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Interactive comment on "Temporal evolution of Red Sea temperatures based on insitu observations (1958–2017)" *by* Miguel Agulles et al.

Miguel Agulles et al.

magulles@imedea.uib-csic.es

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Interactive comment on "Temporal evolution of Red Sea temperatures based on insitu observations (1958–2017)" by Miguel Agulles et al. Anonymous Referee #3 Received and published: 9 October 2019

This paper takes generally available in situ temperature profile data for the Red Sea and Gulf of Aden, combines it with newly available data to create long-term climatological mean fields of surface and subsurface temperature as a baseline for time series of month/year temperature fields (surface and subsurface) for all months for

years 1958-2017. Error estimates are calculated from subsampled GLORYS reanalysis data. Some discussion of season, interannual, and decadal variability is included, with decadal trends of opposite sign at the surface and at 125 m depth. This work is definitely of interest, both for the climatological mean fields of tempera- ture in the Red Sea and Gulf of Aden, and for the analysis of seasonal to decadal changes in the temperature field, with their influence on the climate and biota of the region. The authors write very clearly regarding the method used, with a particularly C1

nice explanation of optimal interpolation and of the calculation of error statistics. A more thorough examination of the data would improve the paper, as would validation of the subsurface long-term mean fields against existing products, and more discussion of results, particularly trends of opposite sign at different levels in the water column. Details below.

-We thank the reviewer for his/her overall positive evaluation. In the following we try to address all the his/her comments.

First, the addition of the KAUST data set is a welcome augmentation of existing data for the Red Sea, especially with the possibility of continued monitoring by this source. I do not know the data policy for this journal, but the data used within the paper should be publicly available for reproducibility. The authors should note in the paper where the data can be obtained.

- The final product will be made freely available in the Pangea repository once the paper is accepted. This sentence is added at the end of the paper, in the acknowledgments section.

Figure 2 shows a rather startling distribution of temperature values in the Red Sea, especially with what appears to be a very large number of profiles with temperatures well outside the range of Red Sea temperatures at deeper depths. It would be a great service if the authors could detail the data a little more especially those which they state must have erroneous positions. This would help users (and maintainers) of CORA and similar data sets to examine and either flag or correct the erroneous data. Did the authors use CORA quality flags? Did these erroneous data have CORA quality flags?

- Yes, we used the CORA quality flags to discard suspicious profiles. Specifically, we downloaded quality flags related to temperature and depth and only kept those profiles flagged with a value of 1 (Good data). It must be said that as a previous control we also tried to keep observations with flags equal to 2 (Probably good data), with the intention of applying a postprocessing, but this approximation does not increased the number of good profiles. Therefore, we decided to use the more restrictive selection keeping only profiles flagged as “Good Data”. Regarding the control of erroneous positions, CORA quality control process considers “bad location” those profiles on land positions (positions more than 5km distant from nearest coastline with elevation above 50m). As you can see in the next figure (Figure 1-RC3) for the Red Sea, no observations are located on land, so CORA flags were not available to identify the mislocation of the profiles we have discarded. The full description of CORA database flags are obtained from (http://resources.marine.copernicus.eu/documents/QUID/CMEMS-INS-QUID-013-001b.pdf).

Figure 7shows a patter of RMSE Glorys vs. climatology (and optimal algorithm) that appears suspicious -with what looks like the exact same pattern in the 1960s, 1980s, and 2000s centered at 1000 m with the intermittent decades showing near zero error. Can the authors explain this?Is it some kind of decadal cycle embedded in Glorys, rendering it maybe less than useful for error analysis?

- The reviewer is right noticing this periodicity in the diagnostic. The reason is that, in order to obtain RMSE Glorys Vs Optimal Algorithm (Figure 7b in the paper) we needed to extract the observation locations of observations for the whole period of CORA (60 years, 1958-2017), but Glorys record is only 23 years long (1993-2015). Therefore, we concatenate the 23 years of Glorys till cover the time of observations, so we can extract the CORA locations for the whole period. It must be noted that we do not really care about the actual values of Glorys. We only use it as a synthetic reality so we can

test the impact of the mapping procedure.

It also might be nice to enlarge the upper few hundred meters where the largest errors are found, but hard to see in the full vertical graphic.

- We thank the reviewer for the suggestion. We have modified the vertical axis of Figure 7 to increase the zoom in the upper layer.

