Reply to Reviewer 1

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

By Daniele Ciani , Claudia Fanelli , and Bruno Buongiorno Nardelli

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.

We thank the Reviewer for the constructive comments provided. Please find below our replies to the specific points.

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.

We thank Reviewer 1 for providing this comment on the manuscript. In order to balance the results between the numerical experiment and the analyses based on satellite data, we added spectral analyses to inter-compare the power spectral density (PSD) of the surface kinetic energies (KE) derived from standard Altimeter (up-sized to the 1/24° grid) and super-resolved ADTs, to have insights on the effective spatial resolution of the two datasets. This analysis was performed using a fast Fourier transform (FFT) over the time range 2008-2019 in two land-free areas of the Mediterranean Basin: i) one area across the Central/Aegean Basin (33.7°N-34.5°N--16.5°%-27.7°E) ; ii) one area across the Algerian Basin/Sardinian Channel (37.4°N-38.3°N--4°E-11.4°E), both depicted in Figure 1.R1-c of the present document. Both regions are known as dynamically active areas in the Mediterranean Basin (Pujol and Larnicol, 2005). In particular, the KE spectra were inter-compared against the theoretical predictions of two-dimensional turbulence (Vallis, 2006)), i.e., the k^{-3} and $k^{-5/3}$ slopes. Especially in the Central/Aegean area (Figure 1.R1-a), the spectral analysis confirms the improvement brought by the CNN reconstruction.

The SR and Altimeters KE spectra are super-imposed for small wavenumbers, indicating a similar description of the large mesoscale motions and are aligned with the predictions of energy/enstrophy transfer, following the k^{-3} slope for larger wavenumbers and the $k^{-5/3}$ slope for smaller ones. The improvement of our methodology with respect to standard altimetry processing is evidenced by overall higher PSDs at larger wavenumbers and by a closer alignment with the k^{-3} slope for wavenumbers ≥4 degrees^{-1}, i.e. scales ≤30 km (although not fully recovered through the entire range). This reflects a more efficient representation of mesoscale features associated with our reconstruction. For the Algerian/Sardinian area, the analyses are sketched in the Figure 1.R1-b below and led to similar conclusions as for the Central/Aegean area, although both KE estimates (Altimeter and SR) show less agreement with the $k^{-5/3}$ at wavenumbers ≤3 degrees^{-1}.

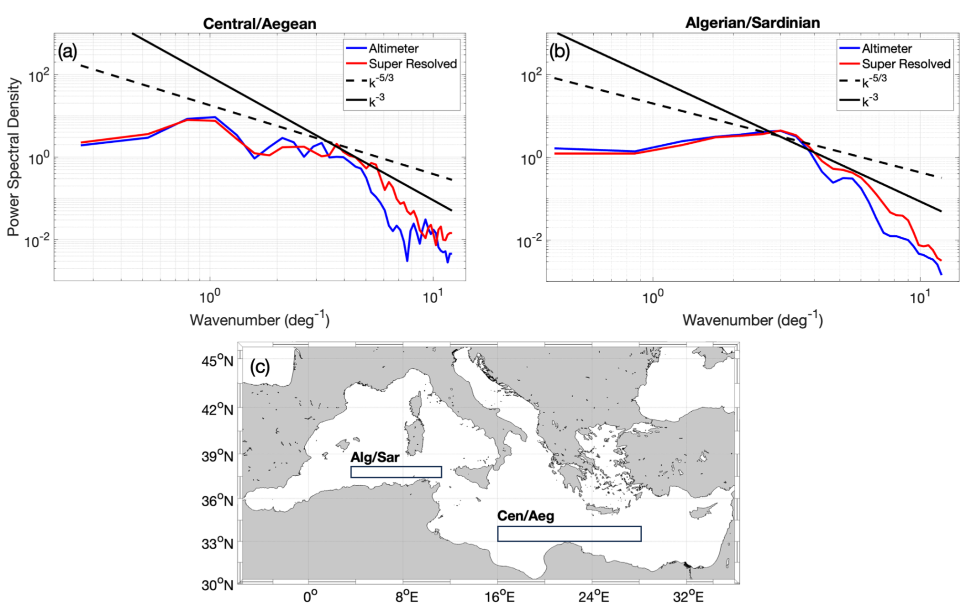

Figure 1.R1: Comparative spectral analyses of the KE maps derived from standard Altimeter (blue) and SR (red) ADTs. Panels (a) and (b) respectively refer to the Central/Aegean and Algerian/Sardinian areas, depicted in panel (c).

