

2nd revision of the Manuscript by Arjona-Camas et al. by Esther Portela

I appreciated the detailed response of the authors to my comments. I particularly liked the inclusion of a broader context by using the reanalysis data. However, I think the last figure should be better discussed, while other parts of the paper seem much less relevant to me and could be streamlined.

I find the paper very long with a too detailed results description and with part of the discussion that really doesn't belong there. Discussion seems to be much longer than in the previous version and there are entire parts that don't seem relevant to me (as I stated in my comments below), but I could be missing something important, and if that's the case, please let me know.

My main criticism is still the same, that despite the nice data compilation, I still think this study doesn't add much and has little implications regarding what is already known. That said, I am a big fan of exploiting all available data to address different scientific questions before new data collection with the associated carbon print. I also don't think research has to be always innovative, but interesting and well conducted, which is largely the case here. Because of that, and since I'm not an expert of this region, I prefer not to make a strong judgment on the novelty of this study, and will just trust the other reviewer's and editor opinion about this point.

Please find my comments below

L20-23 : I still find this sentence a bit ambiguous regarding the magnitude and extent of the DSW cascading and export. Mainly it is the term "export" what bothers me. What does "export" refer to? Can we use this term when we find the given water mass to be transported 100 km away? 10 km away ? 1 km away?

L47: This is what I understand by "export"

Methods.

I appreciate the authors gave us more information about the interpolation (or gridding) method. But I still think more details are needed, mostly about the spacing of the grid or the length scales used (if so)

L.234. SPM has not been defined at this point

Results. Section 4.1.

I find a bit hard to interpret this section if I still don't know what, how and when, is related to the dense water export.

L361-363. Actually the strongest ocean heat loss starts around November 1st and goes until approx the end of February.

L334-335. Why should discharge be related to the wind?

L336-340. And related to the Hs, isn't it?

Main comment:

L397-398. I feel like this sentence should be the beginning of the story. The authors chose to provide the context before showing the presence of Dense Shelf Water, which is the object of the study. As I said in my previous comment, all that previous information is kind of empty if

we don't know where is the DSW observation and how it looks like. It is a matter of style maybe, but I'd find it much clearer the other way around.

Fig 4e. Why is the y-axis scale so large? You could reduce them a lot so the variability would be much more visible. Currents seem to be nearly zero at the CCC during the cruise time, which is actually the focus of this study. I also wonder what does the alternating positive negative current pattern means. Do you have an explanation for the up-canyon and down-canyon currents to be so regular?

Fig 5. Please make the axes labels larger.

L406. I can't see the high dissolved oxygen as compared with the upper layer.

L461-463. Try to avoid subjective language, mostly when numbers are provided, and let the reader decide. 150 W m⁻² is 25% less than 200 W m⁻², and is half of 500 W m⁻².

Discussion: I find the discussion to be way too long. While the writing is very clear and the connection with the bibliography is excellent, the new discussion is substantially longer than in the previous version, with many additions for which I do not really see the relevance (unless it's in response of the reviewer comments, but even then..). There are also lots of repetitions of the results numbers and references to figures. I would recommend to summarize and streamline, I've made more concrete suggestions about this in the following comments.

L495-496. But as important as talking about current speed is current direction.

L519-535. I cannot see the point of these lines. This part seems disconnected from the rest of the study, it does not really provide any useful information, and is also misplaced in the discussion section.

L551-169. I find this paragraph too long. Mostly because it only addresses a small part of the results, the SPM concentrations in the plume, which is also not so surprising.

L582-583. This is an example of results repetition than can be avoided.

L591- From here, I'd say this belong to results and not to the discussion section. Also, the comparison would be more clearer with a map (even in the S.I), as with the data provided in the table we can't really see how well the spatial patterns are represented. For instance, the reanalysis provides 12.42°C for T1-03, and T104, while their observed mean temperature differed by 0.4°C.

L605-606 I guess this means that the transport has been averaged for the dense waters density layers. But then, There are many instances with up-canyon transport, which is quite weird, isn't it? Or is there an explanation for this?

Figure 10 is nice, but I miss more discussion around it. Also can you explain how did you differentiated between mild and intense events? Is this a wind, or heat loss threshold as you mentioned in the introduction? It is surprising that transport is often as intense in mild events as in intense events.