Reviewer comments are in black, answers from the authors are in blue and corrections added to the manuscript are in green

Anonymous reviewer 1

Summary: The manuscript investigates several aspects of heat flux dynamics in the Mediterranean Sea. First the manuscript compares the long-term mean heat flux from two reanalysis products with different spatial resolutions (ERA5 and ECMWF) and the higher resolution reanalysis (ECMWF) is found to provide a heat flux consistent with the 'closure hypothesis'. The authors attributed this difference to spatial resolution. Then the authors look at the PDF of the turbulent heat fluxes and also look at the impact of extreme events on the heat budget. The authors find reasonable PDFs that capture the statistical patterns in the heat fluxes terms and that fall/winter cooling events are criteria to achieving the negative long-term mean of ECMWF that is consistent with the 'closure' hypothesis.

The work is interesting and will likely be of interest to a broad range of scientists. Couple of questions that I think should be address in some fashion.

Authors: we thank the reviewer for the interest in our work, and we will try to answer all the questions posed.

Major comments

Seems like there's an issue with using the closure hypothesis as evidence for which result is 'correct'. Finding data/results that fit the expectation assumes the hypothesis is truth which seems a bit problematic to me. It seems like the authors should be framing the results differently

We base the closure hypothesis on the heat and mass conservation equations for the Mediterranean Sea. These equations are fundamental for all the earth system modelling; they are not a specific dynamical balance. We argue that, since Gibraltar unequivocally brings heat and water in, the surface fluxes should in principle balance this input. How much is the balance it is not known, and we have hypothesized it is perfect; this is clearly an approximation. As in the seminal work of Bryden and Kinder (1991) and the recent work by Cessi et al. (2014) and Jorda' et al. (2017), the balance between volume integrated heat and mass content helps to understand the basin dynamics. Specifically, being heat entering laterally the Mediterranean Sea, then we search for a negative net surface heat flux. How negative we do not know but searching for a negative net heat flux is a conservative assumption aligned with current scientific understanding.

We have now modified the introduction at line 60:

Furthermore, the estimate of the Mediterranean Sea heat budget from ECMWF meteorological analysis data sets has not been done before.

After line 76 we discuss the hypothesis behind the "closure of the heat budget":

We realise that assuming perfect balance between lateral and vertical heat fluxes, even in the Mediterranean Sea, is an approximation. Being heat clearly entering the Mediterranean Sea through Gibraltar, we search for a negative net heat flux, which we call the closure hypothesis.

How negative such net heat flux is, we do not know but searching for a negative value is a conservative assumption aligned with current scientific understanding.

References

- Bryden, H. L., & Kinder, T. H. (1991). Steady two-layer exchange through the Strait of Gibraltar. *Deep Sea Research Part A. Oceanographic Research Papers*, *38*, S445-S463.
- Cessi, P., Pinardi, N. and Lyubartsev, V., 2014. Energetics of semi enclosed basins with two-layer flows at the strait. Journal of physical oceanography, 44(3), pp.967-979, https://doi.org/10.1175/JPO-D-13-0129.1
- Jorda', G., Von Schuckmann, K., Josey, S. A., Caniaux, G., Garc.a-Lafuente, J., Sammartino, S., ... & Mac.as, D. 624 (2017). The Mediterranean Sea heat and mass budgets: Estimates, uncertainties and perspectives Progress in Oceanography, 156, 174-208 https://doi.org/10.1016/j.pocean.2017.07.001

It's not clear that the spatial difference in the reanalysis products is the only potential cause of the difference. Could there be other possible causes? For example, could the cloud physics be the issue. Looking at Figure 1, the long wave and short-wave radiation are the terms that look most different. Since SST is the same in both cases, the differences in long wave radiation could point to the cloud cover parameters, maybe? Is cloud representation done the same way in these two reanalysis products. It is not clear to me the only difference in this reanalysis products is the spatial resolution. Maybe I missed that though.

We agree that spatial resolution may not be the only factor causing the difference. Our method allows to eliminate the SST as a possible cause, as noted by the reviewer. We agree with the reviewer that the distribution of cloud cover is also an important difference (reported in Fig A1 below). In fact, the difference is a complex function of different quality of the atmospheric variables. To be noted is that ERA5 and ECMWF analyses use approximately the same model and data assimilation systems. However, we have added also the consideration of cloud cover among the potential differences.

We would like to point out that we had already a sentence at line 223: “Furthermore, ECMWF and ERA5 different values are connected to different cloud cover.”

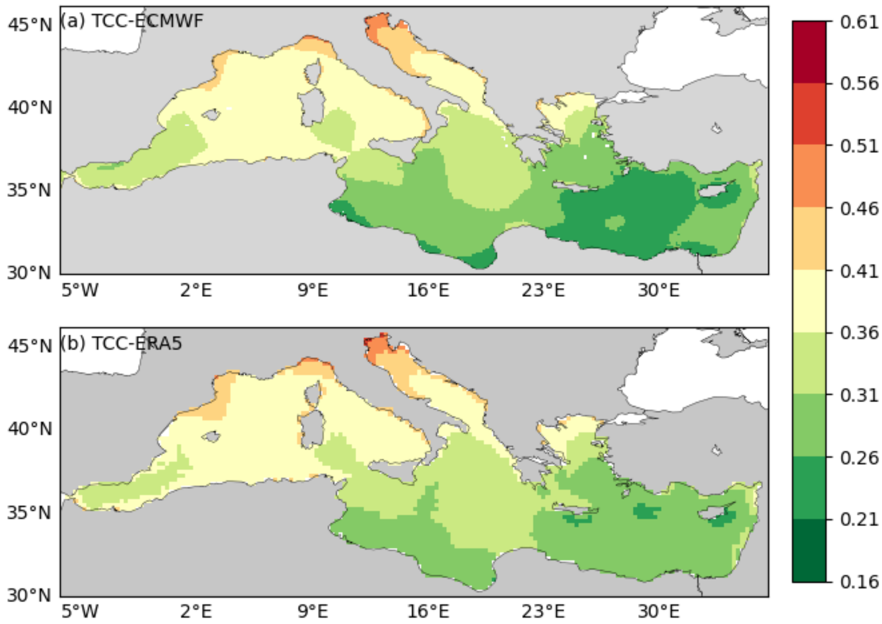


Figure A1: Total cloud coverage (%) mean computed for the period 2006-2020, a) ECMWF and b) ERA-5 input datasets.

