

Ocean Sci. Discuss., referee comment RC1  
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## **Comment on os-2021-51**

Corinne Trott (Referee)

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Referee comment on "Sea surface salinity short-term variability in the tropics" by  
Frederick M. Bingham and Susannah Brodnitz, Ocean Sci. Discuss.,  
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Review of "Sea Surface Salinity Short Term Variability in the Tropics"

In this manuscript, the authors utilize data from satellite observations, moorings, and a high-resolution numerical model to compute and validate short-term variability in the tropics. The introduction section excellently describes the objectives of this work, is well-cited, and expresses the need for this type of study. The results are explained well and are supported by a more in-depth discussion section. However, I have minor questions about the authors' methodology. Due to their expertise in this field and past refereed publications on similar topics, I expect the authors to easily be able to address my concerns. For those reasons, I suggest minor revisions.

My comments:

Why choose a free-running model over a data assimilative one? In this case, it is more of an evaluation of model physics than a data-assimilative model, which might be more reflective of the actual ocean. Also, it would provide the opportunity for suggested improvements of short-term tropical variability of SSS in currently operationally used models.

Related to my last comment, some discussion on how this work may improve parameterization of models would be interesting. Where do you see the greatest value of this work? In SSS processing? Rainfall identification?

Abstract: How was 5-14 chosen? There is no other mention of it in the text.

Lines 43-44: "SMOS (Soil Moisture and Ocean Salinity) does not have a simple footprint due to its interferometric method of sensing and wide field of view." Has anyone attempted to compute this? Is there a range of values? Is it similar at similar latitudes?

Regarding the comparison between SFV between SPURS-1 and SPURS-2, what is the main reason for differing footprint sizes?

Section 2.3: I need some clarification on "weekly evaluation times." Is this via taking each mooring measurement +/- 3.5 days? Or days 1 thru 7, then 2 thru 8, then 3 thru 9, etc. for the full time period of 1992-2020 at every point? Are the initial dates of each ensemble evenly spaced throughout the year? How many ensembles are taken? Figure 2 is a clear portrayal of the analysis done for one segment, but more elaboration in section 2.3 is needed. There's no need for methodological changes, just more explanation.

Lines 155-160: Please add some exact numbers for “larger values”

Figure 1: If the direction of the arrows have no meaning, would it be better to color code each region with values corresponding to current magnitude (a la Fig 6)? If magnitude is the only important feature here, the arrow-length approach is difficult to clearly read. A more equilateral projection would also be easier to read and would allow for larger figures, but that is up to the authors.

Overall, this was a very well-written and thoughtful paper. I have a lot of confidence in the authors and will recommend publication once my concerns are addressed.