The long-term climatological mean field is discussed at length, but only validated with a comparison with AVHRR at the surface. It should be compared at subsurface depths to the World Ocean Atlas 2018 (WOA18) field, which are on the same grid size (0.25 x 0.25) and over nearly the same time period (1955-2017) - or another widely used long-term global climatological mean field. This comparison could yield some interesting results as to the efficacy of concentrating on a specific region, instead of using a region of a global climatology, with attendant extra attention, quality control, and in this case new data sources. C2

- Thank you for this suggestion, the comparison with another widely used product is really worth to be done. We have checked the availability of WOA18 database but just climatology fields are available at the repository. Therefore we have compared our TEMPERSEA product with another well-known hydrographic gridded product used in the IPCC reports (Ishii et al., 2003) In spite of having coarser spatial resolution (1°), it provides monthly field temperatures from 1955 to 2012, thus allowing a more in – depth comparison for the common period. Several diagnostics have been computed. First, we compare the annual mean temperature at three different depths (at surface, at 125m and 325 m of depth), both for the Red Sea and the Gulf of Aden (Figure 2-RC3 and Figure 3-RC3). It can be seen that both products are highly correlated in the upper layer, while they differ much more in the subsurface layers. This is also confirmed by the second diagnostic, the spatially averaged RMS difference computed each month for the whole domain (Figure 4-RC3). There is a clear maximum in the RMSD at 125m. Unfortunately, there is not independent data that could be used to decide which product is more accurate. Therefore, we have decided to not include this comparison in the paper as we are not able to show the added value of the product.

Grid size - sampling strategy: is a 0.25 x 0.25 degree grid really necessary to capture temperature change in the Red Sea? According to the authors discussion, less than 10 temperature profiles per month are necessary to adequately quantify temperature change in the Red Sea. If that is truly the case, would not a 1.0 x 1.0 grid along the axis of the Red Sea be sufficient to capture temperature change?

- To better define the changes of the temperature along the abrupt coast of the Red Sea and his characteristic strait in the South, it is necessary to work with a relatively fine grid. Even if the final structures have large characterisic length scales, we prefer to provide the data in a way that properly capture the coastlines. Moreover, there is another mathematical reason. If the characteristic length scales are about 100-150km (as computed from the Glorys data), we need at least 4 grid points to properly capture those structures, so 0.25° is the minimum resolution to be in the safe side.

In figure 4, it is very hard to see the grid structure used - is there another way to represent it? Maybe just in black and white rather than color?

- Thanks for the suggestion, we have modified the figure in the paper.

But assuming there are multiple grids laterally across the Red Sea at each latitude, it appears that the K-mean algorithm aggregates data into one or sometimes two grid areas across the Sea longitudinally (Figure 5). These appears to lose any advantage of a 0.25 x 0.25 grid resolution. It may be due to the graphic, but the authors should spend some more time discussing the importance of the 0.25 x 0.25 grid resolution to this work.

- We think the reviewer has misinterpreted the figure. The goal of the K-means is to reduce the number of observations at the time of computing the background field. We do that clustering them, so we can remove points that would provide redundant

information for the computation of the climatology. Then, the spatial analysis for the background field is performed on the standard 0.25° grid. For the monthly analysis the number of observations is much reduced so we can use all of them in the mapping procedure and take advantage of the periods/locations when/where there are many observations.

Time frequency: similarly, what is the advantage of the month/year time frequency (12 monthly temperature fields in the Red Sea per year 1958-2017)? As the authors note (with the term "surprisingly" though I dont think it should be surprising to the authors who are familiar with historic measurement strategies in the Red Sea) there are many months without any data at all in the Red Sea, and other months with very few measurements. Seasonal temperature cycle in the Red Sea is examined from a climatological (long-term) perspective. I dont see any particular explanatoin of the advantage to month/year fields over simple yearly fields in quantifying and discussing interannual and decadal variability, especially for data sparse years. The authors should do a little more explanation of why monthly fields are produced. At the least a matrix of coverage (or lack thereof) for each month/year should be presented graphically. This would give a better understanding of data sparsity influence on error, as a companion to figure 21.
C3

- Even if in our analysis we only make advantage of the monthly fields when computing monthly std variability, we strongly believe that it is better to provide the product at the highest time resolution. Then, the users could decide at which level would they like to aggregate the data. It has to be kept in mind that during some periods there were enough profiles to accurately characterize monthly variations as can be seen in the following figure (Figure 5-RC3). In the product the periods of better quality are reflected in the error maps and error time series as discussed in the text.

