply: For the question dealing with the IPC and its surface signal or Navidad, see comments in Section 1.1 in General Comments of this document. For the rest of the comment, we kindly request the reviewer for additional help in order to fulfill those suggestions, particularly, we would need he/she to be more concrete about the reference that he/she asks us to cite. In addition, the sentence will be modified from “to identify the IPC signal on the SST ” to “to identify the surface signal of the IPC on the SST images ”. Effectively, we are talking about the surface signal of the IPC, Navidad, IPC-Navidad... and not about the IPC itself, as those do not have to be in phase as shown by Fig 118 in Pingree (1994).

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3801 5 really it is atmospheric , the ocean conditions eg SLA are a response.

Reply: The sentence will be modified replacing the “atmospheric and oceanic conditions that enhance” with “atmospheric conditions that enhance”.

3803 18 spell out

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).

3803 27 removing the daily climatology. Explain more fully with another sentence.

Reply: The sentence will be modified adding “(daily seasonal cycle)” after the “daily climatology” in order to clarify what the daily climatology means.

3807 16 worsen?

Reply: “worsen” will be replaced with “does not have a negative impact in” in order to make the sentence clearer.

3808 14 westernmost? Give boxes I II III IV V for a location clue.

Reply: “westernmost area” will be replaced with “south-western corner” in order to clarify the sentence. The reference to the boxes is not justified in this case from our point of view, and in addition the area V (the box nearby the location being mentioned) will be removed from the analysis in the revised version as explained in Section 1.4 in General Comments of this document.

3810 8 mean climatology, add another sentence

Reply: “running mean climatology” will be replaced with “running-mean climatology” to make clear that is a “running-mean” of the climatology and not a running “mean

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climatology”.

3809 25 resemble well >match

Reply: This will be corrected.

3810 18 actually NE winds . This is expected as there is more Spanish than Portuguese slope in boxed regions.

Reply: Although this sections does not deal with the boxed sections, the comment on the NE winds is still valid and will be added to the sentence.

3810 27 1987 Jan (12.5C in BoB) without a surface signiture of Navidad (it is temp not current IPC) and other earlier years (compares well with your ïňAg 7) shown plotted in Fig 2 of JMBAUK 1994 94 107-128, see also 1990 (14C), max of the 1967-1993 period plotted.

Reply: We don’t understand reviewer’s point here, so we would need additional information in order to understand what the reviewer is asking to include/modify/correct.

3811 4 remove yr

Reply: This will be corrected.

3811 14 see ïňAg 5 caption date error

Reply: No such error could be found...

3812 9 on>of 3812 10 on>in 3812 13 in>near

Reply: First one will be corrected, second one misplaced?, third one alternatively “along” instead of “in” (also following suggestion by Rev1).

3813 First para see comments on Table 2 below.

Reply: Answered later.

3813 15 1991 is measured 15 month record! rig 129, weak (the in situ measurements exist) but comes mid Jan (see JMBAUK 1994), min Sept Oct (SO)and min again Mar April (MA). This is the SOMA signal also very evident for 1989 (rig 118), a strong Navidad. Does the IPC/Temp off Iberia show SOMA?

Reply: The point in this section is to underline that there might be some weaknesses in the estimation of the time evolution given in Figure 7. That is why the text lists the discrepancies with the estimates by Garcia-Soto et al (2002), not to question the results by those authors. Accordingly, a comment has also been added pointing that a weak development was described by Pingree (1994) by means of current measurements, confirming the results by the previous authors for 1991. With regard to the SOMA, we are not able to answer the question by the reviewer as the moths selected in our analysis (Nov-Dec-Jan-Feb) do not include the Sep-Oct and Mar-Apr months. The selection of the months used in our analysis was constrained by the PCA analysis used to deduce the variability mode shown in Figures 6 and 7. The inclusion of additional months (Sep-Oct or Mar-Apr for example) has an strong effect on the patterns obtained from the PCA analysis, and the consequence is that the pattern analysed here is not obtained any more. However, the problem regarding the Sep-Oct and Mar-Apr moths is an interesting problem for future studies extending the results obtained here, perhaps with different methodologies.

3813 15-25 rewrite. IPC/SST are not the same thing. A weak Navidad can have a strong signal SST at NW Spain ONLY; in a strong Navidad year the SST signal reaches 4W. So does a strong SST at NW Spain mean a weak IPC or a strong IPC? What is the deïňĄnition for the IPC at NW Spain? When was the term ïňĄrst deïňĄned and used? Give reference. Where are the ïňĄrst current measurements in the BoB, see Title of manuscript. At line 16, January 1999, Give date for SST satellite image showing structure along the Spanish slope to say 4W, G-S&P (2012) say no marked SST

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along Spanish Slope. At line 19 does this mean Torres and Barton also confused SST development along the Cantabrian shelf with IPC SST only well marked off NW Spain? At line 22, noisiest, give standard deviation (Fig 7). Give mean standard deviation of other years. At line 24, it is more than a null value? What has gone wrong?, images for 12 Jan 2001, and 21 Dec 2000 show structure (Table 2, G-S&P, 2012). Remove offending sentence or rewrite.

Reply: See comments in Section 1.1 in General Comments of this document for the responses to many of the questions made above. The entire comparison of the bibliography will be rewritten to correct the errors made in those paragraphs in relation to the mixing of the IPC and its surface signal. Whenever comparing the IPC-Navidad time series with a source of data characterizing the IPC and not its surface signal this will be explicitly quoted and a word of caution will given. This will be the case also when comparing the time series with western Iberia and NW Spanish coast occurrences. With regard to the standard deviation comment, a new version of Figure 7 is shown here in the response document. This Response Figure 7 is similar to Figure 7 in the document but the monthly standard deviation and the number of reconstructed days per month have been added. We prefer the original version as we think it already has enough results and is clearer, but if the editor or the reviewer thinks the one given here is better we would have no problem to make the replacement and the necessary text corrections. We rest of the comments will be taken into account when rewriting this part of the text, and the offending sentence will be removed.

3814 9 entering the BoB where it is called NAVIDAD.

Reply: “where it is called Navidad, IPC-Navidad here” will be added to the revised document. Also, see comments in Section 1.1 in General Comments of this document.

3814 (line 11) to page 3815 (line 15) Esnaola reviews the interannual variability of the IPC described in previous studies but again the most recent work by GS-&P (2012)

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with satellite an in situ observations up to 2010 is not mentioned. The authors should correct this. GS&P (2012) mentioned the following years of strong Navidad: 1979, 1982, 1984, 1988, 1989, 1990, 1996, 1998, 2001, 2003, 2007 and 2010 (January years). This makes the sentence at (page 3815 Lines 3-4) look very odd! Rewrite this section as others knew about these years, also check your Table 2 for consistency.

Reply: That reference will be added to the comparison, the mentioned sentence will be removed and Table 2 will be checked for consistency.

3814 26 IPC (=Navidad) not really correct as stated again, Navidad is temp and IPC is poleward current, the PC bit of IPC by deïňĄnition.

Reply: See Section 1.1 in General Comments of this document where this point is discussed, and also the reply to the 3813 15-25 comment.

3815 1988/1989 measured at rig 118, 9 month record , data show only strong events only in Nov/Dec and Jan/Feb at 7W.

Reply: The Navidad-IPC time series will also be compared with the record found in Pingree (1994), and that reference will also be added to the Table2.

