

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 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.”

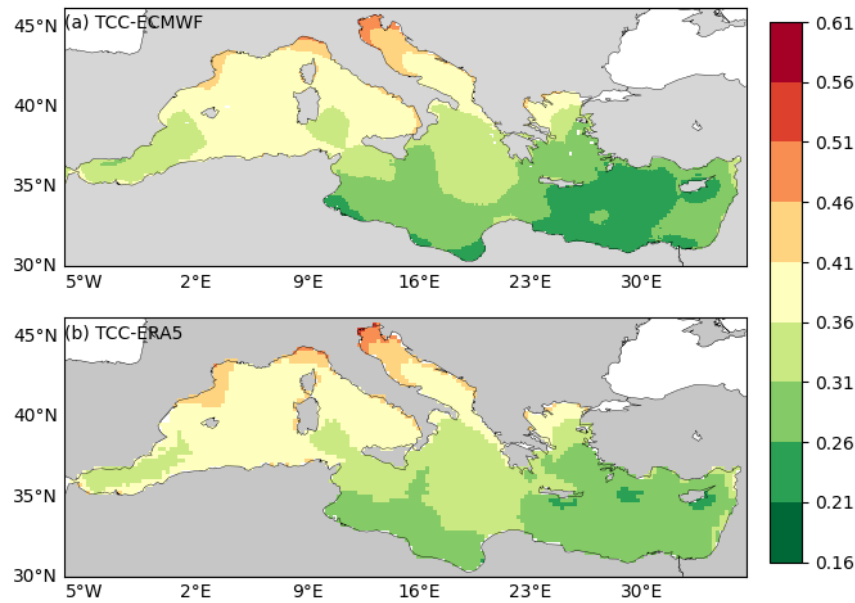


Figure A1: Total cloud coverage (%) mean computed for the period 2006-2020, a) ECMWF and b) ERA-5 input datasets.

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.”

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:

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

57 After line 76 we discuss the hypothesis behind the “closure of the heat budget”:

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

63 -----

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

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

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

72 “Our initial question was: is the Mediterranean Sea in the past 15 years still losing heat at the
73 surface? The answer is yes if we use ECMWF atmospheric analyses. Additionally, comparing
74 the Q_{net} estimates derived from ERA5 and ECMWF with the same bulk formulas demonstrates
75 that the uncertainty peaks in the atmospheric forcing resolution and possibly cloud cover. This
76 uncertainty impacts the regional heat budget closure hypothesis.”
77

78 -----

79 I have also main concerns related to:

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

86 Thanks for asking this question. We used the anomaly of latent heat (LH) distribution with
87 respect to daily season cycle, where exhibits positive and negative values. We believe, there is
88 no condensation and supersaturation dynamics in the full computed LH time series which
89 remains consistently negative. Following Pettenuzzo et al. (2010), we used constant turbulent
90 exchange coefficients, which are multiplied by stable and unstable condition parameters and
91 updated in final computation using maximum and minimum wind speeds condition.
92

93 -----

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 atmospheric dataset? Should the threshold to define clear sky be adapted and?

We agree that cloud coverage difference generated large variation in SW fields (Fig. 1 d, h). Here we presented the maps of cloud coverage (Fig A1). We would prefer not to change the clear sky Reed (1977) formula because the concept is to use same formulation with different atmospheric datasets.

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

Thanks for your comment. We think we have already clarified this part in the answer to your initial remark.

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.