In discussing results, the authors note that most interannual variations in the upper layer of the Red Sea can be explained by large scale changes in the air temperature. This is not completely convincing. There is a good correlation, but isnt it equally as likely that it is the air temperatures influenced by the upper ocean temperature rather than the other way round? Short wave radiation as well as trapped long wave radiation is absorbed by the ocean surface and radiated back at a slower rate to the lower atmosphere. A little more discussion would be needed to convince that it is large scale air temperature which is the major factor in the upper ocean.

- When preparing the first version of the manuscript we had a thorough discussion with atmosphere scientists about this issue. They suggested to use air temperature at 850mbars (roughly 1500 m height) to ensure that the ocean feedbacks are minimized. We agree that the sea has an effect on the air temperature, but this is restricted to the lower layers. Air temperature at 850 mbars (1500 m height) is too far from being significantly affected by the sea temperature of a small region like the Red Sea. In the manuscript we have added a sentence clarifying that 850mbars correspond to 1500m height. Also we discuss the correlation with air temperature at two heights, close to the sea surface (1000 mbars) and at 850 mbars, showing that correlations are higher close to the sea surface due to the air-sea feedbacks, but that correlations with temperature at 850 mbars is still very high.

One of the remarkable features the authors find is that upper ocean temperatures are increasing (decadally) but lower depths are decreasing. How can this be if the main factor in the temperature change is air temperature, and there is little exchange with any water source outside the Red Sea? It may be that the answer has to do with the interannual change in the depth of the thermocline.

Figure 14 shows thermocline depth seasonal change. Thermocline depth in the south is fairly constant over the year, but changes in the north. If the thermocline were to shallow in February say, cooler water would be higher in the water column and heating would be concentrated closer to the surface, creating the opposite sign trend pattern with depth shown by the authors. This is speculation, but it would be worth a bit more investigation by the authors to validate and maybe explain the change in sign for decadal trend.

- We don't really have an explanation for this discrepancy between the long term evolution of both layers, and it is out of the scope of the paper to run a full analysis on this interesting issue. Regarding the interannual change in the depth of the thermocline we do not understand why that would explain the long term discrepancies between layers, as the thermocline depth is a diagnostic, not a mechanism.

Small things - line 98: what does "delayed mode" mean here? - lines 116, 117, if OSTIA and ICOADS are acronyms, they should be defined. - line 132, "sea-ice concentration" maybe could be removed. GLORYS may assimilate but it is irrelevant in the Red Sea. Thanks for the comments. We have updated the paper with those corrections. it would be nice, in figure 3 to give some indication of the data which came from KAUST as opposed to CORA.

- We have included a dashed line in Figure 3 to indicate the observations coming from KAUST.

line 204, add space between "as" and gamma. - line 206, "pof" should be "of"

- Thanks for identifying the typos, they have been corrected (L213 and L215).

lines 315-317, why would satellite data from the top mm of the water column have a larger variability than in situ data from 2-4 m?

- It is stated in the manuscript that the product is representative of the first 4 m of the water column. Therefore, one can expect that that fraction of the water column is less responsive to changes in the forcing that the first mm of the water column (as it involves more mass). Consequently the variability is somehow damped.

line 412, "imposed to" should be "imposed on" - lines 493-494, lateral advection seems to play an important role..." replace "seems to play" with "plays" if there is actual evidence for this. - line 561, "specially" should be "especially"

- Thanks, these have been corrected.

lines 565-566, "Our results show that multidecadal variations have been important in the past and can bias high the trends from 30-40 years of data." How can multidecadal trends, presumably a cycle, bias high trends? Are the authors referring to multidecadal trends which are not fully represented in 30- 40 years?

- Yes, this is exactly what we mean. Multidecadal variations (not trends) not fully represented by the 30-40 years of data can enhance/reduce the underlying long term trends.

It appears from figure 19 that this could be so in this specific case, but as a generality a partial cycle could bias trends either high or low. Authors should either remove "high" or refer specifically to the Red Sea trend.