3815 27 sentence starting As Torres . . . doesn't read quite

Reply: The sentence will be modified in order to make it easy 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 11 Spanish coast

Reply: The sentence will be changed including "Portuguese and Spanish coasts" instead of "Iberian coast".

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3817 2 nevertheless Area I is just warmer and saltier than Area V? so no too good as a source region? You would need a Lagrangian track for this conclusion based on proïňĄles.

Reply: area V will be removed from the analysis. See Section 1.4 in General Comments of this document.

3817 10 I have put my comment below under Fig. 9. Conclusions should be based on direct measurements, I have given some refs. Alternatively give position and ref of a winter mode water with T 11C and S 35.6psu (ENAWp) in any subpolar region. What latitude do you consider subpolar?

Reply: Ideally, Sverdrup's two lines for NACW are different for western (WNACW) or eastern (ENACW) waters, due to the effect of mediterranean waters. For ENACW there exists significant mixing with mediterranean waters. Conversely, for WNACW, the lack of mediterranean influence means that lower values of salinity and temperature can be found at shallower depths until mixing with subartic waters happens. In order to further clarify this point, we will cite the following references in the final version of the paper, since they use similar values as the ones that we use in our study.

McCartney, M.S. and L.D. Talley, 1982. The Subpolar Mode Water of the North Atlantic Ocean. J. Phys. Oceanogr., 12, 1169-1188, their Figure 1.

In reference to Pollard, R. T., M. J. Griffiths, S. A. Cunningham, F. R. Read and F. F. Perez (1996). "Vivaldi 1991 - A study of the formation, circulation and ventilation of Eastern North Atlantic Central Water." Prog. Oceanog. 37: 167-192. We can consider from our paper ranges as the following ones (T in °C, salinity and σ_t) 11.00, 35.500, 27.166 and 11.00, 35.600, 27.243. We see that $\sigma_t = 27.243$ is considered inside their group 4.5 (Density range 27.20-27.30). See also their Table 1 (Central Water Salinities).

Section 2.2 in "HYDROGRAPHY OF THE SOUTHERN BAY OF BISCAY SHELF-

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BREAK REGION: INTEGRATING THE MULTI-SCALE PHYSICAL VARIABILITY OVER THE PERIOD 1993-2003", JOURNAL OF GEOPHYSICAL RESEARCH 111, C09021 by Llope et al. (2006) mentions similar values (11°C to 13°C and 35.53 to 35.74).

See also: WORTHINGTON L. V. (1981) The water masses of the World Ocean: some results of a fine scale census. In: Evolution of physical oceanography. B. A. WARREN and C. WUNCH. editors, The MIT Press. pp. 12-60.

In order to point out the fact that this happens at the latitudes covered by our study, we cite the following paragraph from "Water masses in the upper and middle North Atlantic Ocean east of the Azores", by Ríos, et al. (1992), Deep Sea Research Part A. Oceanographic Research Papers 39 (3), 645-658.

"The thermocline of NACW is defined in different ways and with different nomenclatures. The introduction of subpolar mode water formed in the northeastern cyclonic gyre (McCARTNEY and TALLEY, 1982) produces Eastern North Atlantic Central Water (ENAW) defined by HARVEY (1982) for the large volumes of central water formed north of 46N, with temperatures and salinities between 4°C, 34.96 and 12°C, 35.66. We call this water ENAWp (for subpolar). The introduction of these mode waters is an improvement in the T-S relationships given for NACW by SVERDRUP et al. (1942), since it differentiates between two different water bodies of NACW in this area. As these mode waters advance or diffuse to the south-southeast they form a front with the MW that restricts its presence in the southeast North Atlantic (POLLARD and Pu, 1985. Fig. 3; COSTE et al., 1986)."

The fact that the intergyre area covers the latitude band 40N-50N is further clarified in Figure 12 by Curry and McCartney (2001) "Ocean Gyre Circulation Changes Associated with the North Atlantic Oscillation". J. Phys. Oceanogr., 31, 3374–3400.

Additionally, we mention that from 20W, salinity on the 27.1 kg m^{-3} isopycnal surface increases progressively to the east rather than decreasing from the effects of coastal

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runoff (Pollard and Pu, 1985; Pérez et al., 1995). The spatial trend is similar in different years despite interannual variability in the TS characteristics. Pérez et al. (1995), from the database of several oceanographic cruises in the North Atlantic, showed a temporal variation in the T-S relationship and the evolution of salinity on the isopycnal $\sigma_t = 27.1$ kg m^{-3} at 42N, 10W.

R.T. Pollard, S. Pu, Structure and circulation of the Upper Atlantic Ocean northeast of the Azores, Progress in Oceanography, Volume 14, 1985, Pages 443–462.

Pérez, F.F., A.F. Rios, A.K. Brian y R.T. Pollard .1995. Decadal changes of the q-S relationship of Eastern North Atlantic Central Water. Deep-Sea Res. I, 11/12, 1849-1864.

Therefore, we can safely assert that we have used the values according to common intervals mentioned by other authors in existing literature.

3817 24 is this convincing? the broad 14C to 16C distribution is saltier in Area 1? Why would properties mix along the ENAWt –ENAWp line?

Reply: It is clear that the T/S diagram represents a sampling over extremely different cases that include some situations characterized by a strong positive salinity anomaly over the thermocline (late eighties and early nineties). Additionally, we must mention that Eastern North Atlantic Central Water (ENACW) is a Winter Mode Water formed by winter cooling and deep convection (Pollard, R.T., M.J. Griffiths, S.A. Cunningham, J.F. Read, F.F. Pérez and A.F. Ríos, 1996. Vivaldi 1991- A study of the formation, circulation and ventilation of Eastern North Atlantic Central Water. Progress in Oceanography, 37: 167-192.). Therefore, we can not expect that values repeat unchanged from year to year. Considering that, we can also assume that, besides the inter-annual variability there exists a certain degree of isopycnal or diapycnal mixing, depending on the energetics of the mixing or the structure of the thermocline. However, it is clear that isopycnal mixing does not represent the whole range of measurements shown in the

TS diagram.

3819 Actual atmospheric pressure is not single valued wrt NAO etc or the same NAO can have different pressure distributions so correlations are never going to be perfect. Monthly NAO is variable and can miss situations between months or at 15 day intervals? In JMBAUK 2005 85 1301-1315 NAO was correlated with N Atlantic circulation based on large scale SLA gradients like the ones seen in your ïňĄg 10 and the results gave values as high as r -0.8 (for ïňĆow SLA difference between the subtropical and subpolar gyres (a broad NA Current) lagging the NAO by as much as 6 months. So as is obvious NAO is going to correlate differently depending on spatial scale and temporal scale situations. We could use the daily met variables over the N Atlantic but then a lot of time series analysis.

Reply: We think that instead of using teleconnection indices, the analysis is better performed using actual data at different grid points. In fact, we agree with the reviewer that this means that a lot of statistical analysis must be performed (we, actually, performed them and some of the results are shown in Figure 10). The advantage is that simple indices such as the NAO or other teleconnection indices are deduced from geopotential anomalies (usually at monthly time scales but also at daily time scales, see Blessing et al, 2005) subject to some mathematical constraints such as orthogonality or rotation or EOFs. Although they are extremely useful for several applications, they are not able to explain all the variability of additional fields such as heat fluxes and that is the reason that we preferred to perform the diagnostic using physical fields from the Reanalysis.

