Review of “Global Thermocline Vertical Velocities: A Novel Observation Based Estimate” by Diego Cortés-Morales, Alban Lazar, Diana Ruiz Pino, and Juliette Mignot.

In this manuscript, Cortés-Morales et al. develop and validate a new vertical velocity estimate for the ocean. The manuscript is generally well-written, and the topic is timely. I believe that providing a vertical velocity dataset is extremely important for the oceanographic community. I have a few comments that, while not dramatically changing the manuscript and its results, will require some revisions from the authors. I hope these will help clarify key aspects of the manuscript.

For these reasons, I recommend a major revision of the manuscript.

Please see my specific comments below.

We thank the reviewer for the revision and for highlighting all these key points. In response, we have made substantial revisions to the manuscript to clarify the relationship between Ekman pumping and the geostrophic vertical velocities at the ocean surface and interior, and the metrics used for the validation of the product. In addition to addressing the specific issues in their locations in the text, some changes have been applied to the abstract and the conclusions section to reflect them.

We also realised that the manuscript repeatedly referred to our previous study (Cortés-Morales and Lazar, 2024). To lighten the text, we now refer to this wors as CM24 throughout the manuscript.

Major Comments:

1) Ekman Boundary Condition

Ekman dynamics are not geostrophic in the traditional sense, as they involve other processes (i.e., wind stress, vertical momentum transfer, and viscosity) and do not include the pressure gradient. Thus, it is confusing to refer to a vertical velocity that incorporates such dynamics as "geostrophic". In my opinion, this is more complex. A purely geostrophic vertical velocity would be represented by (2), with wg(zref) determined solely by geostrophic processes (perhaps using $\partial_t\eta$, where η is the sea surface height). I would understand that you do no want to alter your framework, which is reasonable; however, you need to clarify this point and use more accurate terminology.

In section 2.1, if one derives the full equation, it becomes evident that this condition must be imposed by "patching" the Ekman layer and the interior ocean. Therefore, the condition exists not at the ocean surface but at the base of the Ekman layer.

- Thank you very much for your comment. We have revised Section 2.1 and added an Appendix A to include a more detailed discussion of the relationship between geostrophic velocities and Ekman pumping, supported by literature review. This revision clarifies the implications of assuming the Ekman pumping at the base of the Ekman layer and why in our formulation the Ekman pumping is defined at the ocean surface.

The sentence on lines 106-107 contradicts what is discussed in section 3.5. If the Ekman condition governs wg, how can wg at the boundary differ from Ekman? Either there is an issue or something is unclear in your method.

In section 3.5, you begin by stating, "one might wonder how near-surface wg compares with Ekman pumping (wEk)", yet your methodological description suggests they are the same (i.e., surface boundary conditions). You are comparing at different depths. This entire paragraph, discussion, and section are confusing; I do not understand the goal or conclusion. More importantly, how is wEk defined at different depths? Should it not be defined at the base of the Ekman layer? Are you setting it to random depths for comparison? This does not make sense.

- Thank you for pointing out this potential inconsistency. We clarify four points:
    - First, at the surface boundary, we impose the condition $w_{\mathrm{tot}}(z=0)=0$. It is exact for the stationary flow of the time-mean state we first show in the paper, but not for the time-varying flow of the annual mean flow with interannual variability. It is therefore an approximation to neglect d(η)/dt (η=sea surface height relative to the mean state reference surface level), and the very good results we obtain (correlation in Fig. 4b) validates a posteriori the assumption. Then we assume that the ageostrophic flow is at first order only produced by surface wind stress and friction (Ekman three-dimensional flow) and note $w_{\mathrm{tot}} = w_g + w_{ag}$. Hence, the above surface condition implies:, $w_g(z=0) = -w_{ek}(z=0)$.
    - Second, this boundary condition does not imply that the geostrophic vertical velocity is identical to the Ekman pumping throughout the upper ocean. Applied to the Ekman layer, the assumption in the ocean of incompressibility of the total flow implies:

      $div(\boldsymbol{u_g}) = -div(\boldsymbol{u}_{Ek})$

      where $\boldsymbol{u_g}$ and $\boldsymbol{u_{Ek}}$ are the geostrophic and Ekman components of the threedimensional velocity field.