- We agree, we have removed the adjective "high"

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Please also note the supplement to this comment:
https://www.ocean-sci-discuss.net/os-2019-66/os-2019-66-AC4-supplement.pdf

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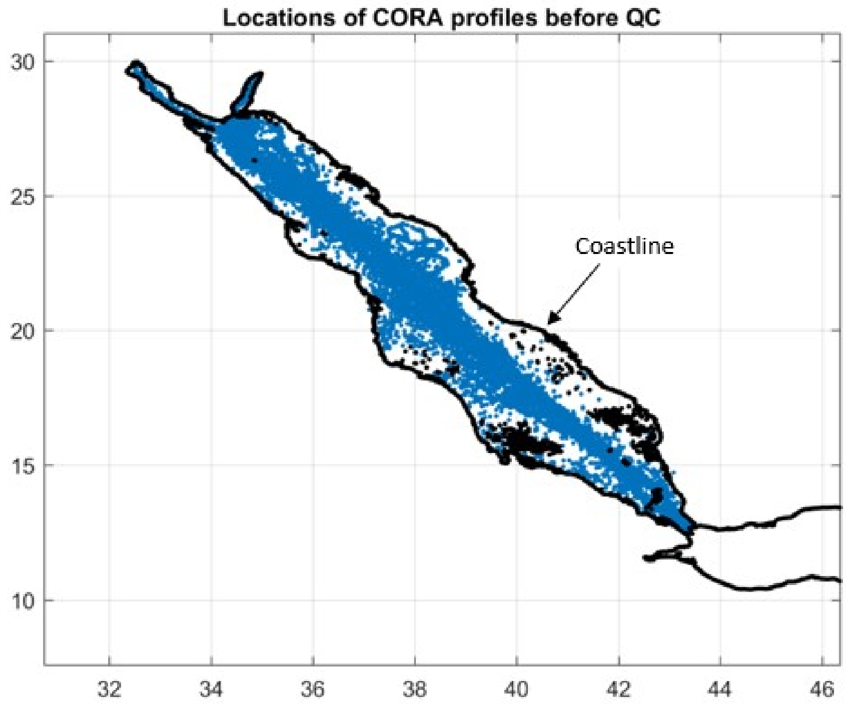


Fig. 1.

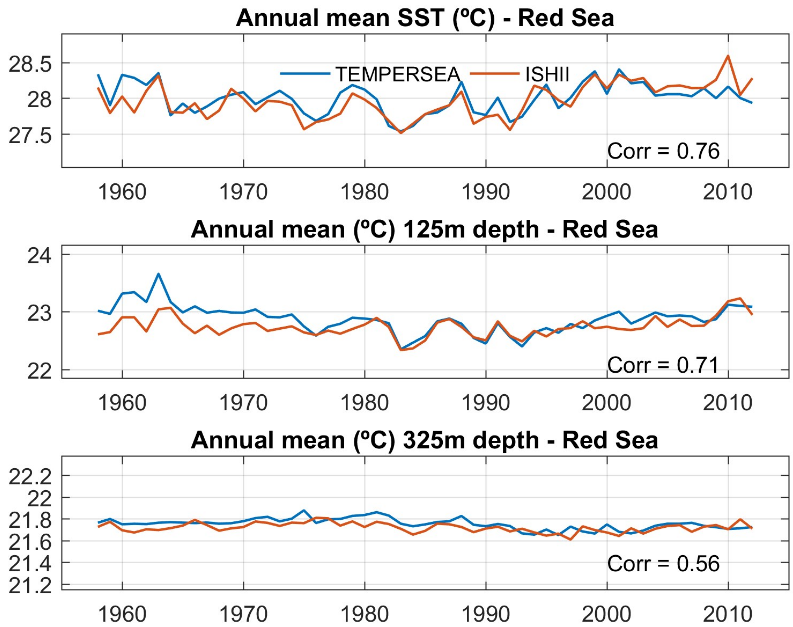

Annual mean SST (ºC) - Red Sea
TEMPERSEA
ISHII
Corr = 0.76
Annual mean (ºC) 125m depth - Red Sea
Corr = 0.71
Annual mean (ºC) 325m depth - Red Sea
Corr = 0.56
1960
1970
1980
1990
2000
2010

Fig. 2.

