0. Referee #1. This manuscript evaluates the impact of slowly varying components of wind stress and ocean stratification on the Niño3.4 index for sea surface temperature in the central tropical Pacific. The analysis offers an interesting analysis approach for evaluating processes governing central Pacific variability. However, I have reservations about the study in its present form.

Authors.
We sincerely thank Referee #1 for the careful reading of our manuscript and for the constructive and insightful comments. We greatly appreciate the time and effort devoted to evaluating our work.
In the interactive discussion phase, the discussion paper cannot be modified. Therefore, in the responses below, we address each comment individually and indicate how we intend to revise the manuscript in the next submitted version. Where appropriate, we also provide the proposed revised text or describe the planned changes to clarify how each point will be addressed.
We hope that our responses satisfactorily address the referee's comments and help improve the quality and clarity of the manuscript.

<u>Major concerns</u>

1. Referee #1. The study makes use of Copernicus data extending from 1960 through 2020. The record therefore begins prior to the start of the satellite era, and it is not clear that the data used for the analysis are consistent or realistic for the duration of the record. The authors do not seem to comment on questions about data validation or appropriateness for the analysis.

Authors.
Referee #1 is correct in pointing out that ERA5 SST data, while popular, are considered less reliable before the satellite era (i.e. 1979) compared to the modern era. This is due to the fact that the daily observations used in ERA5 have increased from 30,000 per day in 1960 to 25,000,000 today (Soci et al. 2024, doi: 10.1002/qj.4803). This is particularly true in the tropical Pacific, where SST observations were sparse before 1979. In order to fill these gaps, ERA5 relies on statistical interpolations. However, due to the lack of data before 1979, the interpolation scheme used in ERA5 may dampen the spatial amplitudes of daily SST gradients. Therefore, the use of this data set may limit the analysis prior to 1979. It is difficult to fully address this issue without comparing it to a more reliable data set. ERSSTv4 or 5, while having a lower spatial resolution of $2^o \times 2^o$, are obtained using artificial neural networks and spatial reconstructions that are optimized to handle historical Pacific sampling gaps. This data set can serve as a baseline to validate the N34 deduced from ERA5 in the period 1960-1979, although this comparison may not fully address Referee #1's concern. In Figure L.1 of this letter, we have plotted the average SST over the region 6ºS-6ºN, 190ºE-240ºE obtained from ERA5 and ERSSTv4 (https://pls.noaa.gov/data/climateindeces/list/) data sets. This region covers the Niño 3.4 region, but it is slightly wider. This is because the spatial resolution of ERSSTv4 is $2^o \times 2^o$ and latitudes start from 88ºS. We believe that comparing these average values may help to address Referee #1's concern. In Figure L.1, we have also added error bars based on the standard deviations of the average SSTs. In order to make the figure more readable, we have plotted one error bar for every 12 months, starting from January 1960. We can see that within the margin of error, the two data sets are equivalent in terms of spatial averages. Since this study uses SST anomalies averaged over the Niño 3.4 region, this result reassures us that our findings would remain qualitatively the same even if we used ERSSTv4.

Since the journal format does not allow for the inclusion of "Supplementary Material", we address the comment from Referee #1 in a new appendix (Appendix D: Selection of SST Data Set), based on the previous discussion.

