

Response to Editor

Dear Editor,

We appreciate the constructive feedback provided by the reviewers.

Reviewer #1 raised concerns regarding the novelty of our study, particularly the potential overlap with Martín et al. (2013), as well as the overall balance of the manuscript sections. To address these points, we have revised the text to streamline the Results and Discussion sections, avoiding repetitions, and reducing descriptive details. The new Discussion follows a more logical order, better highlights the results of the paper, and places them in context by comparing the winter of 2021-2022 with previous mild and intense cascading events.

To clarify the novelty of our manuscript, and remove any ambiguity about the novelty of our work, we have prepared a comparative table (also included in our response letter to Reviewer #1) that clearly demonstrates the substantial advances and unique contributions of our study beyond Martín et al. (2013).

Comparative table: our study vs. Martín et al. (2013)

Aspect	Martín et al. (2013)	This study
Process investigated	Downwelling of coastal waters	Mild/shallow DSWC event
Period studied	March 2011	Winter-spring 2021-2022 (with detailed observations in March 2022)
Instrumentation	Moorings + CTD transect at the southern canyon flank	Multi-platform approach: moorings + hydrographic transects (glider + ship-based CTD) at the shelf and canyon + ADCP + reanalysis product. Original observations and new data collected during the FARDWO-CCC1 Cruise (2022).
Main forcings	Storm-induced downwelling	Moderate net heat losses during winter. Eastern storms further enhanced DSWC
Water mass characteristics	Pot. temp.~ 11.5-12.5 °C Salinity ~ 37.7 Pot. density ~ 28.78 kg·m ⁻³	Pot. temp.~ 12.2-12.7 °C Salinity ~ 38.1-38.2 Pot. density ~ 28.9-29.1 kg·m ⁻³
Dense water volume	No significant dense shelf water formation. Downwelling to the canyon head ~0.2 Sv and 10 ⁵ t of SPM	Dense shelf water formed in the Gulf of Lion during winter 2021-2022. 0.7 Sv and 10 ⁵ t SPM cont. shelf 0.3 Sv and 10 ⁵ t SPM upper canyon 0.05 Sv and 10 ⁴ t SPM mid canyon
Detachment depths	200-300 m depth	150-400 m depth
Main contribution of the paper	Documentation of a storm-induced downwelling event, with the absence of DSWC under limited external forcing during a mild winter. No comparison with previous events is provided.	Detailed observational characterization of a mild/shallow DSWC event, as well as the shelf-slope dense-water and sediment transport during a mild winter. Comparison with previous mild and extreme events (broader spectrum of cascading events in the region).

We believe that all these elements demonstrate that our manuscript presents original observations of a shallow/mild cascading event, extending previous findings in the canyon under similar meteorological regimes, and contributing to a better understanding of the interannual variability of both mild and intense DSWC events in the Cap de Creus Canyon. We have made the pertinent changes throughout the manuscript to highlight these differences. We hope these revisions have addressed her concerns about length, focus, and balance.

Reviewer #2 mainly suggested minor clarifications, which we have incorporated to the manuscript. Also, she suggested to include a comparison between this study with previous mild and intense cascading events. Accordingly, we have expanded the Discussion section to include it. Our comparison considers the atmospheric forcings and dense water transport values, which allows us to put our study within the broader spectrum of cascading intensities in the Cap de Creus Canyon.

Response to reviewer 1 (2nd revision)

Dear reviewer,

We thank you very much for your relevant comments to our manuscript. Below, your reviews are reproduced in **black** font and our responses in **blue**. Since the other reviewer has raised important points, we kindly suggest to review her responses. Please, note that all line numbers in our responses refer to the **clean version** of the manuscript, not the tracked-changes version.

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.

Reply: We thank you for your constructive comments. We have carefully considered your suggestions and made substantial revisions to improve the clarity, focus, and balance of the paper.

Last figure: We have added more discussion around Figure 10 (lines 524-536), comparing the winter 2021-2022 with previous mild and strong winters in terms of transports and associated forcing conditions.

Streamlining the Results and Discussion sections: We have carefully revised the Discussion section to avoid repetitions with the Results, removed information that was less connected or added limited discussion to our paper, and incorporated additional discussion. We believe that this final version of the Discussion (from line 504) follows a more logical order, better highlights the results of the paper, and places them in context by comparing them (with a bit more detail) with previous mild and intense cascading events.

Novelty and contribution of our study: To remove any ambiguity regarding the novelty of our work, we have prepared a comparative table that clearly demonstrates the substantial advances and unique contributions of our study beyond Martín et al. (2013).

