Estimating ocean currents from the joint reconstruction of absolute dynamic topography and sea surface temperature through deep learning algorithms

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This work present an Observing System Simulation Experiment to improve the spatial resolution of Absolute Dynamic Topography, as a primary objective and Sea Surface Temperature as a secondary objective, based in deep learning methodologies. As a first step, the authors used model outputs to synthesize satellite observations and train a Convolutional Neural Network model, that is later applied using real satellite observations to retrieved 12 years (2008-2019) of higher spatial resolution ADT and geostrophic currents. The study is focused in the Mediterranean Sea, where without any doubt this approach can be challenging and have big impact since the Rossby deformation radius is small (10 km). The manuscript represent a substantial contribution to improve spatial resolution of surface ocean currents retrieved from satellite observations. I think the manuscript can be slightly improved before it can be accepted; therefore, I recommend minor revisions. I detail my major concerns bellow.

Major comments.

- I found that the manuscript describes in detail the deep learning methodologies used, the improvements made with respect to previous works, and the validation of the Convolutional Neural Network model. However, I think section 3.2 where the trained neural network is used to predict super resolved ADT from satellite altimetry and SST is unbalanced. I found it way too short, and in my opinion, it is one of the substantial contributions of this work. The authors have reconstruct super resolved ADT and derived geostrophic currents for the period 2008-2019. They validate the resulting current fields with in situ currents measured by drifters, providing Root Mean Square error as a metric. The RMS provides information about the accuracy, i.e, it provides an estimation of how well the model is able to predict the target value. It is indeed a good metric, however I would suggest the authors to consider other metrics that can assess the dynamical quality of the retrieved fields. Is this approach valid anytime of the year, or on the contrary it has similar limitations as the ones they stated in the introduction reported by previous works (González-Haro and Isern-Fontanet, 2014; Rio and Santoleri, 2018; Ciani et al., 2020). I am aware extending way far the validation in section 3.2 can be even out of the scope of the manuscript, but I think this deserve at least further attention and discussion.

- The proposed CNN approach enhances the characterization of mesoscale dynamics of current altimetry observations, it is undeniable with the spectral analysis shown in Fig. 4 and 5. However, I find misleading the following affirmations, although they are right:
  - *l 233 : Progressively approaching smaller scales, i.e. from ≃100 km downward (1 deg−1 wavenumber onward), the Super Resolved ADT spectrum (SR-ADT, red line in Fig. 4 (c)) evolves in fair good agreement with the ground-truth (green line in Fig. 4 (c)), confirming an improved representation of smaller mesoscale features compared to standard altimetry products.*
  - *L 236 The SR-ADT spectrum eventually shows the injection of noise below scales of ≃ 20 km, as confirmed by a flattening of the spectrum.*

  Although I do agree with the former affirmations, I think the authors should be more clear and state that the effective spatial resolution of the super resolved ADT is about 50 km (2 deg^{-1}). This is wavelength in which the PSD deviates from the ground truth (green curve). It is to say, from 100 km to 20 km the PSD of the super resolved ADT is closer to the ground-truth, when compared to the satellite PSD, but it has already lost energy. I would also suggest the authors to include the theoretical spectral slope curve k^{-5/3} in Fig 4c and Fig5c, for completeness and to facilitate interpreting the PSD curves.

- As briefly introduce in a point earlier, I would suggest the authors to further discuss about the fact that even the effective spatial resolution is improved, the description of dynamical features at the surface may be not guaranteed (l385)

Minor comments:

- I would suggest the authors to rephrase the abstract and state that the primary objective is to improve the spatial resolution of ADT.
- Line 52 Consider also other references here: González-Haro et al 2020, Miracca-Lage et al. 2022
- Line 56 I think it could be convenient to state here that the avarage revisit time of SWOT is about 11 days. It provides higher spatial resolution but the temporal one is much limitated.
- Line 194: *In particular, we forced the validation dataset to be a time series of samples adjacent in time (during the late fall/early winter season), instead of applying a random selection from the available samples.* Justify here why, it is stated further in the text line 357: *In other words, the CNN is pushed to predict ocean circulation features in periods of enhanced small mesoscale/submesoscale activity (e.g. Callies et al. (2015)) never seen during training.*
- Line *197 four predictors: namely the SE-ADT, SE-ADT error, SST and its temporal derivatives (∂tSST)* shouldn't "SST" here also be SE-SST?
- Figure 7: suggestion: could you mark in different colors the dates corresponding to cases shown in Fig. 4, 5 and 8?
- In general, and because there are a number of datasets it is difficult to follow the resulting spatial resolution of retrieved fields. I am assuming all of them are giving at the same spatial resolution than the model: 1/24 degrees. It can be deduced from the Power spectral analysis. I am also assuming that the retrieved fields from satellite observations in section 3.2 is 1/24, please state it clearer in the text, even in the abstract.