SIMON BLESSING, KLAUS FRAEDRICH, , MARTINA JUNGE, TORBEN KUNZ and FRANK LUNKEIT, 2005, Daily North-Atlantic Oscillation (NAO) index: Statistics and its stratospheric polar vortex dependence, Meteorologische Zeitschrift, Vol. 14, No. 6, 763-769

3819 19 ‘complex’ . . . change sentence to show that others already have some

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understanding of the situation, see above.

Reply: See section 1.3 in General Comments of this document.

3819 20 daily? See above

Reply: Yes, the composite analysis is based on daily data and we think the diagnostic is better performed at this time scale, as already discussed above.

3820 6 The subtraction means that you might have found the situation (-ve) for the occurence of the P20, an upwelling situation perhaps. Also how linear is the system?

Reply: That is perfectly possible. See also the comments on the asymmetry of the positive and negative phases of the IPC-Navidad in terms of the SST signal in Section 1.2 in General Comments of this document. For the last version of the paper we can check this computing also linear (Pearson) correlation maps in order to identify this issue.

3820 16 Figure 10. I have put my comments under Fig.10 below.

Reply: Answered later.

3820 28 you have to give values/units for wind stress curl

Reply: Units of the wind stress curl (Nm^{-3}) will be added to the sentence.

3821 It seems to me that the pressure anomaly explains most of the SST.

Reply: As it well established in bibliography (Cayan, 1992a; Cayan, 1992b; Zorita et al, 1992; Esnaola eta al, 2012; to cite some), the surface heat-flux anomalies (latent and sensible heat-fluxes, or turbulent heat-fluxes) are the major drivers of the SST anomalies and not the SLP anomalies. The heat-flux anomalies (positive/negative) are related to meridional atmospheric surface level transports (northward/southward)

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of humidity and heat, which in turn are related to the SLP anomaly.

Cayan, D. R., Variability of Latent and Sensible Heat Fluxes Estimated using Bulk Formulas, Atmosphere-Ocean, 30(1), 1–42, doi:10.1080/07055900.1992.9649429, 1992a.

Cayan, D. R., Latent and Sensible Heat Flux Anomalies over the Northern Oceans: The Connection to Monthly Atmospheric Circulation, Journal of Climate, 5(4), 354–369, doi:10.1175/1520-0442(1992)005, 1992b.

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.

Zorita, E., V. Kharin, and H. von Storch, The Atmospheric Circulation and Sea Surface Temperature in the North Atlantic Area in Winter: Their Interaction and Relevance for Iberian Precipitation, Journal of Climate, 5(10), 1097–1108, doi: 10.1175/1520-0442(1992)005, 1992.

3822 10 If you analyse the mean seasonal set up across the NW European margin you will ïňĄnd it is a maximum in Nov.

Reply: The composite analysis does not have any preferred date or months. The lags apply to the full period, and there seems to have been a misunderstanding by the reviewer (note about November). Dates all over the Oct-Feb period can be in a P20 or P80 data set and the lagged composite analysis shows the conditions in the previous days before a date in that period. For example for a 0-15 day lag example, is January 15 is in one of those data sets, then the period January 1-15 is taken into account in the composite and not the 0-15 period before November when the data used in the analysis begins.

3822 15 Cape Penas . . . this is Aviles Canyon region and slope ïňĆow is disrupted as

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off Lisbon, Setubal and Lisbon Canyons. At Cap Breton and Cap Ferret it's the same? With the production of Swoddies.

Reply: "Cabo Peñas" will be replaced with "Aviles Canyon" and its location will be given (see comment by Rev1)

3823 2 slope is given a unit 'rad'? These values are 10x smaller than annual means towards the slopes off Cape St Vincent. 2 year record here with 400 day slope current mean at 480m (Rig 149) shown in Pingree 2002 (ïňAg 13). This record also shows continuous slope current overshoot with ïňĆow not able to make the corner of the promontory.

Reply: The values of the slope 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 almost the same values as $arctan\theta \approx \theta$ for the low values found in that case).

3823 5 their Fig 9?

Reply: "Fig. 9" will be changed to "their Figure 9".

3823 4 make clear whether this is about topographic beta or planetary coriolis variation. If the latter can we expect a balance? What about curl and Ekman up welling/downwelling?

Reply: It is the former. This will be added to the sentence.

3823 24 add? but the wind driven response in other areas may not necessary be the maximum responses for those regions

Reply: This seems to be misplaced, we could work out what Rev2 is referring to but we will be pleased to follow this suggestion if more information or a reference to another

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location in the paper is given.

3824 29 show >shows

Reply: This will be corrected in the revised document.

3825 5 in G-S&P 2012 page 200 1st para it states the 1996 (Jan) and 1998 (Nov 1997) comes in 2 SLA pulses . Also that the ocean response to a low pressure like your ïňAg 10 takes much longer.

Reply: This seems to be misplaced, we could not work out what Rev2 is referring to but we will be pleased to follow this suggestion if more information or a reference to another location in the paper is given. Anyway, and with regard to the response time of the ocean to the pressure anomaly, the ocean can have several response times depending on the time scales involved. For instance, Esnaola et al (2012) showed that when using daily scale data, and for the Bay of Biscay area, the response time of the SST anomalies to the turbulent heat-flux anomalies is approximately in the 4 day range. These heat-flux anomalies, in turn are related, to SLP anomalies. So the conclusion is that different response times are to be expected depending on the persistence properties of the SLP anomalies.

3825 15 you would need to ïňArm up wind stress curl for this statement. As already mentioned the topography does it, Aviles Canyon etc. This should come before wind stress curl. Also loss off NW Spain, this is called Inertial Overshoot, see 1999 Continental Shelf Research 19 929-975.

Reply: We can not work out what the reviewer is suggesting us to do. Does the reviewer mean the the topography should be mentioned before the wind stress curl?? Or that the effect of the topography should be strengthened compared to the effect of the wind stress curl??

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3825 25 and slope current helped along locally with west wind or easterly stress component. The source then is not from Portugal.

Reply: We could not work out what Rev2 is suggesting us to do, but we will be pleased to apply the necessary changes if more information is given.

3826 13 see comment already given at 3825 5 above Reply: see response to the 3825 5 comment above.

3826 18 1997/1998 found to be Nov in SLA see previous comment and at 3825 5. Reply: see response to the 3825 5 comment above.

3826 22 the averaged IPC/SST shows SOMA (rigs 118 and rig 129) which says it 'starts' in SO (September/ October) and 'ïňĄnishes' in MA (March April) this was done from 1988 to 1991. It's all a question of deïňĄnitions of IPC, what did the early observations show?

Reply: For the comments about SOMA see reply of comment 3813 15.

3827 ENAWp? Sub Polar water is usually considered to be in area A of ïňĄg 1 of Pingree 2002 the otherside of the of the N Atlantic Current, ie the Sub Polar Gyre. You have already given a good name for your region of interest in 'Indroduction', 'Intergyre'? Changes of temperature for the región will follow AMO well? The less saline water in Area III 300-500m comes from the Bay of Biscay itself refs already given. The BoB water property hasn't changed changed signiïňĄcantly since Nov Dec 1967? See ïňĄg 1 of Deep-Sea Research 1969 vol 16 275-295 near sigma 27.1-27.2. This CTD proïňĄle at Sta Cavall (6534) central BoB is the ïňĄrst high resolution CTD (called STD then) proïňĄle in the NE Atlantic with values every m with resolution 0.001C and 0.002 psu.