To make a more balanced statement we have eliminated in the abstract the sentence at line 35 "This highlights the importance of high-resolution atmospheric data for accurately capturing air-sea interactions and ensuring physically consistent climate modelling over the Mediterranean Sea."

replacing it with:

"Only ECMWF fields are consistent with the heat budget closure hypothesis."

Why are the statistical distributions just the turbulent heat fluxes explored. Its fine, but it seems like the author should comment on why these are the target and why the distributions of the longwave and shortwave radiation are not explored.

We thank the reviewer for pointing out this missing justification. We wanted to compare our turbulent flux distributions with similar work in the literature which uses only turbulent heat flux components like Gulev and Belyaev (2012) and Korolev et al (2015), also listed below.

- Gulev, S. K., & Belyaev, K. (2012). Probability distribution characteristics for surface air–sea turbulent heat fluxes over the global ocean. Journal of Climate, 25(1), 184-206, https://doi.org/10.1175/2011JCLI4211.1

- Korolev, V., Gorshenin, A., Gulev, S., & Belyaev, K. (2015). Statistical modeling of air-sea turbulent heat fluxes by finite mixtures of Gaussian distributions. In International Conference on Information Technologies and Mathematical Modelling (pp. 152-162). Springer, Cham, https://doi.org/10.1007/978-3-319-25861-4_13

Furthermore, if we look at the time series of the fluxes (Fig. A2 below), it is clear that the turbulent components are the one that exhibit larger anomalies with respect to the seasonal cycle, hinting to the presence of skewness and kurtosis in their distribution.

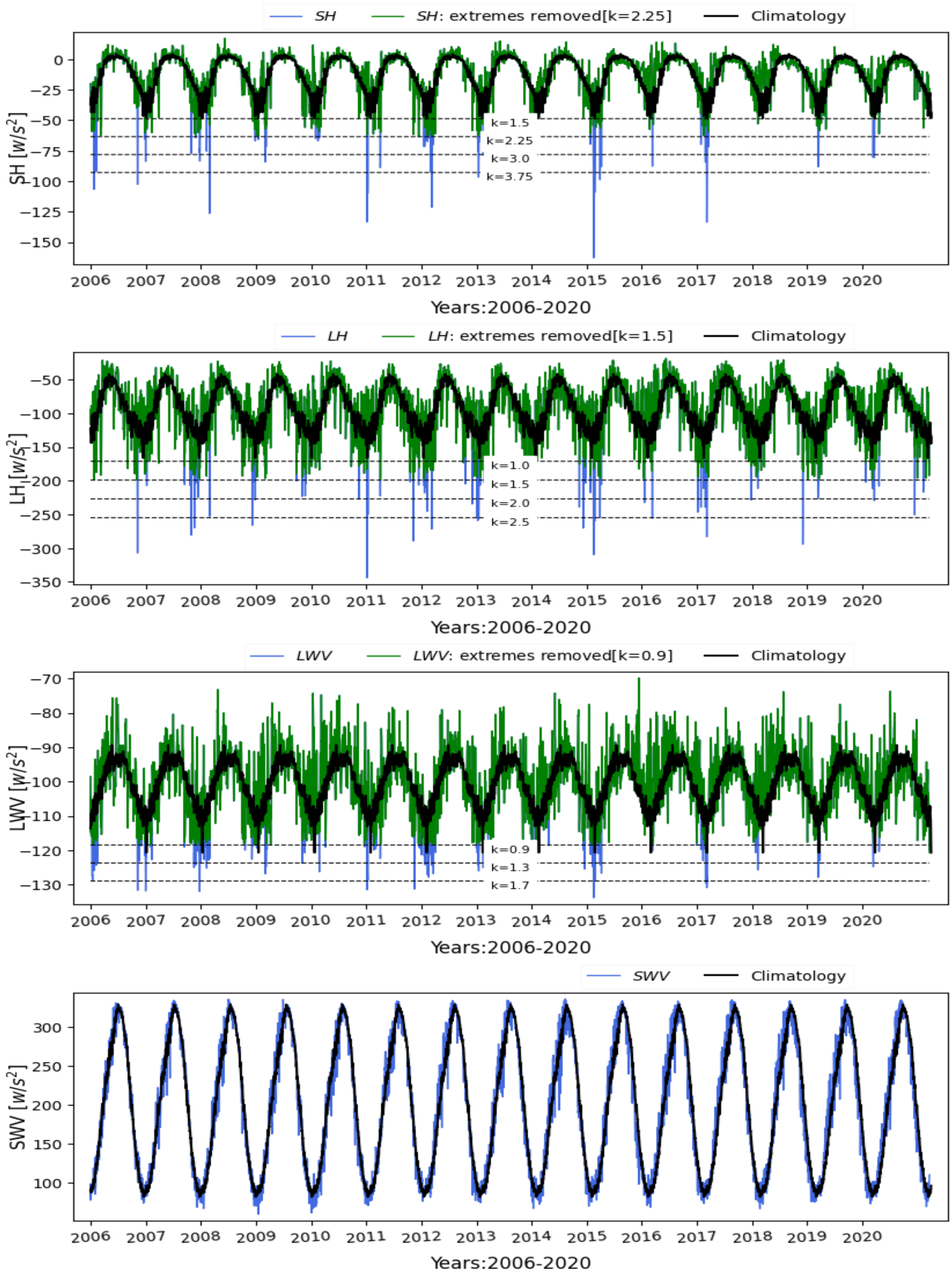


Figure A2: Time series of the basin averaged SH, LH, LWV an SWV and their respective extremes removed, overlapped with long-term yearly climatology. Lower quantile boundary lines are marked with dashed lines with different k values used to remove and replace with climatology values

We have now changed the beginning of the section 4 adding, after line 295, the following statement and Fig. A2 in the supplementary material:

Recent studies by Gulev and Belyaev (2012) and Korolev et al. (2015) have analysed the statistical distributions of turbulent heat fluxes, and their findings are used here for comparison. Radiative flux components are excluded from this analysis, as they do not exhibit extremes of comparable magnitude to those of turbulent fluxes (Supplementary Material, Fig. S3). This suggests low skewness and kurtosis in their distributions, reducing the relevance of a detailed probability density function analysis for these components.

