Ocean Science

Discussions

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Interactive comment on "Mixed layer depth variability in the Red Sea" *by* Cheriyeri P. Abdulla et al.

Anonymous Referee #2

Received and published: 1 May 2018

General comments:

The paper is generally well-executed and well-written, although it does need some editing. The authors say that MLD results are not available from previous research, but I am not competent to judge that, so I will take them at their word on that point. There is a lot of good material here, but I also have reservations about some of it. These comments are summarized below. Please note that these comments are not in order of importance, but are in the order I encountered the material in the paper.

The bottom line is that the basic description is well-done and should be published. The special latitude bands identified in the correlation plot are not proven to be real, at least to my satisfaction, but the Tokar Gap signal at 19N is interesting and corresponds to a clear "tongue" in the MLD climatology. I'm ignoring a rule I agree with that we cannot

just point to features in a plot and interpret these without a "null" test that the feature could be noise, but we'll discuss that more down below. The paper would be much better if you were to get rid of the AVISO SLA analysis and Section 3.3 and the other latitude bands and focus on the overall description and the Tokar Gap results, again see discussion below.

I should say that after writing this review I read the comments by the first anonymous reviewer. This person gives a very thorough review, and we have points of agreement and disagreement. I think the major disagreement is how we view the material concerning the Tokar Gap winds and subsequent eddy spin-up. I really liked this material, but the first reviewer perhaps did not like it so much. I think this is for the authors and the editor to sort out.

Specific comments:

L40 – I am not sure that "deep water formation" is appropriate. Common usage of that term is for NADW and ABW. Perhaps "intermediate water formation"? At the least tell us how deep this high salinity water reaches.

L95 – 1 by 1 degree spacing is very coarse for this region. With such a model can you really expect to resolve the scales that are important in the Red Sea?

L108-115 – The AVISO SLA is HIGHLY suspect in the Red Sea for resolving eddies. Yes, they grid it at quarter degree spacing, but how much actual data is there? Also, their covariance functions for the OI are not tuned to the Red Sea in general, or to the Tokar Gap in particular. See below, too, but I would remove the AVISO SLA eddy material and focus on the Tokar Gap analysis that doesn't require it.

L122-129 – Well-done to switch to an along-axis coordinate system and to do the analysis in an along-axis, time space. Nice!

L150-162 – I'm worried that we are over-interpreting in this section. The general seasonal pattern in clear, but the along-axis changes are not so clear. Without error bars

it is difficult to tell when we are interpreting real changes, or just noise.

L160 – As an example of the comment I just made, I cannot make any sense of a "general pattern of deeper MLDs in the latitudes". I just cannot see in Figure 2 where this statement comes from! You will need to be much more specific to convince me of this and it will also require appropriate error estimates. You lose little by sticking to the bigger picture and skipping the "wiggles", although the "tongue" at the latitude of the Tokar Gap is interesting (see below).

L205 and Figure 5 – I like this plot, but some estimate of the degrees of freedom needs to be made so that we know whether the structure in the curve is real or just statistical fluctuations. And, as usual, the degrees of freedom estimate should consider red noise and not assume white, or independent, noise.

L211-215 – I think we are over-interpreting again. These are very small "wiggles" and 2 of the 5 do not even show the pattern you assert, meaning all three curves moving down together. Again, if you want to interpret these small changes, then you have to do a much more thorough job on the statistics to convince us that we're not just looking at random fluctuations.

L216, all of Section 3.3 – As I mentioned above the analysis using the AVISO SLA is highly questionable here. I think this entire section, and basically all use of AVISO, is not necessary for this paper. You have some good results, but by pushing too far you run the risk of most readers doubting everything. Please note that I am trying to be constructive here and help to improve the paper. I like the paper as a whole, but really do not like this section.

L258-260, Figure 6 – This is a continuation of the previous comment. You say that there is a good match with the number of eddies and the latitude ranges you identified earlier from the correlation curves, but I simply do not see that. And since we're just doing the analysis "by eye", then my eye is as good as yours. You really have to do some statistics if you want to make this point. And once again, none of these things

are core results of your paper.

L277, all of Section 3.4 – Much better! Looking at the 19N signal where the Tokar Gap is, and showing the actual wind results rather than AVISO eddy counts is much more convincing. And note that this is the only latitude band where I see convincing results. It's well-known that strong winds through mountain gaps generate the eddy signals you infer (search for results on the Hawaiian Ridge and the Gulf of Tehuantepec), so you do not need the questionable AVISO results in order to rationalize the "tongue" in Figure 2 at 19N. This is a very nice result.