Comparative table: our study vs. Martín et al. (2013)

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We believe that all these elements demonstrate that our manuscript presents original observations of a shallow/mild cascading event, extending previous findings in the canyon under similar meteorological regimes, and contributing to a better understanding of the interannual variability of both mild and intense DSWC events in the Cap de Creus Canyon. We have made the pertinent changes throughout the manuscript to highlight these differences. We hope these revisions have addressed her concerns about length, focus, and balance. We hope these revisions have addressed your concerns about length, focus, and balance.

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?

Reply: We thank you for pointing this out. We agree that “export” can imply that dense shelf waters reach the open sea or deeper parts of the basin, which is not the case for the event studied here. To avoid ambiguity, we have replaced “export” with “transport” in the manuscript. Here, “transport” refers to the movement of dense shelf waters from the adjacent continental shelf into and along the canyon, reaching at least transect T2, approximately 30 km from the shelf. We have included this sentence into the abstract to clarify: “Dense shelf waters were transported ~30 km from the continental shelf into the canyon” (line 21).

L47: This is what I understand by “export”.

Reply: Please, see previous comment.

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).

Reply: As stated before, the hydrographic profiles were interpolated using the isopycnic method integrated in the Data-Interpolating Variational Analysis (DIVA) software included in Ocean Data View (ODV) (v. 5.7.2). The interpolation parameters were set with scale lengths of 200 m horizontally and 1 m

vertically, a quality limit of 3.0, and excluding outliers. However, it is important to note that the gridding primarily follows the isopycnals. Therefore, although the scale lengths influence the smoothing, the resulting gridded fields are mainly influenced by the isopycnal structure. The main advantage of this approach is that it improves the representation of water masses and reduces the artificial smoothing that the standard depth-based gridding method in ODV would do.

As suggested, we have better clarified this, as well as the spacing of the grid between lines 179-185.

L234. SPM has not been defined at this point

Reply: Thank you for pointing this out. We have now defined it in the Abstract (line 15) and Introduction (line 113).

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.

Reply: We agree that in the previous version we described the winter 2021-2022 conditions without sufficiently framing them in relation to dense water formation and the presence of dense shelf waters in the canyon. To address this, we have thoroughly revised Section 4.1 (lines 328-363) to link the meteorological and oceanographic forcings with the timing and presence of dense shelf waters. In particular, we provide context for both the period prior to the FARDWO-CCC1 cruise (highlighting the strong heat losses, persistent northerly and northwesterly winds that favored dense water formation in the GoL) and the subsequent period after the cruise with easterly storms, increased Hs, and enhanced coastal river discharges.

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

Reply: We have modified this in lines 333-335, which now reads: "The time series of net heat fluxes over the GoL's shelf showed negative values from October 2021 to early April 2022, indicating a heat loss from the ocean to the atmosphere (Fig. 3a). The strongest net heat losses during that winter occurred between November 2021 and late February 2022, reaching values of about $-400 \text{ W}\cdot\text{m}^{-2}$ (Fig. 3a).

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

Reply: We have shortened this section to avoid excessive descriptive details and removed the previous sentence about the December peak discharge. However, the sustained easterly winds observed during March 11-13 favored the advection of Mediterranean humid air towards southern France, causing intense rainfall from the Eastern Pyrenees to the Massif Central, especially in the Aude and Hérault watersheds. In contrast, precipitation in the rest of France, including the Rhône River watershed, were weak (<https://www.eaufrance.fr/publications/bsh/2022-04>). As a consequence, coastal river discharges increased up to $2265 \text{ m}^3\cdot\text{s}^{-1}$, while the Rhône River discharge remained comparatively lower ($884 \text{ m}^3\cdot\text{s}^{-1}$) (Fig. 3d)". You can find this explanation in lines 351-355.

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

Reply: Indeed, the eastern storm on March 13 was associated with Hs > 3 m for over 20 hours. We have rephrased this paragraph to make this clearer (lines 351-355). According to the existing literature, easterly winds can produce large waves over the continental shelf, and lead to an intense cyclonic circulation on the GoL's shelf and to a strong export of shelf waters at the southwestern exit of the GoL (Ulses et al., 2008a; Mikolajczak et al., 2020).

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.

