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Title: A global 3D chlorophyll-a dataset derived from multimodal deep learning reconstruction

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General Comments

This manuscript presents a global, monthly, three-dimensional chlorophyll-*a* (Chl-*a*) dataset for 2005–2025, reconstructed using a multimodal Profile-Surface Transformer (PST) framework. The framework combines BGC-Argo Chl-*a* profiles, Core-Argo temperature-salinity profiles, and MODIS satellite-derived surface Chl-*a* to estimate the vertical distribution of Chl-*a* from 10 to 500 m. The topic is timely and relevant to ESSD, as most existing global Chl-*a* products are largely limited to surface observations, whereas subsurface Chl-*a* structures are important for understanding phytoplankton biomass, primary production, carbon export, and ecosystem responses to climate change. The manuscript is generally clearly written, and the proposed dataset has the potential to become a useful community resource. However, I have several substantive concerns that should be addressed before publication. In particular, the uncertainty characterization of the final product remains limited, several key preprocessing and model-design choices are insufficiently documented, and some aspects of the reconstructed dataset require clearer evaluation and interpretation. I therefore recommend minor revision.

Specific Comments

The uncertainty assessment of the final data product is currently insufficient. I checked the dataset provided through the repository link, and the final NetCDF files do not appear to include uncertainty-related variables. This limits users' ability to assess the reliability of the product across regions and time periods. Section 4.3 is entitled "Evaluation of the interpolated product and uncertainty analysis", yet the analyses presented mainly consist of comparisons against ship-based observations. This represents external validation of the gridded product rather than a comprehensive uncertainty assessment. In addition, only R and R^2 are reported in this section.

For consistency with Section 4.1, it would be preferable to additionally report RMSE, MAE, and bias.

The authors mention in lines 359-363 that interpolation diagnostics were recorded, including the number of profiles used for interpolation, the proportion of missing values within the ocean domain, and the frequency of use of different interpolation radii, yet none of these diagnostics are actually presented anywhere in the manuscript. The sources of uncertainty in the final product, including model prediction errors, errors introduced by the spatial interpolation procedure, and the propagation of input data uncertainties, should be systematically evaluated and provided as ancillary variables within the NetCDF product.

The manuscript states that profiles passing both real-time and delayed-mode QC procedures were retained (line 166-167). It is unclear whether this means that only profiles that have undergone both RTQC and DMQC were used, or whether the dataset also includes RTQC-only profiles. If RTQC-only profiles were included, please report their approximate proportion and discuss whether differences between RTQC and DMQC data quality could affect the reconstruction. In addition, the manuscript does not clearly describe how fluorescence-specific corrections were handled prior to model training. This is important because different correction strategies can substantially affect Chl-*a* magnitudes, particularly in the upper ocean.

In lines 264-265, the manuscript mentions a "density-anomaly-related feature computed from temperature and salinity using a simplified equation-of-state formulation". Please provide the explicit formulation used for this calculation.

The manuscript provides a detailed description of the PST model architecture. However, several design choices remain insufficiently explained. At line 290, the authors introduce an exponentially decaying depth-dependent gate to modulate the influence of satellite surface observations with depth. Please provide the functional form of this gate, clarify whether the decay rate is learned or prescribed, and assess the sensitivity of the reconstruction to this choice.

The model also uses a composite loss function that combines a depth-weighted masked mean squared error with a curvature-consistency term based on second-order vertical differences (lines 294-295). The exact formulation is not given. Please provide the full

mathematical expression, including the relative weights of the two terms and the depth-weighting scheme.

Regarding the deep-background correction applied to ship-based fluorescence profiles (lines 498-501), the authors state that a background value estimated below 300 m was subtracted from the original fluorescence-derived Chl-*a* values. Please specify how this background value was estimated. The external validation is based on 135 cruises, and Figure 4a shows their spatial distribution. Please also provide the temporal distribution of these cruises and indicate whether any years during 2005-2025 lack external validation data.

The manuscript defines SCM depth as the depth of maximum Chl-*a* concentration within the 10–200 m range (line 639). However, as the authors themselves discuss earlier in the text and as clearly illustrated in Figure 5, Chl-*a* maxima in high-latitude regions with deep winter mixing often occur near the surface. In such cases, the vertical Chl-*a* profile may not exhibit a distinct subsurface peak, and the detected "maximum" simply reflects the surface-intensified distribution rather than a true subsurface chlorophyll maximum. Were these surface-dominated profiles excluded from the SCM analysis, or were they included in the statistics? If they were included, the reported SCM depths may conflate true subsurface maxima with surface-intensified profiles driven by different physical–biological mechanisms.

The model appears to operate in log-transformed Chl-*a* space (as suggested by the log-scale axes in Figure 3), but this is never explicitly stated. This should be clarified in the text. Additionally, the Chl-*a* variable in the downloadable NetCDF product lacks a units attribute, which should be added.

Technical Corrections

1. Line 25: "integratessurface" should be corrected to "integrates surface"

2. Line 76: "cycling. (Marañón et al., 2021)." should be corrected to "cycling (Marañón et al., 2021)."

3. Table 1: The description of the spatial cross-validation procedure should be clearer. Please specify what "K" represents and how the spatial blocks were defined.

4. Throughout the manuscript, the usage of “temperature-salinity” is inconsistent, with both en-dashes and hyphens appearing. Please standardize the notation.