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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

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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 temperature 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

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.

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.

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?

Figure 7 shows 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? 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.

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.

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? 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? 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.

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.

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.

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.

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. - it would be nice, in figure 3 to give some indication of the data which came from KAUST as opposed to CORA. - line 204, add space between "as" and gamma. - line 206, "pof" should be "of" - lines 315-317, why wold satellite data from the top mm of the water column have a larger variability than in situ data from 2-4 m? - 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" - 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? 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.

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