Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2019-66-RC1, 2019

Ocean Science
Discussions
Open Access

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

Anonymous Referee #1

Received and published: 27 August 2019

This work analyzes a large data set of temperature profiles obtained in the Red Sea from 1958 to 2017. The data sources are several data collections. The analyses are differentiated for three different areas: The northern and the southern Red Sea, and an outer area to the east of the Bad-el-Mandeb Strait. The analyses presented are quite exhaustive and include a description of the quality control process, the data interpolation method, and an inter-comparisons with model and SST satellite data. First the seasonal cycle of temperature for the different regions and depth ranges are analyzed and then the inter-annual and multidecadal variability is addressed.

In my opinion this work is very exhaustive and interesting. The main objectives of establishing the seasonal cycle of temperature as a function of the geographical location (Longitude/attitude) and depth, and studying the time variability at inter-annual

Printer-friendly version

Discussion paper

and multidecadal scales are achieved. The manuscript is well organized and, in general terms, well and clearly written. For all these reasons I believe it is suitable for publications with minor revisions.

My main concern is the lack of an analysis of the salinity data. I assume that many of the available profiles analyzed come from CTD profiles or Argo profilers and therefore salinity data are also available. The analysis of temperature is very interesting by itself, but it would be much more complete if the companion salinity information was included. Note that the Red Sea is one the places of the world ocean with a highest evaporation and therefore the salinity variability and possible alterations could be of paramount importance. Furthermore, the dynamics of the circulation of the Red Sea would be driven by the density field (despite the wind-driven circulation). If the temperature changes are compensated by salinity changes then the density field is not altered. I think it would be important to know if this is happening or not. I am not an expert in the Red Sea circulation, but as long as I know, there is a thermohaline circulation and a water exchange with the Indian Ocean in order to compensate for the strong evaporation. Once again this depends on the density field and the joined action of temperature and salinity. Nevertheless, I understand that the role of the reviewer is to review the present work, not to suggest a different work. For this reason I consider this as a minor concern. The analysis of the temperature data merits publication by itself and I simply suggest that including a salinity analysis would improve very much the work.

Other minor points. Introduction. Figure 1. For those people not familiarized with this region, a figure from a wider geographical area should be included in order to locate the Red Sea. Then, the present figure 1 could be a zoom from the larger area. At the beginning of the introduction (first paragraph), I miss a description of the Bad-al-Mandeb strait, mainly its maximum depth which I guess conditions the exchange between the Red Sea and the Indian Ocean. Otherwise, the introduction is clear and informative.

Line 107: “the data has been quality controlled. . .”. It is true that the quality control is explained later in section 2.4, but the first time I read it I wondered how had been done the quality control?. Please, include a parenthesis (see section 2.4) for impatient readers like me.

Lines 116 and 117. This is the first time that OSTIA and ICOADS appear. Have this acronyms an explanation? Please, include it.

Line 120: “Both OSTIA products are merged after a cross validation is performed”. What kind of cross validation? How was it carried out? Please, explain it just a little.

Line 143: “. . .to remove spikes, out layers and density inversions”. It is clear what a density inversion is, but the criteria to determine if a data point is an out layer is more subjective. Which criterion was been followed: two standard deviations from the mean value?, three?, those values beyond a certain percentile? Is the procedure the one explained in lines 150-155, or this is a different quality control? Why you use the 1% and 99% percentile criterion in some cases and the three standard deviations in other cases?

Lines 185-190. I do not like very much these sentences. In Optimal Interpolation, the observations are considered as composed by a background field, a signal and an error, which is not necessarily a measurement error, but simply the part of the observation corresponding to a length scale on which we are not interested. The interpolated values are estimated using the statistics of the signal (variance and decaying scale) and the signal/error ratio. So I believe that “the weights are determined from the statistics of the observational errors” is not a good description.

Expression (1) could be improved. When writing in the left had side of the equation V(r) it seems to me that it is the value of variable V at the coordinate vector r (you say at a “given position r”). Then you say that BK is a M-vector. In that case V is also a vector, or r is a vector of positions.

In expression (5) Tij/T, I guess the exponent should be negative in the same way the exponent for the spatial correlation is negative. Otherwise the correlation increases with time.

Figure 12. I would represent directly the values of the temperature for the climatology. In that way you would know the temperature for each month of the year for the climatological cycle. In the present way, you have to look at the mean temperature and then add the anomaly. It is not very helpful. In line 345 and followings it is stated that the minimum anomaly for the seasonal cycle, and then the minimum temperatures along the year (it would be better to see temperatures directly) are found in August in the outer part. Taking into account that this area in to the north of 10° N, therefore in the northern hemisphere, it seems strange to the reader not familiarized with this region of the world that the minimum temperatures are reached in August, when one expect the maximum ones in the northern hemisphere. I think that this result needs some more explanations for the non-expert readers like me.

You compare sea temperature with air temperature at 1000 mbars, considered as the air in contact with the sea, and at 850 mbars. I think that using 850 mbar temperature makes no sense. The heat exchange between the sea and the atmosphere depends on the temperature of the air above it. If the air at 850 mbar is very warm, but the air at the sea surface is cold, the cold air would enhance latent heat and sensible heat fluxes, no matter which is the temperature at 850 mbar. A different question is that 1000 and 850 mbar temperatures are very likely to be correlated, and therefore sea temperature and 850 mbar temperature are also correlated. My point is that we should not use time series to calculate correlations just because such time series are available. There must be some scientific reason. If you already have 1000 dbar temperature, please, do not use 850 dbar. It gives the false impression that there is some sort of phenomenon that can influence the sea temperature from the upper part of the atmosphere.

In line 373 you use the abbreviation std. I suppose it means standard deviation. Please, define it previously.

Some writing errors. Line 442: “the period cover by. . .” should be covered. Line 546. “the formal error from optimal interpolation have. . .” should be “has”.

Interactive comment on Ocean Sci. Discuss., https://doi.org/10.5194/os-2019-66, 2019.

Printer-friendly version

Discussion paper