With regard to the extreme events. I guess it should not be surprising that if you remove the most extreme negative values and then average the heat flux the mean will get warmer. However, potential feedback may not be accounted for that should be mentioned. For example, if the extreme heat flux events are removed but there impacts on SST is not, then the subsequent fluxes may be lower than should be if the extreme event in the heat flux never occurred. So, the relative gain associated with the extreme event may not be as significant indicated by just removing the extreme heat flux. There may be other potential feedback that the removal methodology does not full consider that should be mentioned.

Thanks for raising your concern here. We wanted to show the impact of extreme values on the net heat flux time series because it has never been done before, and it highlights where the major uncertainties in the heat budget closure reside. We understand that in a fully coupled atmosphere-ocean system the feedback can be important but our is a diagnostic study and we cannot change arbitrarily the SST. We do not think we need to comment this in the text but if requested, we can add a comment saying that this is not like to do a simulation changing the atmospheric forcing fields extremes which in turn will generate a different SST.

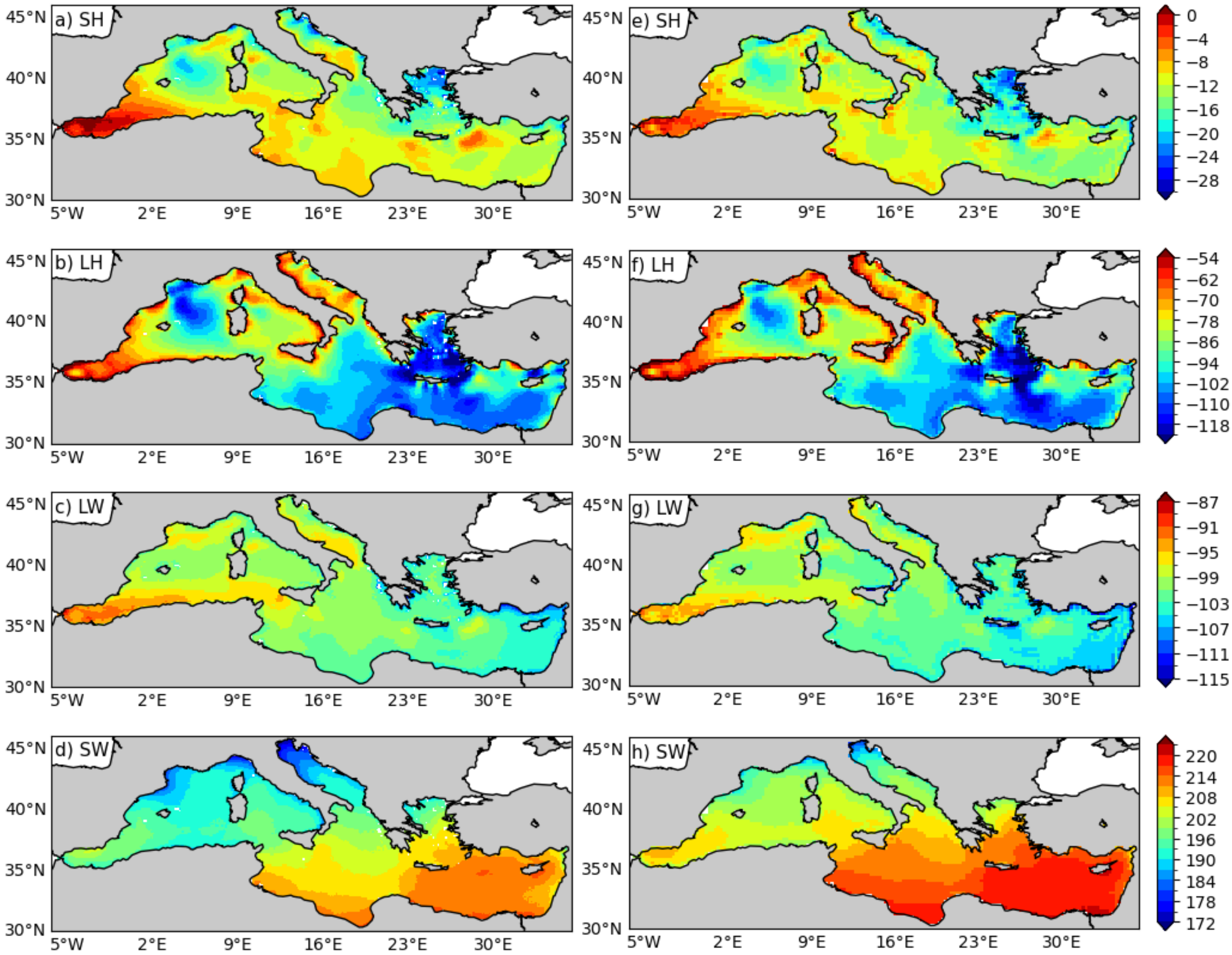
Can more details or discussion be provided on why spatial resolution limits the ability of ERA5 to represent extreme heat loss in fall and winter?

Thanks for the question. A higher spatial resolution is important for capturing many small-scale atmospheric and oceanographic features. In contrast, ERA5 comes with a horizontal resolution of ~31 km, which can smooth many small-scale features and underestimate frequency of extreme fluxes, such extremes are often associated with local wind flows, sharp air-sea temperature contrast and coastal orographic effects.