First, we point out that the net heat budget from the ERA5 fluxes is $+5.3 \text{ W/m}^2$ (Fig A2, left panel), which is again positive. Secondly, we see that the major difference is in the LW and SW radiative components, but the changes compensate giving a similar net radiative balance (Fig A3)

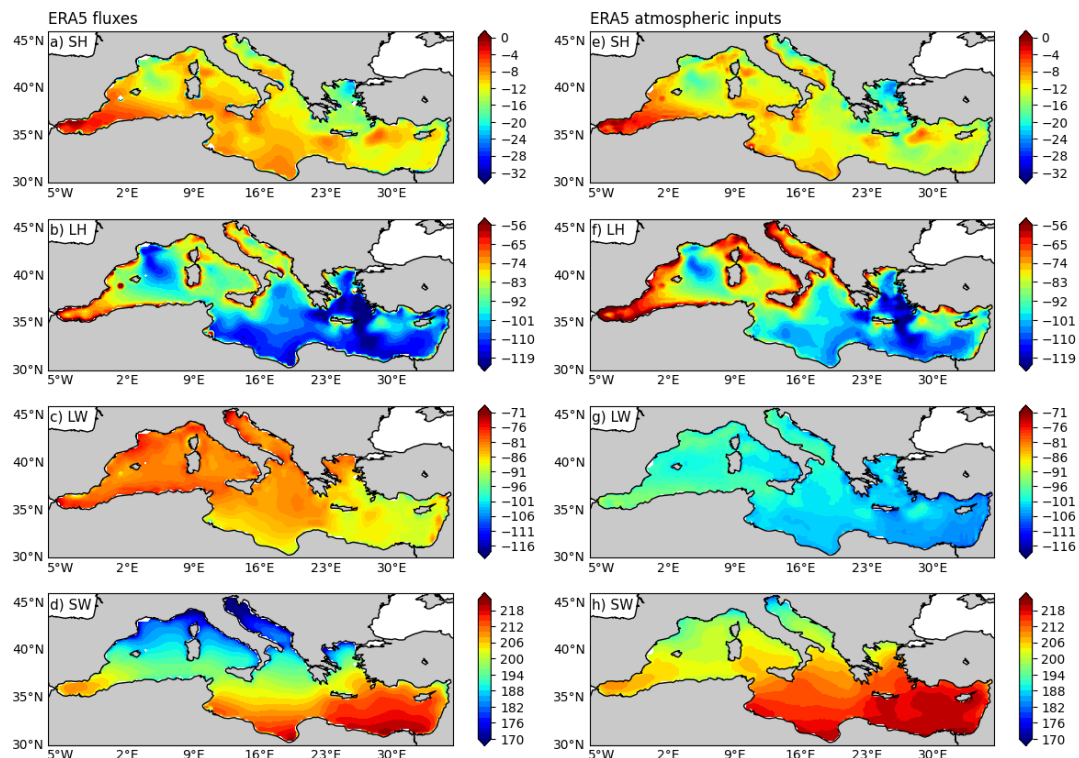


Figure A2: Mean annual heat flux components for the period of 2006-2020 computed from ERA5 fluxes (left) and ERA5 inputs (right) using equations (2-10)

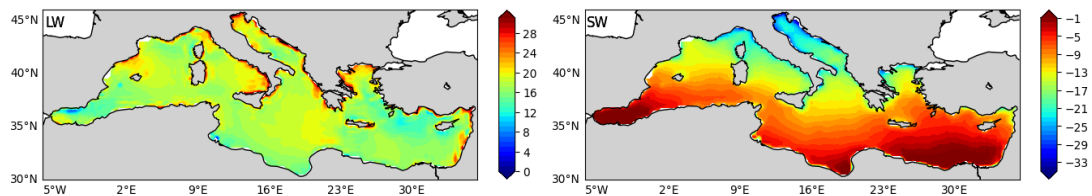


Figure A3: Differences (ERA5 fluxes - Computed fluxes) of the ERA5 fluxes and computed fluxes using ERA5 atmospheric inputs for LW and SW

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 (%) is the cloud coverage converted into fraction.

p5, line 140: what is sec ?

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

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

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

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

137 - Corrected “the following atmospheric near-surface variables”

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

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

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

143 - Corrected “The smallest mean sensible heat loss is observed Gulf of Lion loses
 144 more ..”