Reply: We understand your point, and agree that in the previous version the link between dense water formation and the presence of dense shelf waters (DSW) in the canyon was not fully framed. We have chosen to maintain the structure presenting the meteorological, oceanographic, and hydrological context of winter 2021-2022 before showing the DSW observations in the canyon, as this allows to have a clearer understanding of the background conditions. We agree that in the previous version, it was difficult to locate the observations in space and time. With the new revisions on sections 4.1 and 4.2, we believe the context now sufficiently frames our observations.

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?

Reply: Thank you for your comments.

Fig. 4e: We have reduced the y-axis range to $1 \text{ mg}\cdot\text{L}^{-1}$ to better illustrate the temporal variability of SPM concentrations.

Current speeds during the FARDWO-CCC1 cruise: Indeed, current speeds at 1000 m depth in the Cap de Creus Canyon were relatively low during the cruise. This indicates that dense shelf waters did not reach the bottom at the mooring location during the winter 2021-2022. Our observations are consistent with a mild cascading event reaching depths of $\sim 400 \text{ m}$ depth in the canyon.

Alternating positive-negative current patterns in LDC-1000: Previous studies (e.g., Béthoux et al., 2002) have reported near-bottom oscillatory currents of $0.1 \text{ m}\cdot\text{s}^{-1}$ at 1000 m depth in the Lacaze-Duthiers Canyon associated with intense dense shelf water cascading events. However, during our study, the temperature time series in the canyon and the atmospheric and oceanographic conditions during winter 2021-2022 do not show any evidence of deep cascading, which is usually associated with these oscillations at these depths. Likely, these oscillations could be related to the interaction of the meandering Northern Current (offshore displacements) with canyon topography, as previously interpreted by Durrieu de Madron et al. (1999) for the Grand-Rhône Canyon.

Fig 5. Please make the axes labels larger.

Reply: Done. We have done it for figures 5, 6, and 7.

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

Reply: We have corrected the text to indicate that dissolved oxygen values in this layer were around $200 \mu\text{mol}\cdot\text{kg}^{-1}$ (line 417).

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} .

Reply: Thank you for pointing this out. We have rephrased the sentence to express the differences quantitatively (lines 508-510).

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.

Reply: We appreciate your comment and agree with your suggestions. Some of the additions in the previous version were introduced in response to earlier comments from the reviewers. However, after rewriting the Discussion, we realized that some parts did not connect well with the text and added little value. Therefore, we have summarized and streamlined the Discussion to make the message clearer and specially to avoid repetitions with the Results section. At the same time, we have reorganized it to improve its logical progression and included some more discussion around certain topics (as for Fig. 10) to highlight the results. Please, find the modified Discussion from line 504.

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

Reply: We have removed the detailed description of the mooring data to avoid repetitions with the Results section. Also, we agree that both current speed and direction are important. The mooring data indicated that there was no significant down-canyon flow at the monitored depths by the moorings, confirming the absence of a deep cascading during the studied winter.

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.

Reply: We understand your concerns and agree that the paragraph describing the hydrodynamics of the plume using the Richardson (Ri) and Froude (Fr) numbers may seem disconnected from the main focus of the manuscript. This section was originally included in response to a previous suggestion from the other reviewer to examine the hydrodynamics of the dense water plume. However, after a careful consideration, we agree that the detailed discussion of Ri and Fr does not directly contribute to the discussion of the MDSWC event. Therefore, we have removed this paragraph from the revised manuscript. Nonetheless, we acknowledge that this information is of high interest for a separate study specifically focused on the physical dynamics of dense shelf water cascading.

L551-569. 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.

Reply: Thank you for your comment. However, we believe that this section actually provides valuable information, since one of the main goals of the paper is to investigate SPM transports associated with cascading. We acknowledge that in the previous version these results were not sufficiently discussed, but we have now placed them in a new section (5.3) and expanded the discussion. We now emphasize how the observed SPM values in March 2022 could be comparable to those expected for the entire cascading season identified in the reanalysis, although we acknowledge that they are estimates.

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

Reply: We agree with your comment. To address this, we first have rephrased the sentence as: “The transports of dense shelf waters and associated SPM in the Cap de Creus Canyon during the observed MDSWC event in March 2022 were 0.7 Sv and 10^5 t across the continental shelf, 0.3 Sv and 10^5 t in the upper-canyon, and 0.05 Sv and 10^4 t in the mid-canyon”. We have also moved this to a new section in Results, entitled “4.4. Duration and magnitude of cascading events during winter 2021-2022” (lines 463-503).

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.