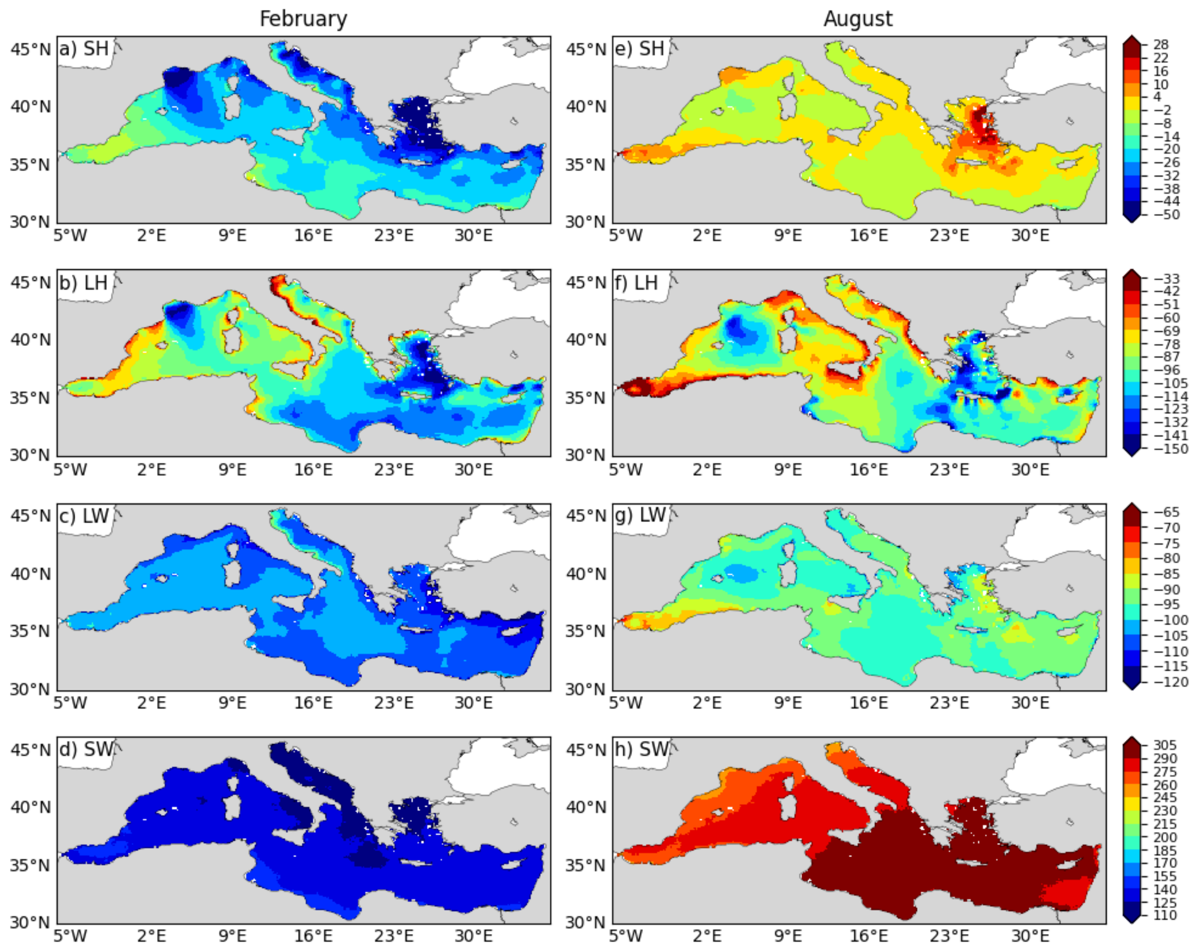
We apologize for the mistake; we displayed a picture of the fluxes (Fig. 1 and 2) with plot smoothing which did not help to see the noisiness of ERA5 with respect to ECMWF. Here we show an updated Figure 1 and 2 where the differences in resolution are evident in addition to other issues.

We have now replaced Fig. 1 and 2 with the new ones, unsmoothed pictures, and commented in the text after line 229:

Fig. 1 shows the noisiness of the fluxes due to the ERA5 low resolution with respect to ECMWF while retaining an overall consistency.



New Figure 1: Mean annual heat flux components for the period of 2006-2020 computed from ECMWF (left) and ERA5 (right) daily time series. The corresponding ECMWF time series is shown in Supplementary material, Fig. S1 and S2.



New Figure 2: Seasonal variations of heat flux components: Left column is the monthly average values for February and right column is the average for August for the period 2006-2020 (ECMWF data).

Similarly related – Figure 7 shows the Qnet, but what is driving the extreme Qnet – typically I assume latent heat flux (and reduced shortwave) is the main driver of fall/winter cooling events, but Figure 1 suggests that these are quite similar between ERA5 and ECMWF at least in terms of the mean… Is that the case in the extreme events?

Thanks for your comment. Yes, we agree that latent heat flux is a major component for the fall/winter extreme events, as shown in Fig. A2 (now Supplementary Material Fig. S3 and commented in Section 4). Additionally, sensible heat flux provides considerable fall/winter extremes.

About the difference between ECMWF and ERA5 extremes we show here Figure A3, which displays the time mean of the Q_{net} extremes considering the values lower than k=0,75 with the IQR method. The Fig. A3 supports the reviewer's observation of a similar pattern for ECMWF and ERA5 extremes. However differences appear in the values, especially near the coasts where ECMWF better resolves the wind jets along the Adriatic, southern Cretan Seas and

Turkish coasts. We did not add this picture to the text since we modified Fig. 8 which gives already information about the extreme locations.