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

147 Added “The first reason for SW differences between Western Mediterranean and Eastern
 148 Mediterranean is the latitudinal position of each sub-basin. Furthermore, radiative heat fluxes
 149 using ECMWF and ERA5 datasets are connected to different cloud cover schemes. The
 150 difference in SW radiation between the western and eastern Mediterranean indicates the cloud
 151 cover differences, leading to a larger heat gain in the Eastern Mediterranean”
 152

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

154 - Corrected: “presumably due to the warm Atlantic surface inflow...”

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

156 - Corrected “the location parameter (μ) exhibits mostly positive values while a small
 157 area in the Alboran Sea show negative values,”

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

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

162 P16, line 400-401: The differences in Q_{net} between ECMWF and ERA5 is mostly due to
 163 differences in SW. Please review the whole paragraph.

164 We thank the reviewer for pointing out this unclear statement. In this picture we discuss the
 165 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 that a comparison between extremes of ERA5 and ECMWF has not been done for the extreme, but it will be somewhat irrelevant since ERA5 has already a Q_{net} positive.

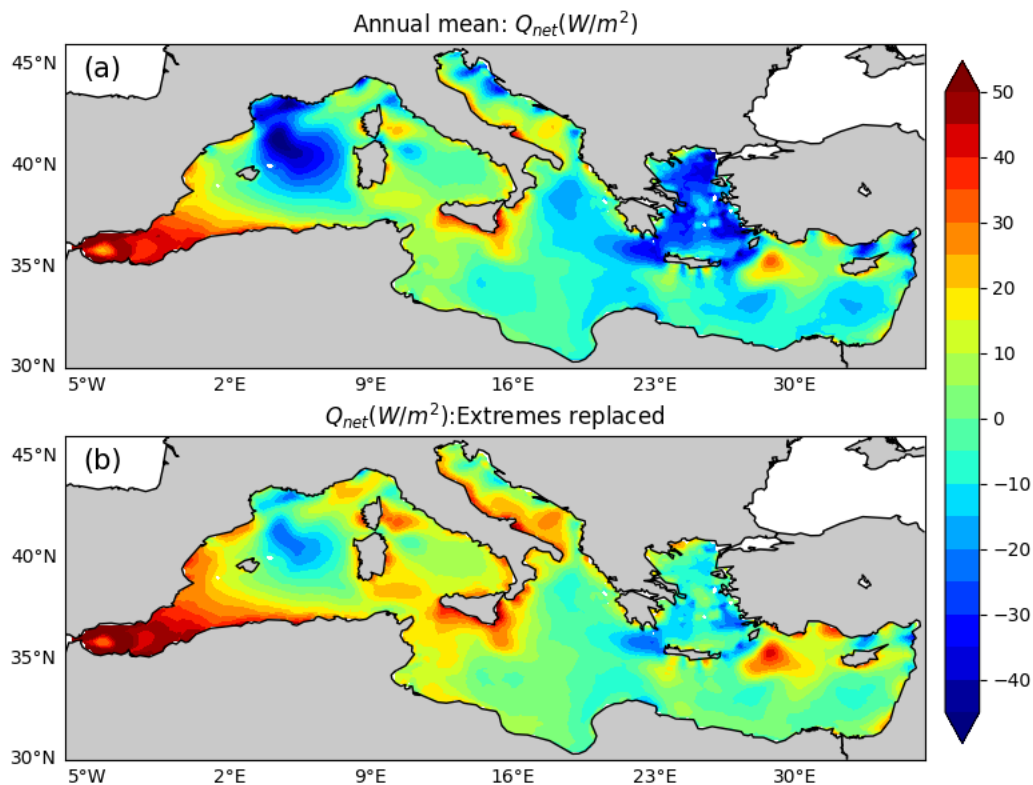
We have substituted the phrase at lines 400-401 with the following:

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

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



New Figure 8: The annual mean after the removal of extremes with significant reduction of negative heat fluxes in the Gulf of Lion, Adriatic Sea and Aegean Sea regions.

P19, line 486: minus sign is missing.

Corrected, thanks

According to my main remarks, I recommend a major revision of the paper.