Reply: We have already discussed in detail in our reply to page/line number 3817/10

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the issues about the location and T/S characteristics of the water we are considering. Therefore, we see no point in repeating again the same replies here. However, we still want to stress that we refer in our study to Atlantic Central Waters (ENACWp), while it seems that the reviewer particularly refers to Subpolar Mode Water of the North Atlantic Ocean (SPMW).

The question about the sensitivity of the region to AMO is already answered in the original version of the paper. We computed the correlations of the monthly values of our index with the AMO index and none was significant (see page 3819 and line 15). Since both reviewers have already identified this part as not clear enough, we will rewrite it in a more succinct and clear way.

The mention by the reviewer that the less saline waters in Area III come from the BoB is mentioned by some authors, whilst others (Llope et al., 2006, see above) prefer to consider the BBCW as part of the ENACW.

It is clear that there exist interannual and interdecadal changes of the characteristics of water masses over the area, but only very strong anomalies plus an increment of the winter mixed layer depth are able to drive those changes to the levels characterized by σ_t 27.1-27.2 (Pérez et al, 1995, 2000). However, they do not significantly affect the daily evolution during winter that is mainly being captured by our analysis. In order to make that clear in the final version of the manuscript, we will add the following paragraph to the text.

During 2005-2007, in the inner parts of the Bay of Biscay (Basque Coast) the values of temperature and salinity for σ_t 27.1 displaced to deeper levels (less saline and colder waters) after the intense winter mixing during 2005, recovering values similar to the ones (during years 1984-1987) that appeared after the intense mixing during winter 1984. Those values are significantly lower than the ones that appeared during 1991-1992, when a strong episode of winter mixing allowed the downward propagation of the positive temperature and salinity anomaly that existed as a consequence of the low

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winter cooling and low precipitations that appeared during previous years (1988-1990).

3827 20 the time series is ïňĄne. The conclusion of this para wrt to salty water (ENAWt cf ENAWp wrt to fresher comment above) is that the water is saltier equatorially (as opposed to poleward) pointing to an origin for a component of the IPC to the south, even south of Area I?

Reply: In this instance, as in the previous case, we prefer not to discuss again this point, since it has already been discussed in detail in the reply to comments by reviewer above (answer to comments about Page 3827, line 10). There are several factors that can modulate or counteract salinification. As mentioned by Pérez et al. (2000): "From 20°W, salinity on the 27.1 kg m^{-3} isopycnal surface increases progressively to the east rather than decreasing from the effects of coastal runoff (Pollard and Pu, 1985; Pérez et al., 1995). The spatial trend is similar in different years despite interannual variability in the T/S characteristics". Pérez et al. (1995), from the database of several oceanographic cruises in the North Atlantic, showed a temporal variation in the T-S relationship and the evolution of salinity on the isopycnal $\sigma_\theta = 27.1$ kg m^{-3} at 10 °W-42 °N.

We, effectively, agree with the reviewer that water can come from southern areas. We do not in fact propose in our paper that the origin of the IPC lies in area I, we just mention that a salinity maximum exists and is measured in Area I, without explicitly or implicitly implying that the salinity maximum is locally formed in Area I. We will rewrite this paragraph in order to make our point even more clear.

3827 24 this needs rewriting, ENAW deïňĄnition line deïňĄnes what?

Reply: The ENAW line is used as a reference for different situations that show (more or less frequently) water masses that are characterized by temperature/salinity values that are over/under statistically representative thresholds (standard or mode waters). The fact that they are statistically frequent is the reason that they have been accepted

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as a reference line. It is not specific to our work and they are very often used with this meaning in oceanography in order to identify water masses. We will rewrite the sentence in order to make this clear.

3827 24 (continued) Give ref showing water actually mixes/ moves along this line. Say instead? That this water is colder than 12C (fig 9) and not in the Area I profile fig 8? Alternatively draw deeper water properties on a sigma theta surface and insert new diagram to show influence.

Reply: We will add the following sentence to the updated version of the paper:

"Straight segments in a T/S diagram correspond to simple mixing processes and transports along neutral or approximately isopycnal levels."

Figure 8 in our paper shows homogeneous depth, temperature and salinity scales, so that shallow layers characterized by higher variability can be effectively compared. We will add this information to the paper to make it easier to understand. In Figure 9 the scale is the same for all plots, and additional data from deeper levels have been included, so as to be able to compare different patterns at the salinity minimum corresponding to ENACW.

3828 2 Fig 10 is good, do you want to introduce doubt, with speculative explanation? Is it hard to understand what you are saying, quantify how much evaporation for a 200m water column. eg Area I, S 35.9psu, Area III, S 35.6psu this requires 2m of rain for this particular journey?

Reply: We agree with the reviewer that we must consider the balance between evaporation and precipitation (and river run-off) and the salinity gradient in the winter mixed layer. If precipitation was the only process active on the area, we would agree with the reviewer, that starting from water with 35.9 psu, arriving to 35.6 psu through a 200 m deep column would require a 0.84% change in the water content (1.7 m over the 200 m deep column). However, It is clear that the difference of 0.3 psu that affects a water column of 200 m depth is important. However, the salinity changes must not be considered as instantaneous and absolute changes but rather these changes are integrated throughout the water column after several cycles of circulation and local modifications. In addition, besides precipitation there are other processes acting over the area: during winter precipitation and river runoff over the Iberian Peninsula cannot be dismissed.

Other authors (Pérez et al., 2000) make a computation about the issue based on data and estimations from Rogers and van Loon (1979) and Reverdin et al. (1994) and also on different hydrological series that explain different salination/desalination cycles . The order of magnitude of their balances and ours are quite similar, particularly if we consider river run-off in the balances and not just precipitation.

FF Pérez, RT Pollard, JF Read, V Valencia, JM Cabanas, AF Ríos (2000) Climatological coupling of the thermohaline decadal changes in Central Water of the Eastern North Atlantic, Scientia Marina 64 (3), 347-353

Rogers, J.C. and H. van Loon (1979) The Seesaw in Winter Temperatures Between Greenland and Northern Europe. Part II: Some Oceanic and Atmospheric Effects in Middle and High Latitudes. Mon. Wea. Rev., 107, 509 519.

Reverdin, G., D. Cayan, H.D. Dooley, D.J. Ellett, S. Levitus, Y. Du Penhoat, and A. Dessier (1994) Surface salinity of the North Atlantic: can we reconstruct its fluctuations over the last one hundred years? Progress in Oceanography, 33, 303-346.

3828 11 500 hPa, the low pressure anomaly is still present at SLP which is where the wind stress is?

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

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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 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 patterns.

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

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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 to Figure 10 a panel showing MSLP composites (not just MSLP anomalies) and the text 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.

3828 19 complex may be but still a low pressure anomaly Index.

Reply: Of course, but not necessarily one of the usual ones like the NAO, EA,... or others. So if no preferred relation is observed with any of those, which objective way

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may one have to deduce such index? That is the reason that we performed the analysis with the full sea level pressure, het fluxes, wind stresses and so on fields. We find it is a much more powerful approach to the problem that identifying some correlations with indices.

3828 but actually related to north component of wind stress to south off Portugal 1993 Deep-Sea Research II vol 40 369-388.

Reply: We could not work out what Rev2 is referring to but we will be pleased to follow this suggestion if more information is given.

3828 29 advection of what?

Reply: That is advection of humidity and heat. See comments about the surface heat-flux anomalies before (3821 and 3825 5). It will be added to the sentence.