Reply: We agree with your comment and have incorporated this information to new section 4.4 (lines 463-503). Additionally, to clarify the spatial comparison between observations and reanalysis, we have included a new figure in the Supplementary information (new Fig. A1) illustrating the spatial distribution of stations. As you pointed out, there are some apparent differences in temperature values, which mainly arise from the different spatial resolution of the MedSea reanalysis (~4.5 km). The T1 and T2 observational transects did not follow a perfectly straight line, and stations were spaced on average 1.5 km. In contrast,

reanalysis stations are located in idealized straight transects, with each point corresponding to the nearest model grid cell. As a result, two nearby reanalysis stations may share the same temperature value because they fall within a single grid cell, whereas nearby observational stations can display different temperatures. Inevitably, reanalysis slightly smooths small-scale variability, especially in narrow submarine canyons, but the overall agreement between the two datasets is still high, with RMSE < 0.2 °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?

Reply: Thank you for your comment. Transport values were calculated by integrating the along-canyon velocity over the dense water density layer. Occasional up-canyon transports during the cascading season likely reflect short-term current reversals along the canyon axis. However, when we integrate transport over time, the net flux is down canyon during the cascading season, consistent with the direction of dense water overflows.

After the cascading season, the transport shifted predominantly up-canyon (data not shown), which reflects the residual flux along the canyon axis. This residual flux has been previously documented within the Palamós Canyon, where a persistent up-canyon flow is superimposed on the periodic (i.e., inertial) along-canyon oscillations (Martín et al., 2006, 2007; Arjona-Camas et al., 2021). Please, see lines 489-495.

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.

Reply: In figure 10 we aimed to provide a long-term context of the interannual variability of dense shelf water cascading events through the Cap de Creus Canyon. As explained in the Methodology (Section 3.5), we distinguished between MDSWC and IDSWC events based on density thresholds: IDSWC were defined by densities > 29.1 kg·m⁻³ and MDSWC events with densities below this threshold. Regarding the forcing mechanisms, the distinction of MDSC and IDSWC events is not solely based on wind or heat loss thresholds, but a combination of them. Nevertheless, the exact triggering mechanisms and magnitudes driving DSWC need further analysis (Fos et al., 2025).

Indeed, it is surprising that the magnitude of transport during mild winters is sometimes comparable to that of intense winters. As previously acknowledged in the literature (Mikolajczak et al., 2020) and discussed in this paper, the major difference between mild and intense cascading events may concern more the preferential transport pathways than the volume exported. In winter 2010-2011, only 30% of the transport occurred through the canyon, while 70% followed along the coast or remained around the upper canyon. In winter 2004-2005, 69% of dense shelf water cascaded through the Cap de Creus Canyon down to the deeper parts of the canyon (Ulses et al., 2008a; Mikolajczak et al., 2020).

References:

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Mikolajczak, G., Estournel, C., Ulses, C., Marsaleix, P., Bourrin, F., Martín, J., Pairaud, I., Puig, P., Leredde, Y., Many, G., Seyfried, L., and Durrieu de Madron, X.: Impact of storms on residence times and export of coastal waters during a mild autumn/winter period in the Gulf of Lion. *Cont. Shelf Res.* 207, 104192, <https://doi.org/10.1016/j.csr.2020.104192>, 2020.

Ulses, C., Estournel, C., Bonnin, J., Durrieu de Madron, X., and Marsaleix, P. : Impact of storms and dense water cascading on shelf-slope exchanges in the Gulf of Lion (NW Mediterranean). *J. Geophys. Res. Oceans*, 113, <http://doi.org/10.1029/2006JC003795>, 2008a.

Response to reviewer 2 (2nd revision)

Dear reviewer,

We thank you very much for your constructive and relevant comments to our manuscript. Below, your reviews are reproduced in **black** font and our responses in **blue**. Since the other reviewer has raised important points, we kindly suggest to review her responses. Please, note that all line numbers in our responses refer to the **clean version** of the manuscript, not the tracked-changes version.

Review Comments:

The authors have substantially improved the manuscript in response to the previous round of comments. The study presents a well-structured observational analysis of dense shelf water cascading (DSWC) and sediment transport in the Cap de Creus Canyon during a mild winter regime. The multi-platform dataset (moorings, gliders, CTD profiles, and reanalysis products) is robust, and the results provide useful insights into the dynamics, timing, and sediment export processes under mild winter conditions.

Reply: We thank you very much for your encouraging comment.