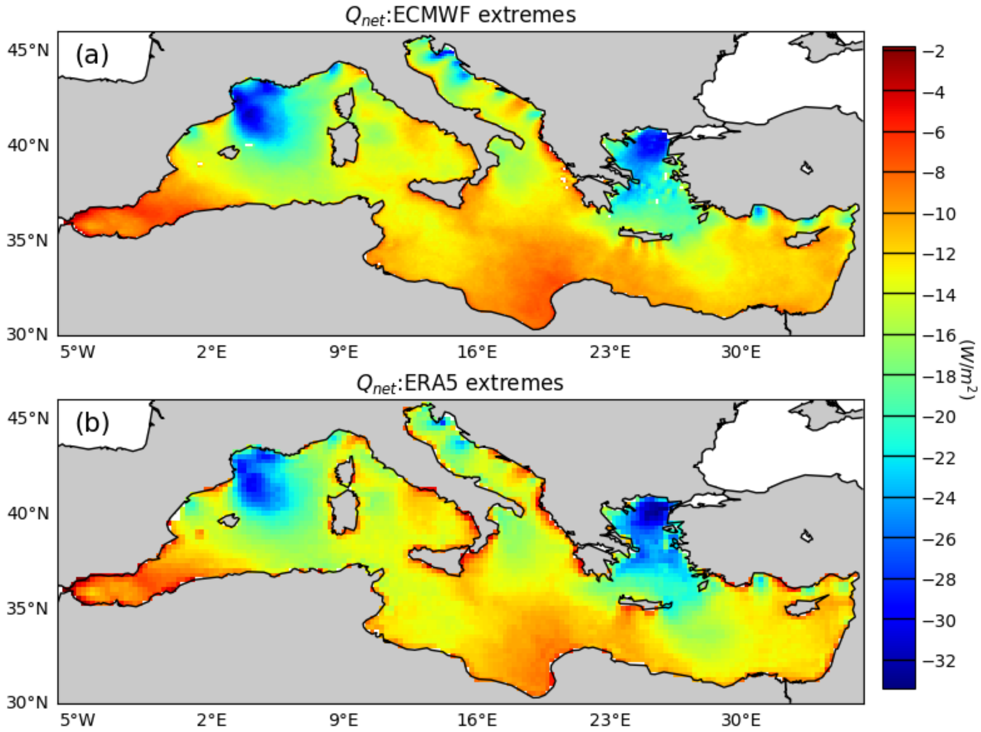


Figure A3: Time mean distributions of the extremes from the Q_{net} time series computed by IQR method (k=0,75). a) Q_{net} extremes: ECMWF b) Q_{net} extremes: ERA5

Minor comments

Line 50-51 Awkward phasing

Thanks, sorry for the unclear phrasing. Now it is:: Moreover, the Mediterranean net heat budget comprises of several terms that show a considerable range of uncertainties (Jorda' et al., 2017).

Line 83 'have shown large deviation' - revise phrasing

Thanks, sorry for the unclear phrasing. Now it is: Using downscaled NCEP/NCAR global reanalysis of ½° x ½° resolution, Ruiz et al. (2008) computed a heat budget of -1 Wm-2. However, their heat flux components values are not close to most of the literature values (for instance, the major difference was in the value for net short wave with 84 $Wm^{-}2$).

Line 162 Was ρ (rho) defined?

Added "ρ =1.22 kg/m3".

Line 400 ' that this is the reason why…'

Corrected

Line 404-405 I don't really understand this sentence.

Thanks for suggesting an explanation, here is the new text:

The Qnet could become an impact indicator of the Mediterranean for sea level trends in the basin. The net heat budget in fact relates to the sea level tendency (Pinardi et al., 2014) in the Mediterranean Sea and could be considered as a key indicator of climate impacts in the Mediterranean Sea.

Pinardi, N., A. Bonaduce, A. Navarra, S. Dobricic, P. Oddo, 2014. The mean sea level equation and its application to the mediterranean sea. J. Climate, 27, 442–447, doi: 10.1175/JCLI-D-13-00139.1

Line 453 'Differences appear…' The main difference were in the long and short wave radiation. Figure 1 showed that latent and sensible heat fluxes were not that different.

Thanks for suggesting the clarification, the text is now: "Differences appear in the structure of the fluxes, especially the SW and LW, when different atmospheric data sets are used,…"

Line 485-487 These sentences are confusing to me. I suggest revising them in some way.

Thanks for pointing out the missing minus sign. The phrase:

"The threshold value that produces a positive basin mean heat loss is 231 W m^{-2}. Thus, if the basin mean heat loss does not exceed this value, the basin is not in steady state. This might be a good indicator of Mediterranean Sea heat content trends to be exploited in the future."

Is substituted by:

"The anomaly threshold value of -231 W m^{-2} (Table 2) results in a long-term positive net heat flux, which is inconsistent with the basin's energy closure assumption, thereby indicating the presence of long-term changes within the basin due to atmospheric forcing"

Line 493 poor phrasing – this sentence should be revised.

Revised "Furthermore, the PDF analysis of turbulent heat fluxes will allow us to have a better understanding of the extreme events and their contributions in the net negative heat budget. "

Reviewer comments are in black, answers from the authors are in blue and corrections added to the manuscript are in green

Anonymous reviewer 2

The paper of Ghani et al., entitled '*Revisited heat budget and probability distributions of turbulent heat fluxes in the Mediterranean Sea*' compared two surface heat budget estimates for the 2006-2020 period over the Mediterranean Sea recomputed by the authors with ECMWF and ERA5 bulk outputs (but the same SST). The paper presents also a statistical analyse of the sensible and latent heat fluxes with a characterization of their distributions and finally investigates the role of heat loss extremes.

This study has questioning conclusive remarks:

The mention that *the heat budget closure hypothesis cannot be satisfied with coarse resolution (lines 457-458)* is not fully exact as shown by Table 1 where previous studies prove their quality to obtain negative heat loss in surface balanced by Gibraltar heat inflow. Possibly you would like to argue that a better representation of the heat budget is related to horizontal resolution; But there are many sources of improvements for representation of the heat budget terms: one is likely resolution, but sea surface and clouds/radiative schemes are also very important. This conclusion must be more carefully discussed in my opinion.

We agree that spatial resolution may not be the only factor causing the difference. Our method allows to eliminate the SST as a possible cause, as noted by the reviewer. We agree with the reviewer that the distribution of cloud cover is also an important difference (reported in Fig A1 of the supplementary material). In fact, the difference is a complex function of different quality of the atmospheric variables. To be noted is that ERA5 and ECMWF analyses use approximately the same model and data assimilation systems. However, we have added also the consideration of cloud cover among the potential differences.

We would like to point out that we had already a sentence at line 223: "Furthermore, ECMWF and ERA5 different values are connected to different cloud cover."