Such results are now introduced in the revised version of the manuscript . Additionally, in the discussion section, we introduced a hint for future validation metrics to quantify feature resolution in the reconstructed L4 Fields (lines 373-391, lines 440-443 of the revised manuscript).

Moreover, we tried to assess the performances of the CNN-based ADT (and derived surface currents) reconstruction as a function of the season. The winter (December to February) and summer (June to August) validation of the super-resolved geostrophic currents with respect to in-situ measurements are reported in Figure 2.R1. In both seasons we can recognize an overall improvement of the Super-Resolved (SR) surface currents with respect to standard Altimetry. In winter (Figure 3.R1-a) the RMS error reduction of the SR currents is around 1 cm/s for both components of the surface flow; in Summer (Figure 2.R1-b) we have comparable performances for the zonal flow and a 0.5 cm/s RMS error reduction of the SR currents, for the meridional flow. It is however really challenging to assess the seasonal behaviour of such statistics, as the numbers of in-situ measurements is not comparable across seasons (see e.g. Fig 2.R1-c,d). In the manuscript, we would thus just keep the overall statistics in order to rely on a larger number of satellite/in-situ matchups throughout the basin.

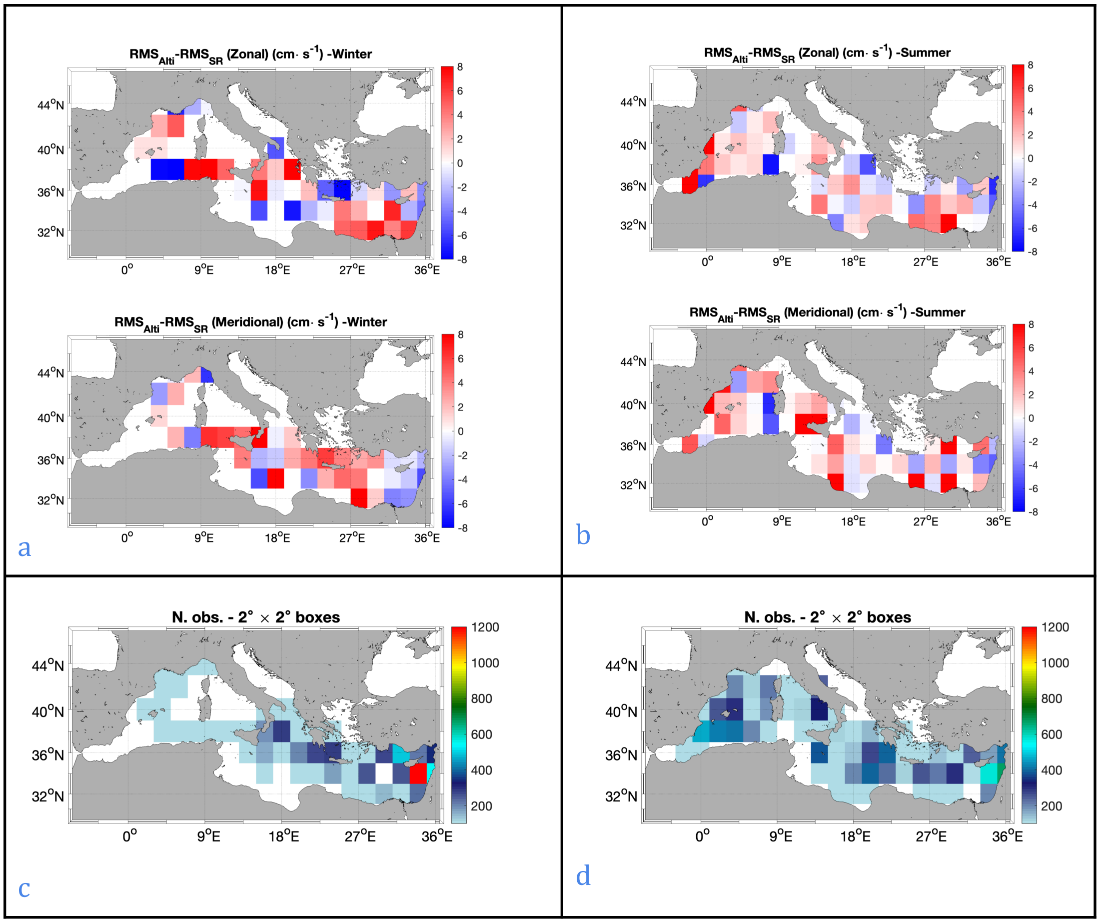

Figure 2.R1 **a)** Differences of RMS errors between the Altimeter derived (Alti) and SR currents during WINTER: (top) zonal flow, (bottom) meridional flow. Red areas express an improvement with respect to standard altimetry. **b)** Differences of RMS errors between the Altimeter derived (Alti) and SR currents during SUMMER: (top) zonal flow, (bottom) meridional flow. Red areas express an improvement with respect to standard altimetry. **c), d)** number of in-situ measurements during winter and summer, respectively. Statistics are provided in 2°x2° boxes.