3829 top sentence give a reference for such a statement. Or do you just mean that a cyclone has lower SST than an anticyclone near the centre?

Reply: See response to the comment before (3821, 3825 5 and 3828 29). References will also be given here once again (Cayan, 1992a; Cayan, 1992b; Zorita et al, 1992; Esnaola eta al, 2012).

3829 4 this is actually a tripole NS for the Ocean with warmer subpolar and subtropical regions? As is the Ocean NAO SST anomaly?

Reply: It does have similarities with the NAO SST tripolar structure, but also discrepancies. See for instance Figures 1, 3 and 4 in Visbeck et al (2003) where the ocean response to the NAO is analysed in detail. The SST anomaly areas in our paper are not located in the same way as in the NAO tripole. I addition, the SST anomalies in our Figure 11 are stronger that those corresponding to the NAO tripole, which is not strange as we are using daily data instead of monthly means. An analogous conclu-

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sion can also be obtained if the 500 hPa level anomaly pattern in Figure 10 is taken into account. Figure 2 in Czaja et al (2003) shows the Maximum Covariance Analysis (MCA, Wilks, 2006) of the NAO SST tripole and the atmospheric circulation anomalies on the 500 hPa level, for the 500 hPa level temperature anomalies in that case. For comparing purposes we repeated that analysis for the 500 hPa level temperatures and geopotential height anomalies (see Figure caption fro details). The results of that analysis (see Czaja et al (2003) or Esnaola et al. (2012) for the details about the methodology) given in Response Figure 4 show a pattern very similar to that of the 500 hPa temperature anomaly shown in Figure 2 of Czaja et al (2003). Comparison of the 500 hPa level geopotential height anomaly pattern with anomalies in Figure 10 of the manuscript also points that we are not tackling with the NAO SST tripolar structure.

The tripolar SST structure related to the NAO has its origin on the surface heat-fluxes and the transports related to the wind stress (Visbeck et al, 2003; Czaja et al, 2003; Deser et al, 2010) to cite some), which in turn are related to the NAO SLP anomaly. Provided that in our case we also have a SLP anomaly with its related to wind stress and surface heat-flux anomalies (Figure 10, surface level), it is not strange that such a SST like the one shown in Figure 11 is observed. We judge that this point deserves some attention in future studies, but that making an analogy between the SST patterns shown in Figure 11 and the NAO SST tripole is speculative based on the results that are given in the manuscript.

Czaja, A., A. W. Robertson, and T. Huck, The role of Atlantic Ocean-atmosphere coupling in affecting North Atlantic oscillation variability, in The North Atlantic Oscillation: Climatic Significance and Environmental Impact, Geophys. Monogr. Ser., vol. 134, pp. 147–172, AGU, 2003.

Deser, C., M. A. Alexander, S.-P. Xie, and A. S. Phillips, Sea Surface Temperature Variability: Patterns and Mechanisms, Annual Review of Marine Science, 2, 115-143, doi:10.1146/annurevmarine-120408-151453, 2010.

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Visbeck, M., E. P. Chassignet, R. G. Curry, T. L. Delworth, R. R. Dickson, and G. Krahmann, The ocean's response to North Atlantic Oscillation variability, in The North Atlantic Oscillation: Climatic Significance and Environmental Impact, Geophys. Monogr. Ser., vol. 134, pp. 113–145, AGU, 2003.

3829 6 steric sea level, point to it in the ïňAgures, I don't see it just SLA elevations along the Ocean Margin or Eastern Boundary Flow.

Reply: those are not given in the the Figures, but in the text (see 3823 2 in the manuscript). The beginning of the sentence will be modified adding "As previously indicated, these...".

3829 10 how deep do you think the wind stress (Ekman) goes to feel the slope topography? Give values and units and associated errors for wind stress curl.

Reply: Our intention with Section 3.3 was to give a sound view of the mechanisms involved in the variability of the IPC-Navidad (see title of the Section 3.3), so we think this level of detail is not necessary here. See for instance Le Cann and Serpette (2009) for a more detail analysis of this topic. With regard to the wind stress curl, its unit have already been added to the text following a previous comments by the reviewer. In the case of the error associated to it two answers may apply. First if the reviewer is pointing to the errors related to the wind stress variable used to deduce it, then the errors are those associated with the ERAInterim reanalysis in relation to this variable. We are using that reanalysis, which is widely accepted as a very good quality product and routinely used in climatological studies, with an end user perspective so that level of accuracy is not necessary once again. Second, if the reviewer is pointing to composite pattern of the wind stress curl given in Figure 10, the statistical significance test applied in that case is (an in all other variables of the composite analysis) can be found in the manuscript. Then, in reference to the result for the wind stress curl composite, it does not make sense to doubt about the validity of that statistical analysis for a given variable

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without doing the same for the rest of the variables from our point of view.

3829 13 unison? Remove this statement. The slope current is usually present there but also seasonal see many refs but in particular 1999 Continental Shelf Research 19 929-975 with 500 day records from 3 different current meters off Goban Spur (promontory) showing seasonal repeat, and overshoot at rig 154. Clearly favourable wind stress with winds from say the SW are going to enhance the shallower slope current in the winter where the slope takes that orientation, eg Ireland to Shetland. Just as a west wind will enhance ïňĆow along the north coast of Spain and a SW wind will help both Portugal and Spain (as in your ïňĄg 10). These wind effects can all be done with a slightly different low pressure anomaly, a cyclone as shown in G-S&P 2012 ïňĄg 11 and strikingly in ïňĄg 12 with satellite images. Dec 1989 is just right for a bit of increased ïňĆow off Goban spur and in the Bay of Biscay, see ïňĄg 13 PathïňĄnder SST anomalies. Of course the depression wont stay still so effects have to be integrated. That's why the ocean needs a few months of winter NAO negative to get a signiïňĄcant subsequent response. Force magnitude and full response are generally not in phase. These authors (Garcia –Soto et al 2002 and Garcia-Soto and Pingree (2012) did not suggest unison but demonstrated using monthly thermal satellite observations that the poleward current can extend from Portugal to Scotland in particular years (see their ïňĄgs for Jan 1990 and 1998) that patches of poleward SST can occur extensively with variable meteorolical conditions with a large extent of development. Make this quite clear here and again in 'conclusions points 7 and 10'.

Reply: We will replace the incorrect statements pointing to a unison development, in both the discussion and the conclusion sections, saying that if persistent enough atmospheric forcing conditions take place, then the poleward current can have a large extension.

3830 1st sentence I don't think this makes sense rewrite or remove and sentence before . A force, wind stress say, gets a reponse, how long it takes depends on many factors even whether we are talking about SST (eg upwelling off Africa) or currents (the development of the Subtropical Gyre) , size of region considered etc.

Reply: That sentence will be removed in the revised document, and also the next one for consistency.

3830 8 or just the geosophic adjustment without any heat ïňĆux

Reply: Both this sentence and the next one will be removed.

3830 20 give the grid scale for the SLA data and the separation of ascending and descending tracks somewhere off Portugal. Also state that the anomalies in the ocean result from eddies and could be removed with 5 deg averaging.

Reply: We have used gridded products from AVISO, and not satellite track products (see section describing the data used). Hence, the suggestion about the satellite tracks does not apply. In reference to the comment about the eddies "(i.e. are related to eddies)" will be added to the sentence after "are physically meaningful ". Finally, with regard to the 5 deg averaging see response for the comments about Figure 10 later.