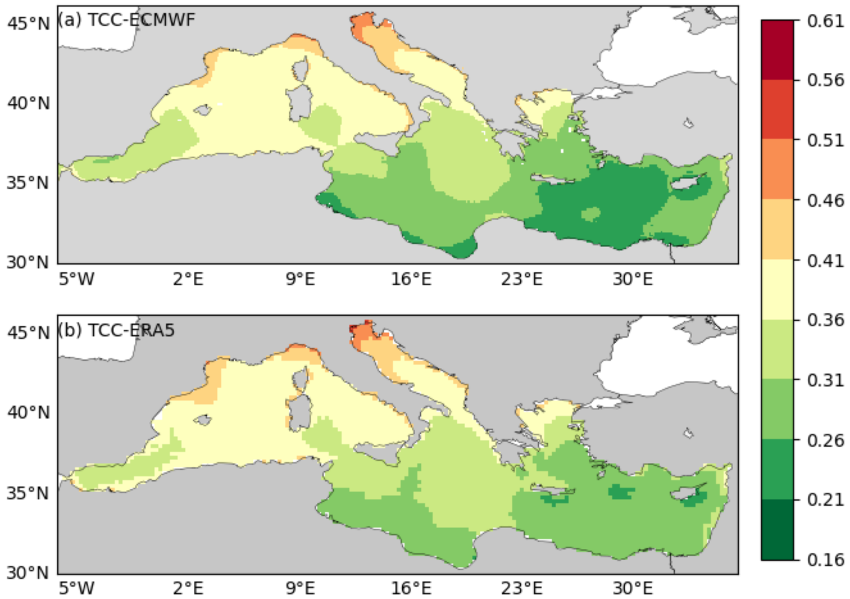


Figure A1: Total cloud coverage (%) mean computed for the period 2006-2020, a) ECMWF and b) ERA5 input datasets.

To make a more balanced statement we have eliminated in the abstract the sentence at line 35 “This highlights the importance of high-resolution atmospheric data for accurately capturing air-sea interactions and ensuring physically consistent climate modelling over the Mediterranean Sea.”

replacing it with:

“Only ECMWF fields are consistent with the heat budget closure hypothesis.”

The statement that *the Mediterranean is still losing heat but only if using a(the) high-resolution ECMWF analysis (lines 459-460)* puzzles me. I am not sure this is a way to promote the results. The fact the Mediterranean Sea losses or not heat is something that cannot be settle by only looking on one dataset. A very large analyse of a large amount of data is mandatory.

Thanks for your concern. Our study is a conceptual study of how two different data sets give different heat balances using the same air-sea flux formulations and SST. The ECMWF analyses and reanalyses are among the most widely used data sets for the Mediterranean Sea and they are special since they assimilate available observations. Yet no specific study is found in the literature. As listed in Table 1, almost all the previous studies have been done with single datasets but not with analyses and reanalyses. We thank the reviewer for forcing us to specify this important novelty of our paper.

We have now modified the introduction at line 60:

Furthermore, the estimate of the Mediterranean Sea heat budget from ECMWF meteorological analysis data sets has not been done before.

After line 76 we discuss the hypothesis behind the "closure of the heat budget":

We realise that assuming perfect balance between lateral and vertical heat fluxes, even in the Mediterranean Sea, is an approximation. Being heat clearly entering the Mediterranean Sea through Gibraltar, we search for a negative net heat flux, which we call the closure hypothesis. How negative such net heat flux is, we do not know but searching for a negative value is a conservative assumption aligned with current scientific understanding.

Please also clarify paragraph p10, lines 273-279. It is confusing to put here the finding is that the Mediterranean Sea still loses heat in surface, as you decided to follow the heat budget closure hypothesis that imposes this.

Thanks for your concern. We do not impose the negative heat budget; we check if the data sets can give a negative net heat flux using the same bulk formulas and SST for ECMWF and ERA5 surface fields.

We have moved all the lines 273-279 to the conclusion section inserting a modified phrase after the line 459, P-18

"Our initial question was: is the Mediterranean Sea in the past 15 years still losing heat at the surface? The answer is yes if we use ECMWF atmospheric analyses. Additionally, comparing the Qnet estimates derived from ERA5 and ECMWF with the same bulk formulas demonstrates that the uncertainty peaks in the atmospheric forcing resolution and possibly cloud cover. This uncertainty impacts the regional heat budget closure hypothesis."

I have also main concerns related to:

the LH distribution (section 4.2). Fig. 5a shows surprisingly a quite large number of positive LH values for all locations. This means condensation, and supersaturation of air mass. This phenomenon is quite rare. It appears mandatory to check the LH values in these distributions. Also, for turbulent fluxes, the computation uses transfer coefficients independent from the wind (equation 9/10). Does this may affect your results in terms of SH/LH distribution shapes.

Thanks for asking this question. We used the anomaly of latent heat (LH) distribution with respect to daily season cycle, where exhibits positive and negative values. We believe, there is no condensation and supersaturation dynamics in the full computed LH time series which remains consistently negative. Following Pettenuzzo et al. (2010), we used constant turbulent exchange coefficients, which are multiplied by stable and unstable condition parameters and updated in final computation using maximum and minimum wind speeds condition.

There are very large differences in SW (Fig. 1d,h). This is the main reason for the ERA5 positive budget (Tab. 1). From equation 2, I understand the differences come only from the cloud coverage C. Did you compare the cloud coverage fields in the two atmospherical dataset? Should the threshold to define clear sky be adapted and?

For these two remarks, a larger discussion of what is mentioned p10, line 263-366 would be greatly useful.

We agree with the reviewer that differences in cloud cover lead to substantial variability in the shortwave (SW) radiation fields (Fig. 1d, h). To clarify this point, we now include maps of cloud cover (Fig. A1) in response to the reviewer's first comment. As noted above, we acknowledge that the manuscript did not emphasize the role of cloud cover sufficiently, and we have revised the text accordingly.

Regarding the reviewer's suggestion, we note that only the ECMWF dataset reproduces a realistic surface heat budget. We recognize that several additional parameters in the Reed formulation—beyond the cloud thresholds—would require calibration, including the coefficients associated with cloud cover and solar altitude, the atmospheric transmission coefficient, and the parameter A_α. However, such a calibration exercise lies beyond the scope of the present study, as it would require extensive in situ and satellite observations specific to the Mediterranean Sea. Our goal here is to conduct a conceptual experiment using consistent bulk formulations applied to different atmospheric datasets, with particular emphasis on the ECMWF analyses, which to our knowledge have not been employed in this context before.