Novelty Assessment:

The novelty is somewhat limited because DSWC in the Cap de Creus Canyon under mild winter conditions has previously been described by *Martín et al.* (2013) for the 2010–2011 winter, including estimates of dense water transport (~ 0.3 Sv) and sediment load ($\sim 10^5$ t). The present study adds:

- A more recent mild-winter case (2021–2022) with higher-resolution, multiplatform observations.
- Measurements across both the continental shelf and canyon transects.
- Integration of hydrodynamic and sediment transport data with updated reanalysis products.

At present, the novelty is primarily methodological and contextual rather than conceptual. However, it can be strengthened by including an explicit quantitative comparison of transport and sediment load values between:

1. The present mild winter (2021–2022),
2. The previous mild winter (2010–2011; Martin et al., 2013), and
3. Known strong-winter events (e.g., Canals et al., 2006; Puig et al., 2008).

Such a comparison would position the study as the first to place recent mild-winter dynamics into the broader spectrum of DSWC intensities in the Cap de Creus Canyon, increasing its interpretive value and relevance for understanding climate driven variability in cascading processes.

Reply: Thank you for your suggestion. In line with yours and the other reviewer's comment, we first have streamlined the Discussion to avoid repetitions with the Results section, and then expanded the Discussion on certain paragraphs, such as around Fig. 10. We have now included a direct comparison between winter 2021-2022 with both previous mild and strong winters. This comparison considers the atmospheric forcings and transport values, and allows us to put our study within the broader spectrum of cascading intensities in the Cap de Creus Canyon. Please, see the new Discussion from line 505.

Abstract Clarity:

The sentence "...yet its dynamics under mild winter regimes remain poorly characterized" should be qualified to avoid implying a global knowledge gap. Since mild-winter DSWC has been documented elsewhere (e.g., Mahjabin et al., 2019, 2020), and even in the Cap de Creus Canyon (Martin et al., 2013), I recommend revising to this line. For example it can be written as:

"...yet its dynamics under mild winter regimes in the northwestern Mediterranean, particularly in the Cap de Creus Canyon, have been less comprehensively described and compared to strong-winter events."

This way:

- It narrows the scope to **region + site** (avoids implying a global knowledge gap).
- It acknowledges **some existing work** (e.g., Martin et al. 2013) but still justifies the new study.
- It sets up the importance of **comparison with strong winters** early in the paper.

Reply: Thank you for your suggestion. We have revised the sentence to narrow the scope, which now reads: “Although intense DSWC events have received most attention due to their large impacts, mild DSWC (MDSWC) events are the most frequent in the northwestern Mediterranean and are expected to become more common under climate change. However, their dynamics, particularly in the Cap de Creus Canyon, have been less comprehensively described and compared to strong-winter events”.

We have also added a new sentence at the end of the abstract to highlight the variability of DSW transports in the Cap de Creus Canyon, even under mild winters, which reads: “Our study reinforces the idea that dense shelf water transports exhibit marked interannual variability, even under mild winters” (lines 26-27).

Minor Corrections and Consistency Edits:

- **SI unit for metric tonnes** – Use the correct SI symbol: *t* (lowercase). At first occurrence, write as *t* (metric tonnes), and thereafter use *t* alone. Ensure a space between the value and the unit (e.g., “105 t”, not “105t”). Replace non-SI or ambiguous forms such as “metric tons” or “T” where applicable.

Reply: Done. We have changed it throughout the manuscript and ensured that there is a space between the value and the unit as recommended.

- **Hyphenation** – Standardize usage to either *dense shelf water cascading* (no hyphen) or *dense shelf-water cascading* (with hyphen) throughout text and captions.

Reply: We have standardized it throughout the manuscript and captions, using *dense shelf water cascading* (without hyphen).

- **Acronyms** – In Section 3.2.1, correct ECMWF to *European Centre for Medium range Weather Forecasts* (ECMWF).

Reply: Done.

Overall Recommendation:

With these relatively minor edits and an expanded discussion comparing the present results with both previous mild-winter and strong-winter events, the manuscript will be well-prepared for publication in Ocean Science. The observational dataset is valuable, the analyses are sound, and the study adds meaningful insight into DSWC dynamics in a mild winter regime for this specific canyon system.

Reply: We thank you very much for your overall recommendation. We have re-structured and streamlined the Discussion (as suggested by the other reviewer) and expanded our discussion in order to compare our results with both previous mild and strong-winter events. We believe that with these changes, we have strengthened our understanding in shelf-slope exchanges during MDSWC events in the Cap de Creus Canyon, their interannual variability, and their relevance under climate change scenario.