      Integrated in z, it gives:

      $$\int_{D_{Ek}}^{0} -\frac{\beta v_g}{f} + \frac{\partial w}{\partial z} dz' = \int_{D_{Ek}}^{0} -\nabla \cdot \boldsymbol{u_{Ek}}\, dz'$$

$$\int_{D_{Ek}}^{0} -\frac{\beta v_g}{f} dz' + w_g(0) - w_g(D_{Ek}) = \int_{D_{Ek}}^{0} -\nabla \cdot \boldsymbol{u_{h,Ek}}\, dz' - w_{Ek}(0) + w_{Ek}(D_{Ek})$$

In this equation, $w_{Ek}(D_{Ek}) = 0$ (because at D_{Ek} each component of the 3D Ekman flow, is dissipated/vanished), the surface condition eliminated $w_g(0)$ and $w_{Ek}(0)$, and Ekman theory gives
$\nabla \cdot \boldsymbol{U_{h,Ek}} = \nabla \times \left(\frac{\tau}{\rho_0 f}\right)$ for $\nabla \cdot \boldsymbol{U_{h,Ek}} = \int_{D_{Ek}}^{0} \nabla \cdot \boldsymbol{u_{h,Ek}}\, dz'$
Hence we obtain:

$$w_g(z = D_{Ek}) = \nabla \times \left(\frac{\tau}{\rho_0 f}\right) - \int_{D_{Ek}}^{0} \frac{\beta v_g}{f} dz'.$$

The magnitude of the vertical flow at the base of the Ekman layer is modified from its surface value by the vertically integrated divergence of the horizontal geostrophic flow over the Ekman layer. These corrections of the theory were recently proposed by Jacox et al., 2018. We understand "near-surface" may be confusing. We have changed "near-surface" "ocean interior" in lines 495-496:"*...one might wonder how ocean interior wg compares with surface Ekman pumping (wEk).*"

- Third, regarding the definition and comparison of w_{ek} and w_g at different depths: in most of the literature (excepts for a few papers like Pedlosky, 1996 and Jacox et al., 2018), the Ekman pumping is often defined at the base of the Ekman layer , instead of the surface as demonstrated above. However, this the depth of that base varies regionally and among the different works. For clarity, due to the performance of OLIV3 in reproducing the temporal variability of the reference datasets, in Section 3.5 our intention is to compare the Ekman pumping signal ($w_{ek}(z = 0)$) with the geostrophic and total vertical velocity at selected upper-ocean levels in an OGCM simulation. This way, we propose an assessment of the error made when assuming that the wind stress derived Ekman pumping is an optimal proxy for the total vertical velocity variability within the upper ocean.
- Finally, to test whether Ekman pumping alone explains upper-ocean variability, we compare correlations of the total vertical velocity with (a) surface Ekman pumping and (b) the geostrophic component evaluated at the same interior depth as the total field. The higher correlations of the geostrophic component at the corresponding level at which we evaluate the total velocity, indicate that variability of the total vertical velocity is not solely due to surface Ekman pumping, but also to interannual (and longer) variability in meridional geostrophic transport.
- We have clarified these points in the text, section 2.1, appendix A and section 3.5. We hope that the review will better understand our arguments.

Lines 518-526: I find this paragraph confusing. Please rewrite it based on the comments above.

- Thank you for highlighting this point. We have rewritten the paragraph in lines 566-567 to clarify that our intention is to examine the relationship between the surface

Ekman pumping and the vertical flow within the ocean interior: *“The wind-driven divergence at the surface and the vertical flow in the ocean interior are strongly correlated across large portions of the global tropical and subtropical gyres, as supported by the comparison between OGCM estimation of the geostrophic vertical velocities (w_g) in the ocean interior and Ekman pumping (w_{Ek}).”*

2) “Overuse” of the Correlation

Even if you demonstrate a good correlation, this does not imply good representation; it only indicates good synchrony. A correlation can still be good whereas one component is significantly stronger than the other. To fully illustrate good representation, you also need to check that the variances are consistent. Hence, if you want to demonstrate good representation, it would be more effective to use the coefficient of determination (R2: https://en.wikipedia.org/wiki/Coefficient_of_determination). I suggest to use instead of correlation for the particular purpose of your study.