- 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 ≃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 ≃100 km downward (1 deg–1 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.

  Thanks for providing these comments. In order to evaluate the relative behaviour of our outputs with respect to the $k^{-5/3}$ spectral slope, we inter-compared the Kinetic Energy (KE) spectra of the dataset involved in our study (as in Ciani et al. 2019 and following Vallis 2017). The KE are computed using the surface currents derived from the Satellite equivalent (SE) , Super Resolved (SR) ADTs and from the model outputs (GROUND TRUTH).

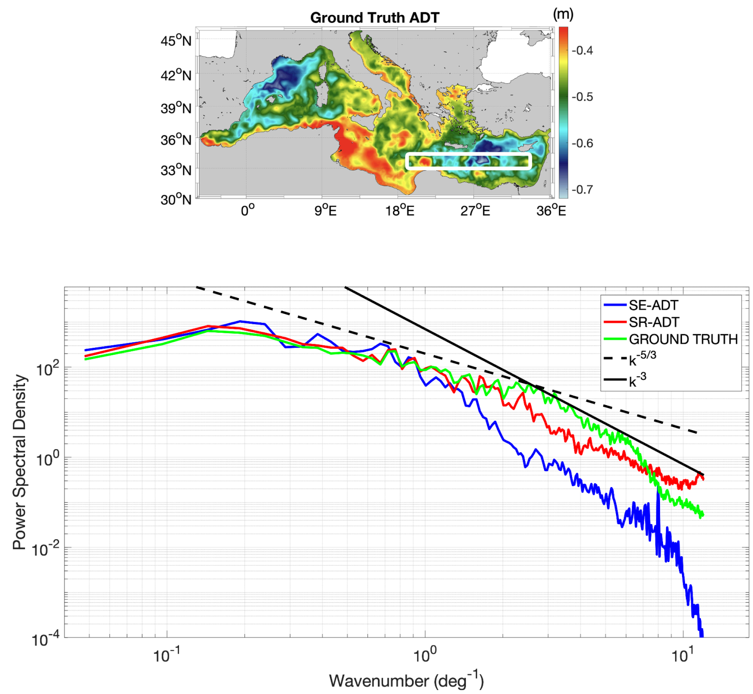

Figure 3.R1. Comparative spectral analysis of the Kinetic Energy maps. Results refer to the 2D box depicted in the top panel . The black continuum and dashed lines represent the predictions of the 2D Energy transfer (k^-5/3) and 2D enstrophy cascade (k^-3), respectively.

As expected, the modelled surface currents (our reference) follow the prediction of the 2D turbulence energy/enstrophy cascade, in agreement with Vallis 2017. In particular, for smaller wavenumbers, the KE spectrum (green line) is closer to the $k^{-5/3}$ slope, while it mostly follows the k^{-3} for larger ones. An expected exception occurs for wavenumbers approaching 10 deg^{-1}, where the spectrum exhibits an energy loss, suggesting that model outputs are unable to fully resolve submesoscale motion. Interestingly, the KE spectrum obtained from SR-ADT currents (in red) is pretty much in line with the evolution of our reference case. A slight energy reduction is observed in the mesoscale range, but the improvement with respect to standard synthetic altimetry is evident. We thus decided to add this figure and comment it in the revised manuscript, also adding information on the fully resolved scales (Please see lines 259-271 of the revised manuscript).