Finally, even if I understand and find fair the objective of having the same fluxes computation method and same SST for both datasets, I would have appreciated a brief comparison with the SW, LW, LH and SH fluxes directly taken from ECMWF and ERA5.

ECMWF analysis datasets do not provide directly surface fluxes but only forecast fluxes, initialized from the analyses. However, ERA5 contains fluxes, and we have now plotted them in Fig. A2 below.

Firstly, we note that the net surface heat budget derived from ERA5 fluxes is +5.3 W m^{-2} (Fig. A2, left panel), consistent in sign with the estimate obtained using our parameterizations. Thus, the different terms distributions do not affect the Q_{net} results. The largest discrepancies are observed in the longwave (LW), shortwave (SW), and latent heat (LH) components. For the radiative terms, the combined SW and LW flux differences are considerably smaller (Fig. A3) due to compensating effects, resulting in net deviations ranging from −5 to +20 W m^{-2}. These differences fall within the range of uncertainty reported in previous observational studies (Marullo et al., 2021; Pettenuzzo et al., 2012). Marullo et al. (2021) further reported that ERA5 exhibits a significant negative bias (−17 W m^{-2}) in longwave irradiance relative to in situ measurements, which is consistent with our findings showing notably lower LW fluxes in ERA5 compared to our estimates.

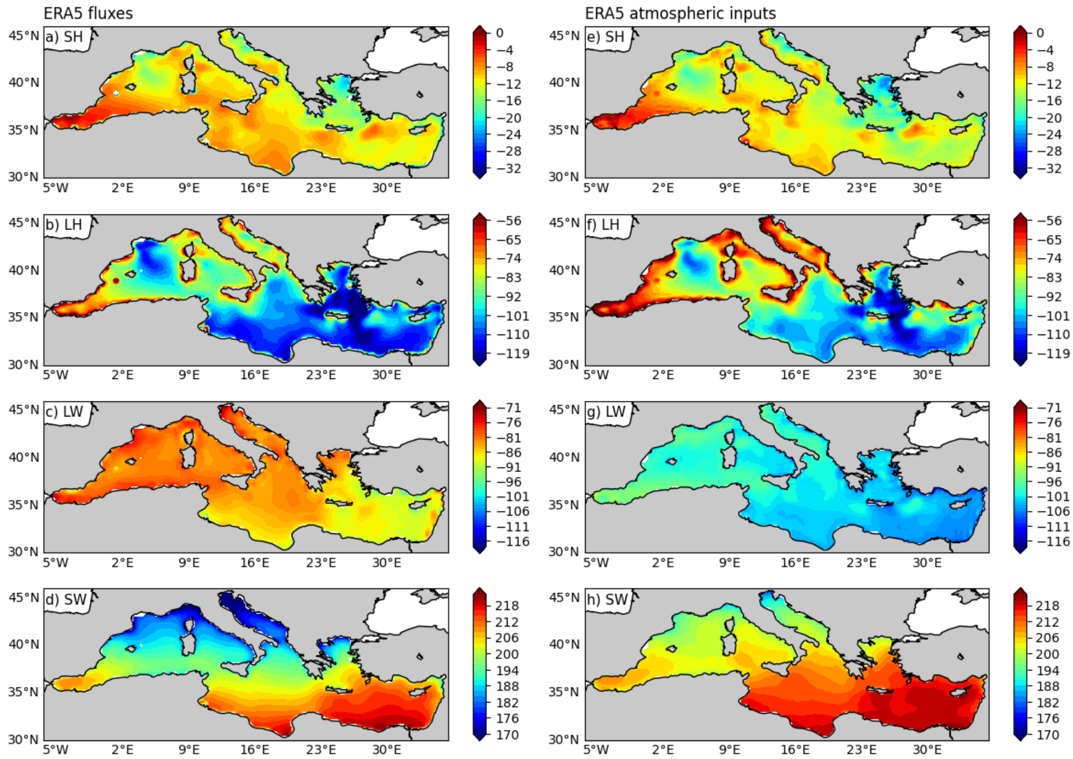


Figure A2: Mean annual heat flux components for the period of 2006-2020 computed from ERA5 fluxes (left) and ERA5 inputs (right) using equations (2-10)

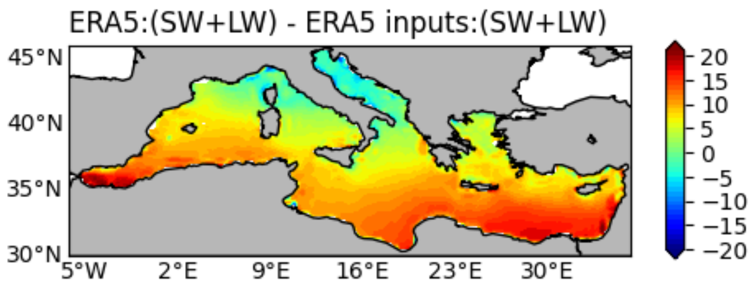


Figure A3: Difference map of the means of ERA5 fluxes (SW+LW) and computed fluxes (SW+LW) using ERA5 atmospheric inputs only.

We now have inserted Fig. (A2) and (A3) in the supplementary materials (as Fig. S7 and Fig. S8) and we added a comment after line 455 in the discussion and conclusions.

We compared the ERA5-derived surface radiative fluxes with our own estimates and found that the ERA5 longwave (LW) fluxes are substantially overestimated in absolute magnitude (Supplementary Material, Fig. S7). The associated uncertainty is comparable in order of magnitude to that reported by Marullo et al. (2021), who analysed an observational dataset at a specific site. Nonetheless, compensating biases between the SW and LW components (Supplementary Material, Fig. S8) result in a net radiative balance that diverges primarily in the southern Mediterranean, where ERA5 exhibits reduced LW flux values. These findings indicate that the ERA5 dataset may not adequately represent surface radiative fluxes in the Mediterranean Sea.

I put below some minor comments.

p5, eq.2: add information in text about the threshold; and unit for *C*.

- Corrected the eq.2 "if $C \geq 0.3$ and if $C < 0.3$", and added in text "C (%)"

p5, line 140: what is *sec* ?