Furthermore, correlation (normalized projection) and the coefficient of determination (R2; relative similarity = 1− relative error) are two distinct diagnostics. They can converge, but they do not have to. Please be specific and avoid conflating the two.

More importantly, how do you compute a local correlation with a "time mean" term (as indicated in the figure title 3b)? If w=fct(x,y,σ,t), the correlation is computed between two scalars... Projecting one scalar onto another does not make sense. The text references interannual correlation, thought, so please clarify!

- Thank you for this detailed and constructive comment. Our aim in this section is to quantify how much the time variability of the total vertical velocity can be attributed to its geostrophic component. We agree that correlation coefficient alone measures only the temporal synchrony between two variables and does not itself guarantee similarity in amplitude or variance. For this reason, our analysis does not rely solely on the correlation coefficient, we also examine the time-mean relative error between the two fields (Figs. 3a and 5), which provides complementary information about discrepancies in magnitude. Together, these diagnostics allow us to assess both the temporal coherence and the amplitude consistency of the geostrophic and total vertical velocity fields. In Figure 3c, we focus on how the geostrophic component describes the vertical structure of the total vertical velocity.

  Regarding the choice of metric, we believe that the correlation coefficient remains appropriate in this case. Unlike the coefficient of determination, the correlation field allows as to identify regions where the geostrophic and total vertical velocities are anticorrelated, as in the case of western boundary currents. This highlights areas where the ageostrophic processes dominate the variability.

  Concerning the reviewer’s question about the “time-mean” in Figure 3 (now Figure 4): this was indeed a typographical error in the title of the subplots. The time-mean corresponds to panels (a) and (c), while panel (b) corresponds to interannual

(annual resolution) correlation between w_g and w_{tot}. We have corrected the figure title.

We acknowledge the possible overuse of the correlation coefficient, and we clarify that results of these figures are only to test synchronicity, while a good representation also implies the time mean comparison in lines 266-267: "*To evaluate the ability of the geostrophic component to represent the spatiotemporal variability of the total vertical flow, we examine three diagnostics:*" and lines 329-332: "*Most notably, the high and widespread synchrony at annual frequencies between wtot and wg suggests that the geostrophic component strongly dominates the interannual variability, and therefore likely in the real ocean as well. While nonlinear processes influence the mean amplitude of the vertical flow, they play a smaller role in modulating this variability.*"

3) Description of the dataset

I believe there was a missed opportunity to present the dataset effectively. Adding a single figure that illustrates vertical velocity at a range of key levels (surface, within the thermocline, and below the thermocline) would be beneficial. A single figure with six eight panels showing vertical velocity in color and the corresponding isopycnal depth in contour would serve as the main visual aid to describe the dataset.

This figure, along with a descriptive paragraph, should be included in section 2.2.

- Thank you for your comment. We have included a new Figure 1 as the reviewer suggested showing OLIV3 at different isopycnal levels across the thermocline.

4) Two datasets (?)

Two datasets are provided and attached to the manuscript (isopycnal level and vertical level). However, only the isopycnal dataset is presented, described, and analyzed. I suggest removing the vertical-level dataset for consistency, as it seems surprising, to say the least, to include a supplementary dataset that is neither referenced nor discussed in the manuscript. Alternatively, a full description and analysis of the second dataset are required.

- Both datasets originate from the same computation, integrating LVB over the native ARMOR3D vertical levels. To address your concern, we have removed the vertical-level dataset and kept only the isopycnal interpolated one. We have revised lines 172-176: *"The resulting Observation-based LInear Vorticity Vertical Velocities (OLIV3) product consists of geostrophic vertical velocities derived from ARMOR3D meridional velocities and surface Ekman pumping from ERA5 wind stress, using the vertically integrated geostrophic LVB (Eq. \ref{eq2}) over the native ARMOR3D vertical levels and then interpolated over isopycnal levels. The product spans the 1993 - 2019 period, with a horizontal resolution of 0.25° and 71 isopycnal levels.".*

Minor Comments:

*) In the introduction (lines 54-58), you should also mention a strategy for reconstructing the full 3D circulation (u,v,w) based on thermal wind balance and Argo float deep displacement knowledge for the reference-level horizontal velocity. The vertical velocity derives similarly from a Sverdrup balance (equivalent to your approach): reference Colin de Verdière and Ollitrault (2016) and Colin de Verdière et al. (2023).

