RC2: 'Comment on essd-2025-14', Anonymous Referee #2, 21 Apr 2025

This is a well written manuscript and the analysis seems legitimate and thoughtful. The first thing that came to me after reading the manuscript is why the new dataset of ocean surface stress is needed? Since there are many assumptions have been made in order to calculate the ocean-surface stress. It will be great if the authors can explain the motivation and reasoning that this data is needed for the community.

Thank you for the thoughtful feedback and for recognizing the quality of the analysis. We agree that a clearer articulation of the motivation behind generating this dataset is important, and we have now made this more explicit in the revised manuscript.

Surface stress is a fundamental driver of polar ocean circulation, as it governs the transfer of momentum from the atmosphere to the ocean—either directly or through the mediating influence of sea ice. In the Arctic, this stress regulates key processes including ice drift, Ekman transport, and the spin-up of gyre systems. Recent studies have shown that thinning and retreating sea ice can lead to enhanced wind–ocean coupling, with significant implications for ocean kinetic energy, vertical mixing, and climate feedbacks (e.g., Martin et al., 2013; Martin et al., 2016; Muilwijk et al., 2024). Despite its importance, surface stress remains poorly constrained in both observations and models, especially in the ice-covered ocean where coupled feedbacks are complex and highly variable.

To address this gap, our dataset offers an observationally anchored, spatially continuous estimate of surface stress across the Arctic and Antarctic, separating ice-covered and open-ocean contributions using consistent and transparent assumptions. While simplifications (e.g., fixed drag coefficients, Gaussian filters) are necessary, they are based on recent literature and can be adapted as new data or parameterizations become available.

This product is intended not as a replacement for high-resolution coupled models, but as a complementary tool for:

Forcing ocean models where stress observations are sparse or missing;

Benchmarking atmosphere–ice–ocean coupling in simulations;

Quantifying trends in stress-driven processes such as gyre intensification or ice divergence.

We have added dedicated paragraphs in the Introduction to better articulate these motivations (line 41-62).

Second, the value of Ekman depth used in the calculation is not explained, eqn. 4 & 5. The Ekman depth is needed in order to calculate the vertical Ekman velocity. What are the values of Ekman depth that you used?

Thanks for this observation. The Ekman depth used in our calculations is set to 20 m following Meneghello et al. (2018), and this is stated at line 112. We chose this fixed value based on typical

estimates from prior observational and modeling studies in polar regions (e.g., Zhong et al., 2018; Ramadhan et al., 2022).

We fully acknowledge that the Ekman depth can vary significantly depending on stratification, ice cover, and wind forcing. This introduces an important source of uncertainty in the derived vertical velocity, which we plan to explore in future work.

Third, for the scatterplots of ITP vs satellite (Fig. 12 & 13), how the ITP data is processed to be comparable to the daily satellite product? Did you do time average on ITP data to daily data or interpolate the satellite daily product to the ITP profile time? From the number of samples, it seems the author interpolated the daily satellite data to the ITP data (profile) time. If so, the scatter plots are meaningless. The comparison is wrong, since the satellite temporal resolution is under sampled. You can not draw conclusions from these scatterplots. Also, the 30-day low pass resulted in too few data points (Fig. 14, 15 & Table 3). I suggest to do a time-average ITP data to daily or weekly and , and redo the analysis.

Thank you for highlighting this concern. We agree that differences in temporal sampling between satellite-derived and in situ data must be carefully considered. In our analysis, we interpolated the daily satellite product to the timestamp of the ITP profiles to maximize the number of data pairs, which is a common practice when comparing gridded satellite fields with in situ observations (e.g., Guinehut et al., 2012).

However, we acknowledge that this approach may introduce representativeness errors if the satellite data does not resolve variability at the timescale of the in situ profiles. To address this concern, we have repeated the analysis by averaging the ITP profiles to daily and weekly means, and then comparing these averages to the corresponding satellite product without interpolation (see revised section 4.2, line 403-562). The revised scatterplots and correlation statistics are now shown in updated Figs. 12–15 and Table 3. We find that the key conclusions remain consistent, though the number of independent points is reduced.