- 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)

We thank the Reviewer for this comment. We agree on the fact that an improved effective spatial resolution (confirmed by the spectral analyses for both the OSSE and the application to satellite-derived data) does not guarantee the full description of small mesoscale features at the ocean surface with the dADR-SR methodology. For instance, one can see that the KE spectra obtained from the Super-Resolved ADT depicted by figures 1.R1 and 3.R1 of the present document do not fully recover the theoretical energy KE spectrum at all scales. The use of an optimally interpolated SST is certainly a major limit for our reconstruction methodology. OI SSTs are indeed known to smooth/distort some oceanic features as a drawback of the interpolation algorithm. In addition, under prolonged cloud cover, (although we are here feeding the network with this information) the extraction of features becomes questionable, as the SST fields tend to reproduce the smoother background field. This could be overcome in future studies relying on a training based on L3C SSTs (as done for single-image super-resolution by Fanelli et al. 2024), which has been already identified as one future development of the present study (See lines 425-432 of the revised manuscript). Following the Reviewer's suggestion, we inserted an additional comment on this topic at lines 440-443 of the discussion section, also introducing one comment on feature resolution evaluation with additional metrics.

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.

  This has been corrected in the manuscript, please see lines 1-3 of the revised version.

- Line 52 Consider also other references here: González-Haro et al 2020, Miracca-Lage et al. 2022

  The references have been added, as suggested. Please double check line 56 of the revised manuscript.

- 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.

  Thanks for this comment, which has now been integrated in the revised manuscript. Please check lines 63-64 of the revised manuscript.

- 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.

  Thanks for pointing this out. We introduced a brief comment on this at lines 201-204 of the revised manuscript, enabling the readers to have an initial idea of the reason behind our choice and referring to Section 4 for more details.

- 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?

  Thanks for this comment. It actually made us realize it was worth further specifying this point. At line 197 of the original manuscript, the SST is indicated as is because it refers to the former CNN architecture by Buongiorno Nardelli et al. 2022, in which the SST was simply extracted from the model outputs and did not account for the satellite processing effects, it was thus assumed as perfectly known. The SST is thus a SE-SST only in the present study. In order to further clarify this, we modified the sentence from
  "In previous formulations, the network considered" to
  "In previous formulations (Buongiorno Nardelli et al. 2022)", the network considered
  Please, check line 206 of the revised manuscript.

- Figure 7: suggestion: could you mark in different colors the dates corresponding to cases shown in Fig. 4, 5 and 8?

  Thanks for this comment. The image (now Figure 8 in the revised manuscript) , as well as its caption, have been modified accordingly

- 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.

  We thank Reviewer 1 for this suggestion. A specific comment has been inserted in the abstract and along the manuscript, at lines 8,14-15, 110-111, 121-122, 150-152, 168-170, 179-180 respectively.

References

Buongiorno Nardelli, B., Tronconi, C., Pisano, A., and Santoleri, R.: High and Ultra-High resolution processing of satellite Sea Surface

Temperature data over Southern European Seas in the framework of MyOcean project, Remote Sensing of Environment, 129, 1–16, 2013.

Buongiorno Nardelli, B., Cavaliere, D., Charles, E., and Ciani, D.: Super-resolving ocean dynamics from space with computer vision algorithms, Remote Sensing, 14, 1159, 2022.

Ciani, D., Rio, M. H., Menna, M., & Santoleri, R. (2019). A synergetic approach for the space-based sea surface currents retrieval in the Mediterranean Sea. Remote Sensing, 11(11), 1285.

Fanelli, C., Ciani, D., Pisano, A., & Buongiorno Nardelli, B. (2024). Deep learning for the super resolution of Mediterranean sea surface temperature fields. Ocean Science, 20(4), 1035-1050.

Pujol, M. I., & Larnicol, G. (2005). Mediterranean sea eddy kinetic energy variability from 11 years of altimetric data. Journal of Marine Systems, 58(3-4), 121-142.

Vallis, G. K. (2017). Atmospheric and oceanic fluid dynamics. Cambridge University Press.