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

Anonymous Referee #1

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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

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.

We deeply thank the referee's comments and the effort he/she made in carefully reviewing our work. In the new version of the manuscript we have implemented all the points raised in the review. Thanks to those advices, the new version of the manuscript has been improved.

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.

Thanks for the comment. We also believe that salinity is important, but there have been several reasons for us to not include its analysis in this work. The number of salinity observations in the basin is significantly smaller than the temperature. At the same time, the correlation length scales for salinity are smaller than those of temperature (Llases et al, 2016), so more data would be required to obtain a reliable product. Additionally, including salinity would require specific tests to calibrate the algorithm, and to quantify the uncertainties, which would involve a huge extra effort. For all this, we have prefered to focus on temperature characterization, specially considering that temperature has been recognized as the most influential factor for Red Sea ecosystems. We hope in the near future there will be enough salinity profiles thanks to the new observational systems that will allow us to produce an equivalent product for the salinity.

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.

This figure has been modified in the paper.

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.

This information has been added in the first paragraph of the paper (L43).

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 likeme.

Thanks for the suggestion. The parenthesis has been included in the revised article.

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

This has been updated in the revised article.

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.

That mergins is done by the OSTIA team. In particular, the cross validation of both OSTIA products is done estimating the bias in each product by calculating match-ups between each product and a reference data-set. The details of the procedure can be found in (Bell et al., 2000). This explanation has been added to the text (L121).

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?

The paragraph that explain this part has been modified to better explain the quality control process. The quality control has been done in three steps:

Firstly, spikes and profiles with density inversions have been removed in all the area studied (Red Sea and outer region). Secondly, those profiles in the Red Sea showing temperatures colder than 20ºC below 500 m have been removed. This has been done because no temperature below 20ºC has been found in the reference KAUST dataset at any depth. Finally, as a third step, for the rest of the profiles (in the Red Sea and outer Region), those lying outside a range defined by three times the standard deviation are also rejected.

The 1% and 99% are used just for visualization of the range of values in the reference dataset, which have helped to identify the 20ºC threshold mentioned about. This has also been clarified in the text

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.

In the original formulation of Optimal Interpolation (e.g. Gandin et al 1965) the weights of the background and the observations are defined in terms of the covariances of the background and observational errors. In the application of OI to atmosphere/ocean data those error covariances cannot be measured so they are defined using analytical formulations that involve a decay scale (e.g. Gaussian functions), and the error variance is substituted by the field variance. We agree that the original sentence in the manuscript was rather vague and we have corrected it. Now it reads:

"OI is an algorithm that estimates the optimal value of the field as a linear combination of available observations and a background (i.e. first guess) field, with weights determined from the covariances of observational and background errors"

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.

The reviewer is right. In the left side of the equation 1 we have removed (r). The left side represents the analysed field which is a vector, not just a point in a given position.

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,

The reviewer is right. We have corrected it. Thanks.

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.

Before initial submission of the paper we had discussed a lot about how to present the seasonal variations. We had prepared both figures (for absolute values and for anomalies) and we had no clear preference as both options have pros and cons. Following the suggestion of the reviewer we have modified Figure 12, so it shows the absolute values.

Regarding the minimum values observed in the Gulf of Aden in summer, they are caused by the advection of cold waters from the Indian Ocean. The description of the detailed mechanism introducing that advection is out of the scope of the paper. Nevertheless we have introduce a sentence in the manuscript (L360) that reads:

"These results suggest that the relative minimum found in the Gulf of Aden during summer could be induced by the advection of cold waters from the Indian Ocean."

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 850mbar 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.

We appreciate your opinion, and we try to explain here our point. The 1000 mbar temperature is the one in contact with the sea, but it is well acknowledged that the sea temperatures also modify air temperatures at the air-sea interface. Therefore, correlations between SST and 1000 mbar temperatures could be due to oceanic effects on the atmosphere. That is the reason why we decided to use the 850 mbar temperatures, not because there were correlations. With that variable we intend to characterize the temperature of the air masses not affected by the air-sea interactions, as stated in the text (L508). By doing this, it is easier to interpret the correlations found: the changes in the temperature of the air (i.e. advection of air masses) is what drives the temperature in the Red Sea.

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

Done.

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

This has been corrected

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