Other small comments

Line 40: "ice-ocean governor," → "ice-ocean governor",

It is revised accordingly (line 39).

Line 60: Figure 1(a) caption: there is no dashed magenta has shown. Is the September ice extent boundaries out of the scope of the plot? The Solid line near the Novaya Zemlya has a portion of dashed line. Is this supposed to be solid?

Thanks for pointing out. In the revised Figure 1 we added the missing contours and noted the marginal seas.

Line 84 It is unclear to me how the De: Ekman layer depth is determined in the calculation. Can you

explain?

In the revision, we now explicitly define the Ekman layer depth as 20 m at line 112, following previous studies such as Meneghello et al. (2018).

Acronym stands for?
Line 98 acronym OAFLux2?
Line 106 acronym SSM/I, AMSR-E, AVHRR, MODIS, QuikSCAT?
Line 107 acronym IABP?
Line 111 acronym CLS/PML?
Line 120 acronym SMMR DMSP, NSIDC?

In response to these comments, a table of acronyms has been added as an appendix to the manuscript to address above comments (see line 603).

Line 139 "Noting the 25 km resolution could introduce uncertainties near the 15% sea ice concentration boundary." --> How did you arrive this conclusion?

We thank the reviewer for this question. This statement is based on the fact that a 25 km resolution grid cell can encompass both ice-covered and open water areas near the 15% sea ice concentration threshold, which is commonly used in satellite products. At this resolution, subgrid-scale variability becomes significant, and the true ice edge may not be well resolved—potentially affecting ice classification and derived variables such as surface stress. This issue has been previously noted in the literature (e.g., Meier, 2005; Ivanova et al., 2015) and is particularly relevant in the marginal ice zone where gradients in sea ice concentration are sharp.

In the revised text "Noting that the 25 km resolution may introduce uncertainties near the 15% sea ice concentration boundary, as such coarse resolution can obscure sharp gradients in the marginal ice zone and misclassify mixed ice–water grid cells (e.g., Meier, 2005; Ivanova et al., 2015)" (line 162-164).

Line 152 "where W represents the local vertical Ekman velocity W_e" → W and W_e were used interchangeably. Can you decide which one to use and keep it consistently?

Thank you for pointing this out. We have revised the manuscript to use W_e at line 194-195, to avoid any confusion.

Line 227 "in the Indian Ocean sector and the southeast Pacific" → Can you label it on Fig 6b?

Thank you for the suggestion. We have revised the text at Line 270 to specify the approximate longitude ranges instead of using regional names. This improves clarity and avoids ambiguity.

Line 232 of 50°S (Figure 6d), → Can you label the latitude and longitude on Fig 6?
Line 293 Baffin Bay, the Chukchi Sea, and north of Fram Strait → Can you label these places in

Figure 8?.

Lien 324: the Mendeleev Ridge. → please label it on the Figures

Line 327-328 Weddell and Ross Seas, Antarctic Peninsula and western Ross Sea, Enderby Land and the Amundsen Sea → please label these places on the Figures.

In response to your above comments, the geographic maps in Figure 1 have been revised to include the names of key geographic locations and marginal seas to improve clarity and orientation (line 80). We note that adding extensive labels or coordinate grids to the main result figures may hinder readability, so detailed labeling is limited to overview maps for reference.

Line 309 What do you mean " full eight-year/six-year period" period?

The phrase "full eight-year/six-year period" refers to the different temporal coverage used for each region, and has been revised as "an eight-year period (2011–2018) for the Arctic, and a six-year period (2013–2018) for the Antarctic" (Line 351-352).

Line 381 What do you mean "paired" observations?

By 'paired' observations, we refer to data points that are co-located in both space and time. This has been clarified in the revised text as: 'comparison of satellite-derived surface velocity components against collocated ITP-V observations' (line 455).

Line 385 "ITP-80 pair shows better agreement in the zonal component after 200 days." → I can't tell if this is true from the figure 12. How did you arrive this conclusion?

Thank you for pointing this out. We agree that the statement was ambiguous and not clearly supported by Figure 12. To avoid potential confusion, this sentence has been removed in the revised version.

References:

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