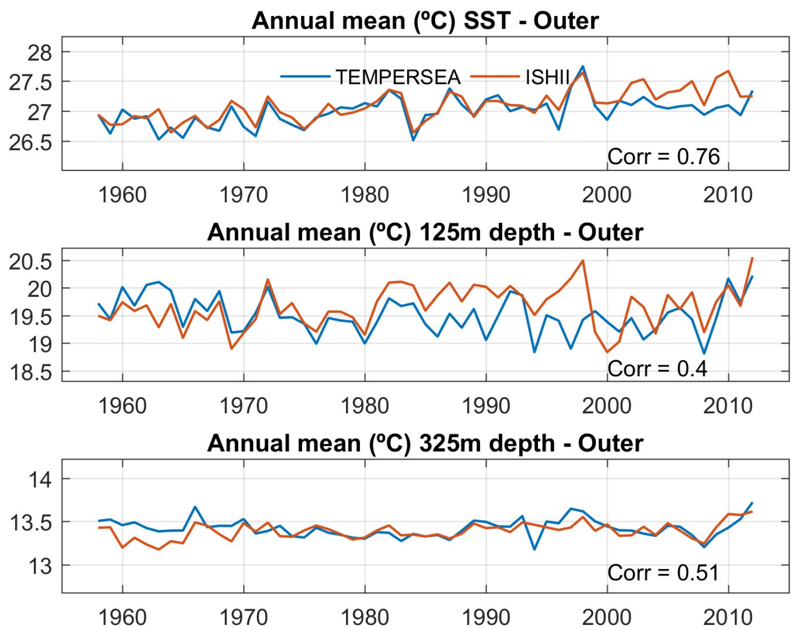
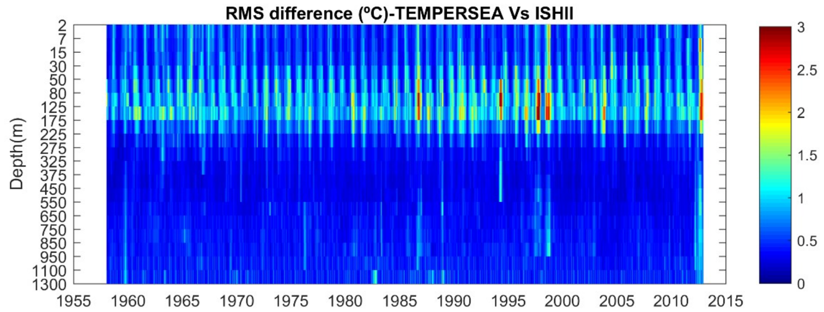
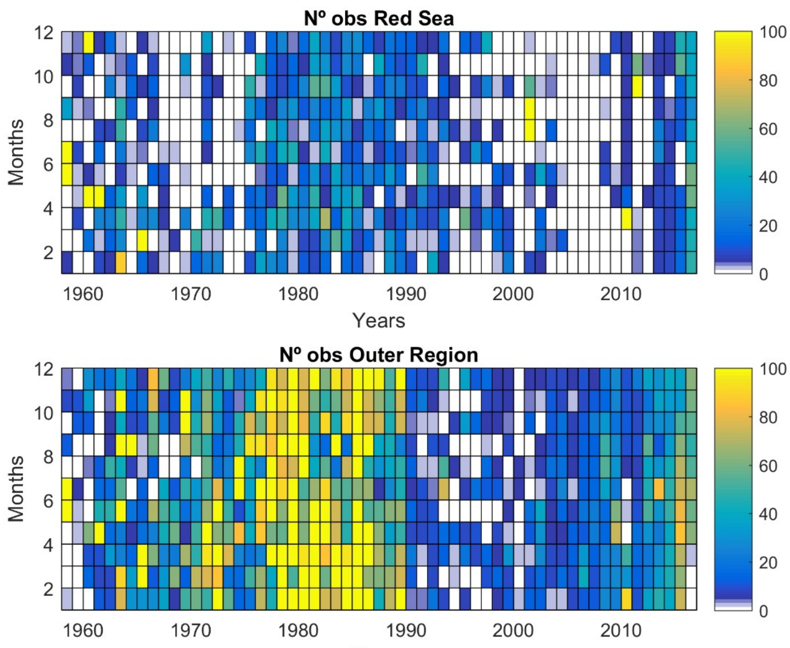

Annual mean (ºC) SST - Outer
TEMPERSEA
ISHII
Corr = 0.76
Annual mean (ºC) 125m depth - Outer
Corr = 0.4
Annual mean (ºC) 325m depth - Outer
Corr = 0.51
1960
1970
1980
1990
2000
2010

Fig. 3.



RMS difference (ºC)-TEMPERSEA Vs ISHII
Depth(m)
2
7
15
30
50
80
125
175
225
275
325
375
450
550
650
750
850
950
1100
1300
1955 1960 1965 1970 1975 1980 1985 1990 1995 2000 2005 2010 2015
0 0.5 1 1.5 2 2.5 3

Fig. 4.



Nº obs Red Sea
Months
2 4 6 8 10 12
1960 1970 1980 1990 2000 2010
Years
0 20 40 60 80 100
Nº obs Outer Region
Months
2 4 6 8 10 12
1960 1970 1980 1990 2000 2010
Years
0 20 40 60 80 100

Fig. 5.