Ref:

Colin de Verdière, A., and M. Ollitrault, 2016. A Direct Determination of the World Ocean Barotropic Circulation. Journal of Physical Oceanography, 46, 255-273.

Colin de Verdière, A., T. Meunier and M. Ollitrault, 2019, Meridional overturning and heat transport from Argo floats displacements and the planetary geostrophic method: applications to the subpolar North Atlantic, Journal of Geophycical Research, 124, 6270-6285.

- Thank you for suggesting these references. We have updated the introduction (lines 45-59) to include a discussion of this additional strategy for reconstructing the full 3D circulation using thermal wind balance combined with Argo float displacements to define the reference-level horizontal velocity. This includes the explicit citation of Colin de Verdière and Ollitrault (2016) and Colin de Verdière et al. (2019) : *"Traditional approaches for estimating vertical velocities across different ocean regions were derived from tracer fluxes (e.g. Stommel and Arons, 1959; Robinson and Stommel, 1959; Wyrtki et al., 1961; Munk, 1966; Wunsch, 1984) or the application of the continuity equation to horizontal current measurement obtained from hydrographic station data (e.g. Stommel and Schott, 1977; Schott and Stommel, 1978; Wyrtki, 1981; Roemmich, 1983) and mooring measurements (e.g. Halpern and Freitag, 1987; Halpern et al., 1989; Weingartner and Weisberg, 1991; Helber and Weisberg, 2001). These early methods provided insight into the small vertical velocities' order of magnitude and upwelling/downwelling patterns of the vertical motions. Vertical velocities have also been inferred from the divergence of horizontal velocity in numerical models (e.g. Madec et al., 2019), although it remains impractical for global observation-based applications because of the sparse distribution of direct current measurements. Exceptions to such application to observations are Freeland (2013), which used in situ Argo float observations to estimate w at a single depth in a limited domain of several degrees, assuming zero vertical flow at the surface, and De Verdière and Ollitrault, 2016 and De Verdière et al., 2019, which computed vertical velocities from the horizontal divergence of geostrophic velocities inferred from Argo-derived three-dimensional thermohaline fields and float displacements at their parking depth. In the last decade, alternative approaches used isopycnal displacements (Giglio et al., 2013; Christensen et al., 2023), the use of mooring data combined with the momentum and density balances (Sevellec et al., 2015), and biogeochemical tracers (Garcia-Jove et al., 2022). The theoretical frameworks have also expanded to include methods based on the Bernoulli function to infer w (Tailleux et al., 2023)."*

*) In several places, “Naveira Garabato” has been referred to as “Garabato.” Please revise this.

- Thank you for noticing. Changed in line 65 and reference list in lines 804 and 851.

*) Line 97: To the best of my knowledge, you do not need to assume the β-plane (i.e., tangent slope to sphere, f varies linearly, or β is constant). You only need to acknowledge that $\beta=\partial_yf$ (i.e., the meridional derivative of f exists).

- Thank you for this clarification. What we intended to communicate is that our formulation requires that the meridional derivative of f exists locally. We have therefore revised the text accordingly in lines 100-101: “*This formulation is equivalent to the continuity equation of the geostrophic flow when considering a β-plane (Pedlosky, 1996), where β is the meridional derivative of the Coriolis parameter without requiring the classical assumption of a linear variation of f across the domain*”

*)After equation (2): Please add “assuming $f\neq0$.” This clarification is important.

- Thank you for pointing out. Added in line 113.

*) Line 102: Please replace “geostrophic flow on a β-plane” with “horizontal geostrophic flow.”

- Changed in line 114.

*) Line 122: This is the reference paper for the ANDRO dataset (deep displacements of Argo floats). I think that this is not be the correct reference, or I don’t understand why you are citing it in this context.

- Thank you for pointing this out. You are correct: although the ARMOR3D product has been validated against the ANDRO dataset in previous studies, ANDRO is not used in the construction of ARMOR3D itself. Since the reference is not relevant in this context, we have removed it from the manuscript.

*) Sentences on lines 123-124: I do not find a reference for that. However, it is “admitted” in the community that this approach is not ideal. Have you tested your ability to execute this? How does the surface error propagate at depth? From my understanding, this is not as straightforward as it appears.

