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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.

Anonymous Referee #1

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This manuscript exploits the long-term remote SST observation series from AVHRR Pathfinder data to reveal the variability of the Iberian Poleward Current (IPC) in the Bay of Biscay (BoB) and, with the help of reanalyses of atmospheric fields, to propose a suite of mechanisms explaining this variability. After a description of the various data sets used in the study, the authors briefly describe the methodology employed to extend the coverage of the winter SST maps, called DINEOF (Beckers and Rixen, 2003). The resulting dataset is evaluated in comparison with the non-extended dataset, and with independent in situ data. The satisfying comparison allows the authors to use the extended dataset for investigating the IPC signature in SST. They find that the first EOF mode of the decomposition of the winter SST anomalies with respect to a 15-day

running climatology, over the shelf and shelf break along the Spanish and Portuguese coasts, represents the signature of the IPC. The authors thus consider the Principal Component associated with this mode as a proxy for the IPC index. The presence or absence of developed IPC based on this index show good agreement with estimates from the literature. In situ data selected from events of marked presence and absence of IPC are used to investigate the respective signature in temperature and salinity of these events, which also compare well with previous studies. The authors then notice correlation (lagged or not) of their IPC index with various atmospheric indices. Finally, they use the same episodes of intense and absent IPC as previously, to derive anomaly indices of various atmospheric and oceanic fields. These anomalies show that intense IPC events are associated with changes in the 500hPa level geopotential height and associate surface fields, corresponding to a weakened anticyclonic gyre of the Northern Atlantic circulation, enhanced southwesterly winds over the Bay of Biscay, and enhanced heat gain over the European shelf. The authors suggest that both anomalies in wind stress and heat flux may explain the positive sea level anomalies over the European Shelf, which is associated with more intense IPC through geostrophy. The authors also found that intense IPC episodes are associated with a meridional sea level gradient west of the Iberian Peninsula, which is known to be the main driver of the IPC through JEBAR effect, and with enhanced local positive wind stress curl patterns along the Iberian Peninsula, which are known to also drive the IPC through topographic beta-effect.

This manuscript provides a convincing tool for identifying IPC/Navidad (in the BoB) events. Its major quality, in my view, is that it is based on an objective index, derived by the authors based on exhaustive data set, and not on subjective appreciation of individual SST maps or isolated data, as is the case in most of the IPC/Navidad studies. In addition, the evaluation of the IPC index provides a useful synthesis, using previous studies, about the signature of the IPC/Navidad current, both from remote sensing and in situ data. These two aspects support the publication of the present manuscript. However, the interpretation of the atmospheric indices for explaining the intensification

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of the IPC is not very precise and requires some clarification. This is why I recommend publication after minor revisions. Detailed comments follow.

Detailed comments

Abstract

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

Results

Sea surface temperature reconstructions

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

Sea surface temperature variability and the surface signal of the IPC

I have a few comments on this part. First, it appears that the reconstruction strategy leads to some smoothing of the data, which is to be expected, since EOF decomposition retains in priority large-scale patterns. In particular, I don't really agree when the authors say that the reconstructed SST fields of January 14, 2006 (Fig. 5) shows meanders as nice as the map from Le Cann and Serpette (2009, their Fig. 1b): the original map showed actual meanders with sharp fronts, whereas the reconstructed map only shows warmer waters extending off the shelf, without as sharp fronts. The

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smoothing is apparent also in Fig. 4, for the 1996 case: the reference data show a clear signature of the IPC as having warmer waters on the shelf break along the western and northern coast of the Iberian Peninsula. In the reconstructed dataset, this shelf break signature has clearly decreased on the west coast, and vanished along the north coast, leaving only warm waters on the shelf compared to the deep ocean. Then, the SST anomaly approach also tends to affect the identification of the IPC, since the climatology used for the anomaly estimation includes part of the current signature, as noted by the authors. Based on Fig. 1, this signature includes a warm anomaly on the shelf break, especially along the north coast. Thus, in addition to the smoothing, the anomaly calculation also removes the shelf break signature of the IPC, which leads to favor, by contrast, its shelf-wide signature. This is clearly seen in the 1996 case of Fig. 4, where the SST anomaly is the most intense on the shelf and along the coast. As a result, the EOF decomposition of the SST anomaly naturally favors the shelf-wide signature of the IPC, especially when the spatial extent is limited to the shelf and shelf slope. I think these aspects should be acknowledged, because the IPC is described in the literature as a slope current, and the authors describe it as such in their introduction. Thus, its main signature is expected to be along the shelf break, although the intense IPC episodes are also associated with warming of the whole shelf. Still, the approach used by the authors is perfectly valid and well justified, but the fact that the IPC shelf-wide signature is favored might explain some of the differences noted with some previous studies.

