EGUsphere, referee comment RC1
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Comment on egusphere-2022-25

Anonymous Referee #1

Referee comment on "Four-dimensional temperature, salinity and mixed-layer depth in the Gulf Stream, reconstructed from remote-sensing and in situ observations with neural networks" by Etienne Pauthenet et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-25-RC1, 2022

Review of "Four-dimensional temperature, salinity and mixed layer depth in the Gulf Stream, reconstructed from remote sensing and in situ observations with neural networks" by Pauthenet et al. submitted to egusphere

Summary:

This paper presents an approach for predicting the vertical profiles of temperature and salinity over the top 1000 m from satellite surface observations by training an empirical machine learning model using in-situ profiles in the western North Atlantic Ocean. The paper emphasizes the treatment of the mixed layer depth and, specifically, a procedure to remove negative stratification from the profiles.

Overall, I found the paper to be an interesting contribution with sufficient novelty to be valuable. The ultimate impact of the work remains to be seen, but I think the paper will be worthy of publication after revision.

My major concerns are as follows:

- I find it surprising and confusing that the paper does not carefully separate capability to model the 4-D climatological annual-cycle from capability to model 4-D anomalies from

this climatological annual cycle. Perhaps such an approach is superior, and the methods are fine as they are, but the evaluation should clearly separate errors in the climatology from errors in the anomalies therefrom. I think the paper would be stronger if it included more explicit and quantitative evaluation of model performance on anomalies from the climatology (Nonetheless, I like the illustrative examples).

- Relatedly, given that the method predicts the climatological annual cycle, I think the paper would be stronger if results were compared to a climatology obtained by objective mapping or optimal interpolation, e.g. updated Roemmich and Gilson 2009 gridded Argo climatology or the mean of the CORA gridded product.
- In the training, it seems that the selection of cross-validation data does not account for spatial and temporal autocorrelation. It is not clear that the testing data are independent of the training data. Perhaps this is ok, given that you're trying to predict or map the climatology. But, the paper would be stronger if more explicit effort was made to train and test on truly independent data (at least with regard to modelling the anomalies).
- Confidence intervals or uncertainty. I'm a bit confused about how these are calculated and thus how to interpret them. The paper would be stronger if this was clearer.
- The main quantitative metric used is root-mean-square-error in physical units. I appreciate that this is physically intuitive, but this may obfuscate the generic statistical properties of the predictions. The paper would be stronger if normalized error metric were included, e.g. some sort of relative error and correlation.
- The word "coherence" is used a lot to refer to a desirable property of the 4-D gridded fields. Is this related to the frequency/waveform of the signal?? I'm not sure I understand exactly what is meant by coherence and why it is a valuable property of the predicted field. For example, in some cases, it may be that "smoothness" is unrealistic, e.g. in MLD predictions from GLORYS. Is coherence related to smoothness?
- Be more specific about what properties of a gridded T/S dataset make it useful for interpreting local oceanographic measurements or for process studies. I'm not sure what you mean? Low error? Correlation with real variability
- There are several areas where minor typographical and grammar issues need to be corrected.