- Thank you for your comment. The statements on lines 159-167 are based on the works of Guinehut et al., 2012 and Mulet et al., 2012, which are cited at the beginning of the paragraph. We did not recompute the geostrophic velocities ourselves, we used the ARMOR3D product as provided for the community. Consequently, we did not perform additional testing of surface error propagation at depth, and our analysis assumes the quality and limitations reported in the product references.

Additionally, I am concerned that the boundary conditions for the vertical velocity are Ekman-based, while for horizontal flows, they are geostrophic. There exists a subtle but fundamental difference between the two. Have you carefully checked this? For instance, is it dynamically consistent to have a horizontal flow at the surface that does not “see” to the wind, while a vertical flow derives from a horizontal flow that does?

- Thank you for raising this point. To compute vertical velocities from the LVB, a reference velocity is required. While level of no motion reference would have been an option, its uncertain location can introduce significant errors. Using the surface as reference therefore appear to us as the most appropriate choice. Concerning, the reviewer’s point, at large scales, the geostrophic horizontal flow dominates the interior ocean dynamics, while large-scale surface winds modify the surface heigh patterns, which in turn are used to derive the geostrophic horizontal field. Within the theoretical framework presented in Section 2.1 and the appendix A, the only ageostrophic contribution considered is the Ekman component. This ageostrophic flow is confined to the Ekman layer and translates as Ekman pumping. As demonstrated theoretically, Ekman pumping can be defined at the ocean surface, while below the Ekman layer, the flow is predominantly geostrophic. Therefore, our choice of variables is dynamically consistent, in accordance with Pedlosky, 1996.

*) Line 230, equation (4): Why is there an absolute value? This seems illogical. Please make a simple difference. Furthermore, it would be mathematically more accurate to use \Delta w / \Delta \sigma on the left; the use of ∂ is mathematically ambiguous (i.e., unclear) and physically inconsistent (in terms of units).

- Thank you for this comment. We have revised Equation 4 (now Equation 13) to improve both notation and clarity. The absolute value of the vertical velocities is used in the definition of the vertical gradient to focus on the changes in amplitude within the thermocline, considering the positive and negative values could be misleading due to the existence of upward and downward vertical velocities across the domain. The revised equation and comments are updated in line 275: “*Negative values indicate a decrease in magnitude with depth, while positive values indicate an increase.”* We replaced the ∂ with \Delta_z.

*) Lines 284-293: Please specify the frequency of the vertical velocity (i.e., w_tot from the OGCM). If it is once every 5 days or lower, I am not surprised by these results. However, if it is hourly, I would be surprised that the vertical flow energy is not dominated by inertial motion, which would weaken the correlation with the geostrophic flow.

- Thank you for this comment. The OGCM provides vertical velocities at monthly frequency, as indicated in Table 1. However, for the correlation analysis presented in Section 3 we have used annual frequency following Cortés-Morales and Lazar, 2024. We have included *“... at annual frequencies ...”* in line 329 to avoid confusion on the temporal resolution of the analysis.

*) Figure 3: This does not represent a vertical gradient but rather a diapycnal gradient. Additionally, if you wish to show a relative term (which is debatable, since the denominator can be 0), you should use an absolute value in the denominator (otherwise, the sign becomes ambiguous, as the denominator can be both positive and negative).

- Thank you for your comment. You are correct: the quantity shown in Figure 3 (now Figure 4) represents the diapycnal gradient rather the vertical gradient. Following your suggestion, we have updated the units to m day^{-1} and computed the gradient using the distance between isopycnal levels, yielding a diapycnal gradient in m day^{-1} m^{-1}. Additionally, now use the absolute value of the diapycnal gradient in the denominator when calculating the relative error, to avoid ambiguity in sign. The relative error is intended to quantify the deviation of the geostrophic vertical velocity gradient from the total gradient.

*) In several places, “x” should be replaced by “$\times$” (e.g., line 303).

- Thank you for your comment. We have changed it.

*) Caption of Figure 7: Please replace “,,” with “,” and replace “and” with “, and.” *)

- Changed.

For Figures 9 and lines 463-465: Again, R2 would provide much more informative insights!

- Commented in Major Point (2).