- Sec represents secant ($\sec(\theta)$) of zenith angel of the Sun

p6, eq 8: why did you not use directly the specific humidity?

- We followed the exact formulation used in Large (2006) and Petenuzzo (2010) using dewpoint temperature data to compute specific humidity.

p7, line 189: *... the following* atmospheric near-*surface variables…*

- Corrected "the following atmospheric near-surface variables"

Fig. 1: Could a column with difference maps for each term be added?

- The maps are based on their original spatial resolution, and we didn't regrid the atmospheric datasets into a common grid for comparison.

P8, line 210-212: ... *The largest mean sensible heat* gain *is observed… Gulf of Lion* loss *more…* [negative is heat loss]

- Corrected "The smallest mean sensible heat loss is observed …….. Gulf of Lion loses more .."

p8, lines 221-223: The first reason for SW differences between Western Mediterranean and Eastern Mediterranean is the latitudinal position of each sub-basin.

Added "The first reason for SW differences between Western Mediterranean and Eastern Mediterranean is the latitudinal position of each sub-basin. Furthermore, SW differences using ECMWF and ERA5 datasets are connected to different cloud cover schemes, leading to a larger heat gain in the Eastern Mediterranean"

P8, line 227: *… presumably due to the* warm *Atlantic* surface *inflow…*

- Corrected: "presumably due to the warm Atlantic surface inflow…"

p13, line 328: From Fig. 4b μ is mostly positive. Please modify the sentence.

- Corrected "the location parameter (μ) exhibits mostly positive values while a small area in the Alboran Sea show negative values, ……"

P15, line 395: *...with long term climatology values for the extreme heat losses days…* : Could you precise how is built this climatology?

- We added: “These extreme values were replaced with long-term daily climatological values (using equation 11) to the respective days of extremes heat losses occurred ”

P16, line 400-401: The differences in Qnet between ECMWF and ERA5 is mostly due to differences in SW. Please review the whole paragraph.

We thank the reviewer for pointing out this unclear statement. In this picture we discuss the sensitivity of long term Q_{net} basin average values to the extremes of the time series shown in Fig. 6. In Table 2 we show that the Q_{net} without extremes becomes positive, as it is for ERA5 in Table 1. It is true however that a comparison between extremes of ERA5 and ECMWF has not been done, but we argue that it is unimportant since ERA5 has already a Q_{net} positive.

We have substituted the phrase at lines 400-401 with the following:

We argue that the ECMWF net heat extremes are the reason why ECMWF has a negative long term mean budget.

P17, line 424: ... *of -289* W/m^2 *(for k=0.75)...*

Corrected thanks and phrase changed.

p17, line 427: Please add the map of differences between Fig8 and Fig. 3a to put in evidence this result.

We thank the reviewer for the interesting suggestion, now the impact of extremes is clearer.

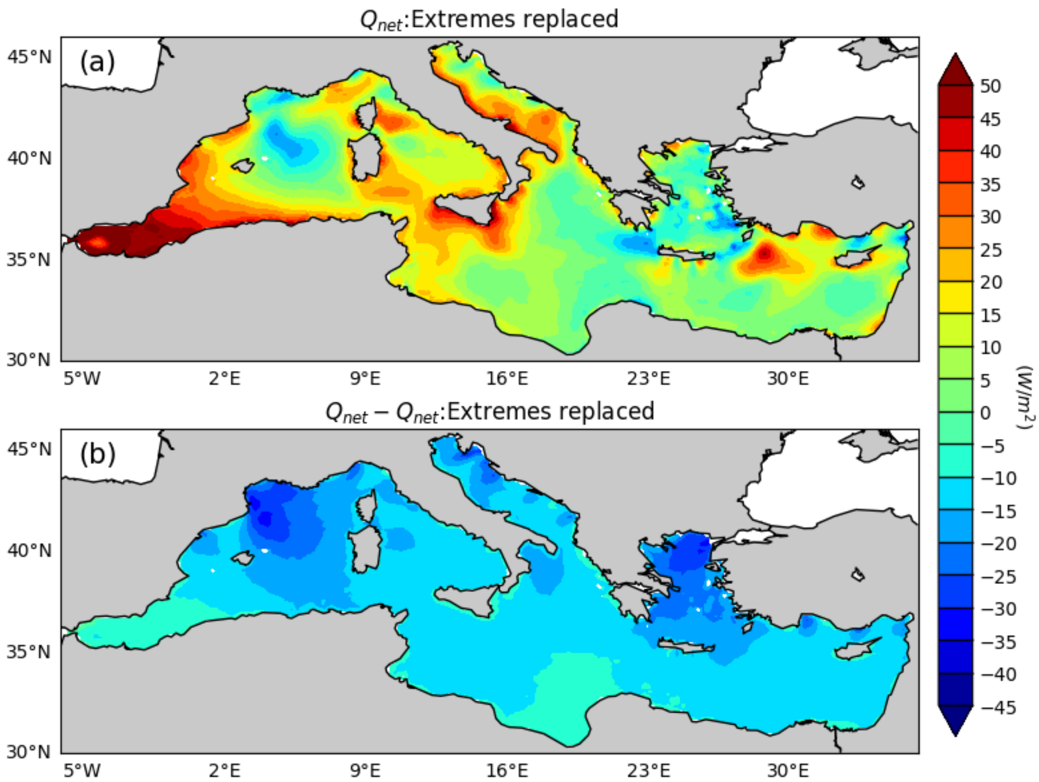


Figure 8: (a) the Q_{net} after extremes have been replaced with climatology and (b) the difference map between the total Q_{net}, shown in Fig. 3a.

We replaced the Fig. 8 with the new one and changed line 423-427 with the following:

Figure 8 presents the new long-term mean spatial distribution of the surface heat budget after removing negative extreme values using a threshold of -289 W m^{-2} ($K = 0.75$). The figure illustrates that these extreme events exert a substantial influence on the overall structure of the net heat budget in the Mediterranean Sea, with particularly pronounced effects in the Gulf of Lion, the Aegean Sea, the eastern Adriatic Sea, and along the southern Turkish shelves.

P19, line 486: minus sign is missing.

Corrected, thanks