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

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would help clarity.

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

Insights on the Iberian Poleward Current variability mechanisms

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

At the end of this section, I was confused by the use of the Absolute Dynamic Topography (ADT) value from AVISO to calculate the meridional steric sea level gradient, which drives the JEBAR effect that is thought to be the main driver of the IPC (Huthnance, 1984, Frouin et al., 1990). The ADT is the sum of the Mean Dynamic Topography (MDT) and the Sea Level Anomaly (SLA). The MDT is the temporal mean of the ADT, which is thus constant. So, the time tendency of the ADT gradient is only due to the SLA part of the signal. But, since the authors calculate the CP composite alone, and not the CP-N, they need the full ADT to access the sea level gradient with the correct

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sign and amplitude. This choice of composite leads to another question: why did the authors calculate the CP composite for the sea level gradient, and not the CP-N as for other variables? If the CP-N composite shows that the sea level gradient is somewhat the same during intense episodes or absence of the IPC, it means that the JEBAR effect is not responsible for the intensification of the IPC. This would be a major result.

Discussion

This part is interesting, but could be clarified, mostly in relation with the analysis of the atmospheric and oceanic field anomalies.

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

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

Finally, I found the part discussing the causes of the SLA anomalies over the Eastern North Atlantic not very conclusive. The authors suggest that both the heat-flux and the

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wind-stress anomalies play a role. The similitude in the signature of the geostrophic current anomalies over the shelf around the Bay of Biscay with Pingree and Le Cann (1989), noted by the authors, support the role of the wind-stress anomalies. Maybe the authors could use the data they already have (ERA reanalysis and AVISO) to evaluate the seasonal SLA and surface heat flux over the continental shelf: the seasonal SLA cycle should be dominated by the steric effect, associated with seasonal changes in surface heat flux. Then, the authors should be able to estimate whether the heat flux anomaly contributes significantly to the SLA anomaly related to the IPC variability.

Conclusions

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

Suggestions

- p. 3796, l. 14: "doesn't have", instead of "has not".

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

- p. 3801, l. 7: "The variety of the datasets used. . ."

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

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

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

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authors could simply mention “various instruments.”

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

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

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

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

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

- p. 3809, l. 6: “discrepancies between of satellite and in situ. . .”

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

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

- p. 3810, l. 19: Western Iberian Buoyant Plume

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

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

- p. 3812, l. 13: “to the North in along the French coast. . .”

- p. 3812, l. 20: the authors assume that the 1st EOF is related to a well developed

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IPC, not “at least partially”. This is the main work hypothesis in the manuscript, it has to stand strong, and the authors have arguments to support this.

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

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

- p. 3814, l. 4: I disagree with the authors’ statement that January 1997 SST map from Le Hénaff et al. (2011) doesn’t show IPC-like signal. Fig. 5 from Le Hénaff et al. (2011) for that year shows a tongue of warmer water along the shelf break, very similar to Fig. 3a from deCastro et al. (2011) in 1989, which is considered a year with marked IPC. This specific disagreement with the authors’ results might be due to their methodology, which favors shelf-wide signature of the IPC.

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

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

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

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

- p. 3815, l. 20: I suggest to use “most developed”, instead of “best developed”.

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

- p. 3816, l. 14: "for area I" instead of "for the I area". In general, the authors should use "area I", "area II" etc. without "the" in front.

- p. 3816, l. 29: ", and" instead of "but".

- p. 3817, l. 7: in general, the authors should use "shelf" rather than "platform".

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

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

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

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

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

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

- p. 3821, l. 21: probably 35°N, instead of 25°N.

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

- p. 3822, l. 5: SLA instead of SLP.

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

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

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

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until 75-90 days “like in the case of previous variables”.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Interactive comment on Ocean Sci. Discuss., 9, 3795, 2012.