3831 5 The G-S& P 2012 response largely results from the NAO change between 1995 and 1996,or atmospheric pressure change, a low pressure anomaly develops. The Winter NAO change is shown in JMBAUK 2005 85 1301-1315, the largest positive to negative Winter Index change on record? Anyway ïňĄgs 3 and 5 of that paper show the NA circulation (between the Sub polar and Subtropical) depicted as NAC Index lags the the driving wind stress by about half a year (look near 96, 1996).

Reply: This paragraph compares the results given in the manuscript with those found in Garcia-Soto and Pingree (2012). Although the ocean response time scales involved in both works (shorter time scale here and longer in the work by those authors) are not the same, similar SST and SLA patterns are obtained in both cases, which motivates the comparison. This fact is already pointed in the paragraph, where its is explicitly stated that the results do not mutually exclude each other. See also comments about different response time scales in the reply to the comment 3825 5 before.

CONCLUSIONS 3831-3832 very breïïňĆy here or in DISCUSSION 1.What is

Reply: This was already stated in the original document (p3812 l8). Anyway, it will be also added to the conclusions section.

2. To what depth can topographic beta be expected to work. Is this realistic (slope goes to >2000m and with stratiïňĄcation)?

Reply: As previously stated, our intention with Section 3.3 was to give a sound view of the mechanisms involved in the variability of the IPC-Navidad. The composite analysis based on the extreme values of our IPC-Navidad time series captures some of the mechanisms previously proposed to have an influence on the variability of the IPC and IPC-Navidad. One of such mechanisms was the topographic beta effect, for whom we found the conditions similar to those already described by Le Cann and Serpette (2009). Our analysis, in that sense, did not pretend to deepen in that mechanism more than simply confirming what was found by those authors, but with an alternative methodology, and also in a wider context (together with other mechanisms). If the reviewer has concerns about the validity of this mechanism, we will be pleased to deepen in this topic based on any concrete suggestion that could help us to do so.

3. Give the heating required to expand the surface (slope/shelf region) by say 4 cm. Is this realistic?

Reply: The sentence pointing to the surface-heat fluxes as a potential source of the SLA anomalies will be removed from the abstract and the discussion so this is not necessary anymore.

4. Give the fresh water transfer for considering salinity decrease from Area south to

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Area III. Does this just not stongly indicate vertical tranfer is not signiïňĄcant overall wrt to advection of properties?

Reply: In the final version of the manuscript we will make clear our position, as we have already discussed (reply to point 3828 2 above), including references and estimations of the involvements of other processes such as river runoff involved.

5.Give a Slope Transport estimate in Sv (need only ïňĄg 11 and 8)

Reply: This transport could be computed based on the altimetry data, assuming a geostrophic flow and a weak vertical shear for the IPC, using the $g/f * d(SSh)/dy$ expression (Gouruff et al, 2011). However as we are using gridded altimetry products, and not satellite track data, we have serious concerns about doing such a thing. In addition, the paper already gives a good set of results and we do not think this is critical for the quality of the manuscript. Anyway, if the editor or the reviewer think this should be in the revised manuscript, we would add it.

Gourcuff, Claire, Pascale Lherminier, Herlé Mercier, Pierre Yves Le Traon, 2011: Altimetry Combined with Hydrography for Ocean Transport Estimation. J. Atmos. Oceanic Technol., 28, 1324–1337. doi: http://dx.doi.org/10.1175/2011JTECHO818.1.

6. Give a reference for a buoy track coming on slope from say more than 14W, at 41N.

Reply: Comments about the possible source of the flow on area V have will be removed, including the area V itself in Figure 1 and in the results section, so this is not necessary anymore. See Section 1.4 in General Comments of this document.

7. Say the winter –NAO and the EA+ do show a low pressure anomaly in the N Atlantic (it's in the deïňĄnition for the Jan NAO dipole, which is north south,) and that a low pressure centred somewhere between Iceland and the Azores will tend to have SW winds off Spain (and S winds off UK , see G-S&P 2012) as it passes? If the anomaly is centred further south we could even get a north stress off Iberia (from a south wind).

Would this pattern be better for making the IPC? It is interesting that a south wind still produces a shelf ïňĆow along the N Spanish Coast (Pingree and Le Cann 1993). That is because of Ekman transport and that ïňĆow can be caused by wind set up in other regions, a low in Area III in this case. Finally if we move the low even further south we get SE winds (north stress) off Iberia with a high SLP to the north. It may not happen very often and it is hardly surprising that NAO –ve cannot work too well as high pressure to the north becomes NAO + correlating with Navidad/IPC. But such events do happen and occurs in the 1988-1991 mooring data shown in JMBAUK 74 107-128. It also happens (S-SE winds) in the SST P80 time series?

Reply: From our point of view this is not necessary. Those teleconnection patterns should be used as long as they are useful. The atmospheric variability is a lot more that simply NAO, EA, or any other teleconnection pattern, so pretending to simplify all atmospheric circulation anomalies to such patterns is an oversimplification, and even erroneous. The beginning of section 3.3 in the previous version of the document showed no preferential relation with any of those teleconnection patterns of the northern hemisphere. That is why we avoided the limitation of the analysis to one (two,...) of those patterns, and used a wider point of view. Those results (now removed from the text and given in section 1.3 of this document) are statistically significant, and the methodology is strong. Accordingly, we do not see any justification for adding more comments about the NAO and EA. We included that analysis (now removed) in order to compare the relations found by other authors and the ones for our time series. Se also Section 1.3 in this document.

8. Is there a positive SLP anomaly centre (ïňĄg 10) north of 65N, between here and the N Pole as in NAO? Or is the positive anomaly to the south as in EA? Or do you have both or none?

Reply: See previous comment.

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9. Is you SLA pattern much the same as NAO but displaced / intensiïňĄed to the east and therefore more effective in the Bay of Biscay.

Reply: See previous two comments. Anyway, if our SLP anomaly is displaced with respect to the NAO (CPC index for instance), we do not see the point in trying to relate it to the NAO based on the rotated PCA methodology that is used to deduce the pattern (NAO in this case).

10. If this analysis was done elswhere would we just get a concentration of isobars (with favourable direction) near the region of interest? Have we discovered wind stress is important in our region of interest or generally locally if considering a ïňĄxed point? The more the forcing is local the shorter time for adjustment. The adjustment time scale for currents on the shelf/shelf break is only a few days.

Reply: As the composite analysis shown in Section 3.3 is based on the PC of the SST anomalies over the area in Figure 6, the variability patterns deduced are consequently conditioned by the variability in that area. Accordingly, yes, the results are conditioned by the area that is selected for the analysis, and other results can be expected if the area selected for the analysis is changed. However, we do not think this is a problem as our interest was to identify the atmospheric forcing that will influence the variability of the IPC-Navidad. Anyway, a comment will be added pointing that the results can be affected by the fact that the IPC-Navidad series is deduced from a limited area.

Figures Fig. 1 caption, Word, 200m depth also shown, depths should not be labelled with minus sign

Reply: The caption will be modified to include a reference to the 200 m depth contour.

Fig, 2 non-mising

Reply: This will be corrected in the revised document.

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Fig. 3 like >as

Reply: This will be corrected in the revised document.

Fig. 4 1990 and 1996 do not show warm water coming on slope from region V. Images for January 1990 show poleward ïňĆow from much further south, south of Cape St Vincent even, so from the subtropical front región where there is a maximum dynamic height gradient (2x10-7 rad, see for example ïňĄg 2 of JMBAUK 1999 79 769-792). 1987 and 2005 show temperatura gradients in region V but no slope current? Comment? In 1990 and 1996 it's hot at/along the French coast, near Cap Breton, this is reconstructed, should we believe these coastal anomalies?

Reply: With regard to the area V comments, see Section 1.4 in General Comments of this document. In the case of the positive anomalies along the French coast (right column), the reviewer might be right, but we do not have a way to clearly know that. However, it should be taken into account that (Figure 2) the error estimations over that area are among the highest.

Fig. 5 caption says January cf XII in ïňĄg. Could one say the anomaly is leaving the slope near region V rather than sourcing the Navidad?

Reply: The caption will be corrected as it is "December" and not "January".

Fig. 6 Longitudes 3 8 13 W incorrect; also Latitude 43N. Does not this ïňĄgure suggest we might expect contributions to the slope current from regions south of V (centred 41N)? Of course we don't expect the poleward current to be only on the slopes futher south, shown by buoy tracks , eg ïňĄg 3 and ïňĄg 24 of JMBAUK 1997 77 573-624. The arrow from the slopes near Lisbon (ïňĄg 24) is based on 711 days at 200m on 2000m depth contour and slope ïňĆow from the south and meddies (can leave the slopes near Lisnon and Setubal Canyons 'restrictions' (see buoy and sub surface slope tracks in JMBAUK 75 235-252).

Reply: Longitudes will be corrected in Figure 6. With regard to the source area, area V and all related comments will be removed from the document. Also see comments in Section 1.4 in General Comments of this document.

Fig. 7 caption remove ‘analysis’? Year is better than Years for abscissa? Very ïňĄne ïňĄgure. What about the grey bars in 2002 (Nov Dec 2001)? Red dots don’t always appear to give the mean, 1989Feb? Say dashed positive and negative lines are . . . in caption. Certainly in good agreement with Garcia-Soto.

Reply: “analysis” will be removed from the Figure caption, and the meaning of the horizontal dashed lines will be added to the caption. The deduction of the monthly means (grey bars) has been checked and the results are correct. The good correspondence with the results of Garcia-Soto is underlined in several points of the document.

Fig. 8 Easier if temp scale at top and salinity scale below, since paper is more about SST. Should say what numbers are in () in ïňĄg caption and change salt to sal. Give error bars on proïňĄles. Give stability proïňĄles BVF (cycles per h, OK) for each area. BVF min (a tracer) is around 200-300m in Bay of Biscay (1cph) and BVF appears modiïňĄed by slope current even at depth of min in these ïňĄgures.

Reply: Temperature scale will be shown at the top and the salinity below as suggested by Rev2. The meaning of the number in () will also be added to the Figure caption. As we think that the paper is already very large and has a lot of results, we prefer no to add the BVF profiles to the Figure and the document (See Response Figure 5 in this document). However, they have been computed and they are given as part as this response document. If Rev2 or the editor think they might be of interest, they could be easily introduced as they are already deduced. However, this would be done at the cost of making the paper even larger, and be think on them as results for future research and contributions on this topic.

Fig.9 like in >as, proïňĄles >station data (as just distribution of water properties). The

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ENAWp ENAWt line does not represent ïňĆow or mixing. ENAWp is not Subpolar Water in Area III. Water with these properties (11C, 35.6psu sigma 27.1-27.2) is formed just to the north in the Bay of Biscay in winter near the slopes at 48N. Deep mixing 400 m here forms the BVF min. A deep drogued Argos buoy placed in this min moved south across the Bay of Biscay (buoy 3907, it took 305 days to get to between Area II and III (Deep-Sea Research II 1993 vol 40 369-388, ïňĄg 8). Winter mixing is shown in ïňĄg 5 of Deep-Sea Research 1989 vol 36 735-758. This 11C Water (see Area III) is not subpolar water just Biscay Water (could even be called Biscay Mode Water, see also Fig 1 of JMBAUK 1999 79 769-792). The BVF min at sigma 27.2, 300m depth is shown in ïňĄg 4. The ENAWt is not very helpful; at the limit shown here (this ïňĄgure) 36.3psu 16C this is the Subtropical Front or near surface Azores Current and in the east this water can move poleward towards Cape St Vincent with SST winter (Feb/March) temperatures in the range 14-17C. Finding similar properties in Area V does not show only source unless you show an Area VI to the south (eg as seen in 1990 January images, ïňĄgure 5 shown in Pingree and Le Cann, JMBAUK 1990) that does not have these properties. It would be expected that an Area VI would be somewhat warmer as SST falls along the slope current from Cape St Vincent to Cape Breton. SST to south (Area VI) would be warmer than March 14-17C, range given above in January. This can be ïňĄrmed up with buoy / sub surface ïňĆoat tracks. Give ïňĄrm reference for buoy/ïňĆoat coming in from 41N 14 W (Area V) to slope at 41N 9-10W. What is that H and dot on the subtropical subpolar line?

Reply: We have already discussed in detail this issue in our reply to reviewer's comment (Page 3817, line 10) above. We have provided references.

Additionally, we have already mentioned (same response above) that several authors consider the winter waters of the Bay of Biscay part of ENACW. Additional studies that propose this point of view are:

Treguer, P., P. Le Corre and J.R. Grall, 1979. The seasonal variations of nutrientes in the upper waters of the Bay of Biscay region and their relation to phytoplankton growth.

Deep-Sea Research, 26A: 1121-1152.

Fraga, F., C. Mouriño and M. Manríquez, 1982. Las masas de agua en la costa de Galicia: Junio-Octubre. Resultados de las Expediciones Científicas del B/O Cornide de Saavedra. Suplemento de Investigación Pesquera, 10: 51-77.

They consider this kind of water a modification of ENACW and relate its formation with over cooling and cascading over platform and slopes over the Celtic area during winter homogenization. See, for instance:

Cooper, L.H.N. and D. Vaux, 1949. Cascading over the continental slope of water from the Celtic Sea. Journal of the Marine Biological Association of the United Kingdom, 28: 719-750.

Lines in a TS diagram represent the most frequent occurrences of some water masses and, as such, they represent ideal conditions that can sometimes be slightly different in real cases. We have considered TS profiles from different areas and period of times representing a wide range of upper water masses in a pluriannual context. We have reference them against idealized water modes. Obviously, the TS profiles show variability and modifications with respect to idealized water masses. In fact, interannual variability exists.

With regard to the comments on area V and VI, as already stated the area V will be removed from the paper. See comments in Section 1.4 in General Comments of this document. The inclusion of the area VI could be an interesting research topic for future studies coming after this one. Finally, the meaning of the dot and H will be added to to the caption of Figure 9.

Fig.10 caption should have (a) b c d. Anyway top Z (a), more explanation must be given, give units for the numbers in this ïňĄg. Give -50 meaning.Say what -10m storm tracks mean. Next Surface (b), not possible to see what pressure has caused what wind/stress in white regions, change isobars to black here. Similar pressure distribu-

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tions are given in Garcia-Soto and Pingree 2012 (Figure 11). The low centre is near the atmospheric cyclone centres shown in this same paper (Figure 12). Not clear why wind stress arrows missing in some regions, give also geostrophic wind based on isobars, this is linear and will do qualitatively where wind stress is missing. These will show SW winds off NW Spain as in Garcia-Soto and Pingree? Say why wind stress arrows are missing in the Bay of Biscay (lag 15-30). Wind STRESS anomaly is NE as in Garcia-Soto and Pingree (G-S&P). Of course you cannot compare the position of the lows in G-S&P with that here directly as that would be like comparing anomaly with the actual situation but the interpretation is much the same, namely a north component of stress (a SW wind gives a north component of wind stress) from the Subtropical Front/ Azores Current region to Ireland is the driver, with local variations depending on the slope orientation. Pingree 1993 Deep-Sea Research vol 40 369 -388 (Figure 11) shows the wind stress at 40N 10W and concludes seasonality of the slope current with max tendency change in December is responsible for the timing of the poleward ïňĆow on the Portuguese slopes (IPC) and Navidad follows in the Bay of Biscay, enhanced with the east component of wind stress (SW wind) here, see Pingree and Le Cann 1989 wind response. SLA (c) spell out SLA in caption. All the SLA values in the white region /ocean could be derived with say a running mean of 5deg (averaging as in JMBAUK 2012, 92 213-234). Say in text the OCEAN structure SST and SLA is a tripole for low atmospheric pressure anomaly. SLA v (d) SLA averaging of gridded data should be done to remove white bits? How convincing is this wind stress curl, AGAIN (see a) must give units for these number. Slope current does not appear to come from Area V, comment? Figure should be extended south to show where water south of 40N is coming from, an Area VI? In Pingree and Le Cann 1989, the modelled (from 40 N to 64 N, but only shown from 45N, their Fig 14) slope current did not signiïňĄcantly come on shelf in the Celtic Sea and English Channel, comment? . . . but there is shelf ïňĆow with south and SW winds (see responses and (b) above), so wind and slope current not truly resolved which is why models are so important. These ïňĄgures (a) (b) (c) (d) are important so it is worth taking a bit more trouble with them, making them larger at

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least. Suggest 2 ïňĄgures.

Reply: As both reviewers suggested a, b, c and d indicators will added to the Figure in the revised document. With regard to the Z composites, the meaning of both the storm-track location and the geopotential height anomalies is already given in the manuscript and an effort will be done to explain them more in detail. The units of those composite fields are all m. This unit is given by the color scale in the Figure, but it will be made explicit that it also applies for the Z composites, and not only to the storm-track location. As pointed by the reviewer, some areas appear masked in the figures, or have missing wind-stress arrows or contour. This is not an error and was made on purpose. The reason for that is already explained in the text, and also by the end of the Figure caption, but an effort will be made to make it as clear as possible. The reason is that the difference between the positive and negative composites is not statistically significant in that location for the variable being plotted. This means that for that variable and location there is no statistically significant difference between extreme IPC-Navidad and non-IPC-Navidad occurrences, and hence, those values are not interesting for the process being analysed. This includes the SLA anomalies, and accordingly, the 5 deg averaging proposed by the reviewer is not necessary as those white pixels are showing us results (lack of statistical significance of the difference of means), not a missing value.

The similarities of the SLP anomalies in Figure 10, and those in Garcia-Soto and Pingree (2012), will be noted in the discussion. However, as well noted by the reviewer, this has to be done with caution as here we are showing composites of the SLP anomalies and not SLP fields. In addition, the references pointed by the reviewer in relation to the wind stress will also be taken into account when discussing the wind stress patterns shown in Figure 10.

The SLA acronym will be added to the Figure caption. With regard to the wind stress curl units and the validity of the this mechanism see earlier comments in the reply to the question number 2 made by the reviewer. For the comments related to the area

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V see Section 1.4 in General Comments of this document. Finally, in reference to the suggestion of splitting Figure 10 in new Figures, we would not have any problem to do so, although we like it in the original form.

Fig. 11 Similar to? Say in text the anomalies in other regions may not be representative of the most signiïňĄcan/sensitivet anomalies (seasonal/interannual) found (know to occur) in other regions since the analysis here was for IPC region. How does -15-0 compare with in principle (large scale) with temperature anomaly SST tripole resulting from Sea Level Pressure (low pressure anomaly, NAO -ve) described and shown in G-S& P (ïňĄgure 7A)?

Reply: The caption will be rewritten explaining what the Figure shows but without making any reference to previous Figure 10. For the dependency of the analysis on the area used see response to the question number 10, and for the relation with the NAO SST tripole see reply to previous 3829 4 comment.

TABLE 2 needs a few entries/corrections At line 5 page 3839 after Iberian Coasts insert 'or NW Spain'. Line 6, ZG not in Table. In the Table include this information. GS& P (2012) analyzed the poleward current off northern Spain from 1979 to 2010 (ieTime-evolution) using ERSST v3b. SST anomaly data (their ïňĄgure 8). Also AVHRR PathïňĄnder data (monthly) presenting the important Jan 1990 SST anomalies from Portugal to Norway (ïňĄgure 13), see comment at 3829 13. At Garcia-Soto, Pingree & Valdés (2002) indicate January monthly (their ïňĄg 3B, 1967-2000). Also Garcia-Soto (2004) 18/12/2002 Altimetry. Pingree and Le Cann 1989 Prog Oceanogr vol 23 303-338, Figure 17 shows Navidad around NW Spain and to Cap Breton Dec 83 and Jan 89. Pingree 1994 JMBAUK 74 107 -128, in situ measurements (hourly data, daily running mean) from 1988 to 1991 on moorings 118 and 129 on 1000m contour. Also time series of Navidad SST (January) from 1967 to 1993 together with mooring data (ringed), showing Navidad can penetrate to 210m depth with poleward ïňĆow (Rig 118) reaching 30cm/s in Dec 1988 and Jan 1989, broadly comparable with your

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ïňĄgure 10 values. The mooring results described 'Navidad' evolution and events and showed the seasonal maximum temperature lags the alongslope current by almost a month (the temperature will persist as the current decays). These measurements were for the 1989 Navidad (see your ïňĄgure 7 and Area II). Again your result ïňĄg 7 works ïňĄne with a month of 1989 daily data (January) from the NW Spanish slope. When was the term IPC ïňĄrst used? Give date in text (in Introduction). The Jan SST series also shows the cooling trend in 1970s and increasing temperatures from mid 1970s to 1993 as seen in AMO and Bay of Biscay SST (G-S&P 2012) so longterm changes in winter mixing temp are know, put ref and conïňĄrm or contrast near Perez and Pollard ref 3827 7.

Reply: All missing data related to already mentioned references in Table 2 will be added. This will be the case, as well, for the references pointed by the reviewer and that were not taken into account before. Those references will also be taken into account in the comparison of our IPC-Navidad time series that will be completely rewritten following an earlier suggestion by the reviewer.

Finally, with regard to the question about the first use of the IPC term, we think this was by Peliz et al (2003), and if this is correct we will add a reference to the introduction pointing that this was the first use of the term. We would also like to say a final word on this in addition to the comments given in Section 1.1. As we referred there, most of the manuscript deals with a warm SST anomaly. We are open minded to any suggestion by the reviewers in order to name such SST anomaly. We have made some choices in order to name it but we would not any problem to change our choices and use another nomenclature if the reviewer thinks we should and if the proposed name is of wide use inside the community.

Péliz, A., J. Dubert, D. B. Haidvogel, and B. Le Cann, Generation and unstable evolution of a density-driven Eastern Poleward Current: The Iberian Poleward Current, Journal of Geophysical Research-Oceans, 108(C8), 2003.

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