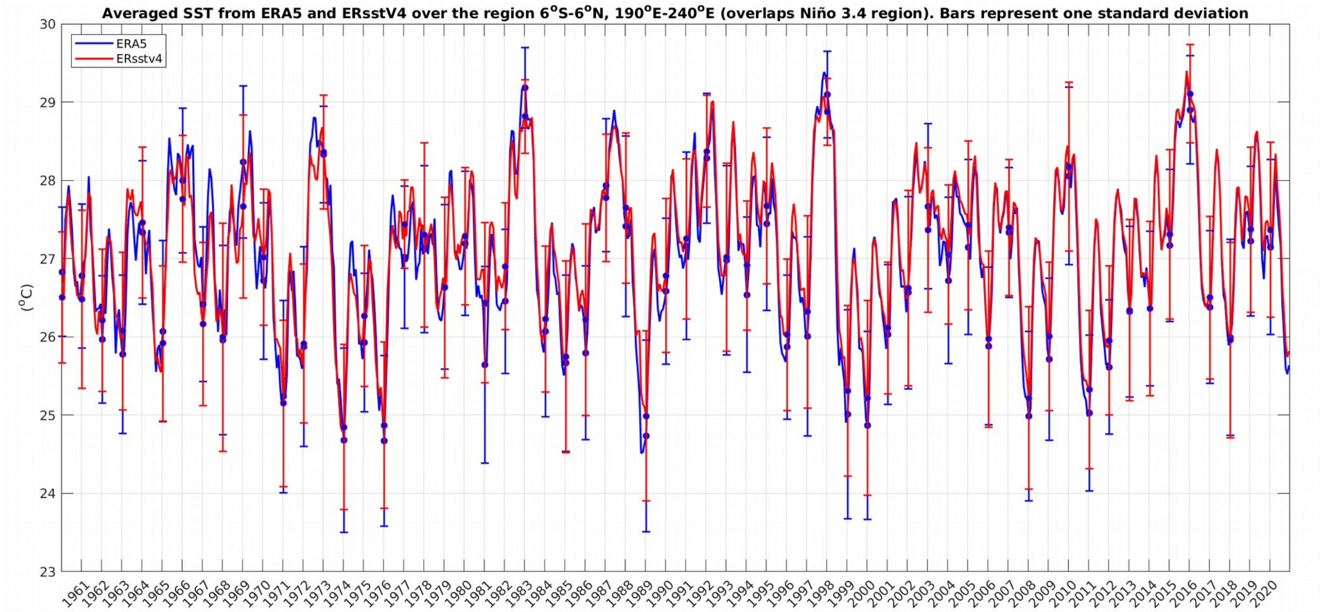

Figure L.1. Spatially averaged values of the SST deduced from the ERsstv4 and ERA5 over the region 6ºS-6ºN, 190ºE-240ºE

2. Referee #1. Figure 5 has an unexpected structure and needs to be checked carefully, and if it does not reflect a coding bug, then it needs to be explained. The most concerning aspect is the step-like monotonic increase in PC1 in Figure 5b. Since Figure 5d does not contain step changes, the year-by-year step changes in Figure 5b look like they could be the result of an erroneous signal fed into the EOF that is somehow dominating the mode 1 solution. For an ENSO-related signal, there is little or no reason to expect anything to show a steady monotonic increase over a 60-year period. I looked at the author’s code to try to gain clarity on this and was unable to identify the Matlab program responsible for the plots in Figure 5.

Authors.
Thanks for spotting this, we corrected the mistake and the updated Figure 5b is anticipated below.

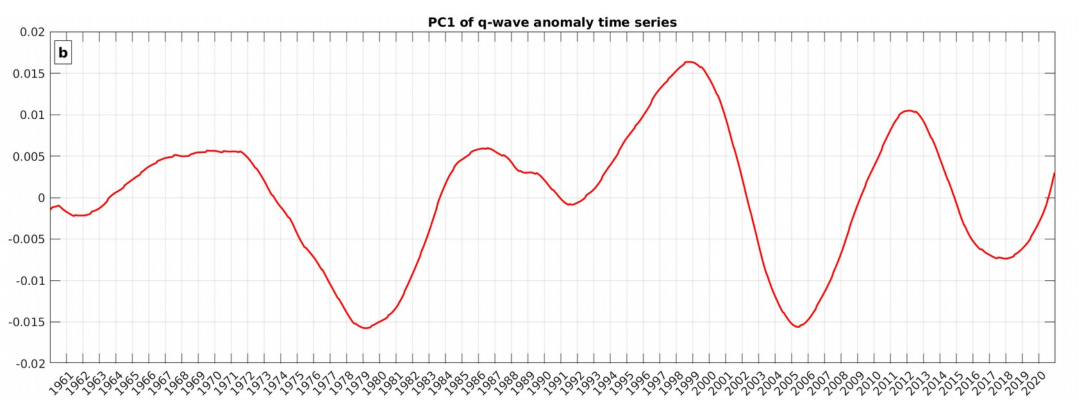

Figure L.2. Figure 5b of the original text corrected.

Naturally, the part of the original text discussing Figure 5b (i.e. L334-L338) was changed to account for the this corrected figure.

3. Referee #1. In section 4, the manuscript switches from discussion Z_{15} to h. The term h is defined as the departure of the interface layer from D, but it is not linked to Z_{15}, and it's unclear how the derivation in section 4 relates to the derivation in the previous section.

Authors.
In the original text, the relationship between Z_{15} and fluctuations of the interface layer (referred to as "h" in Section 4) was discussed in the introduction, specifically in the context of the two-layer description of the Tropical Pacific. This was mentioned when citing Capotondi and Sardeshmukh (2015) (L153-L155). Concurring with Referee #1, a brief introductory phrase at the beginning of Section 4 will be offered to assist the reader in understanding the narrative.

4. Referee #1. Setting h_{eq} and ϕ: At line 260, the authors note that $-\nabla^2 h_{eq} = \nabla^2\phi$, and use this as a justification to set $-h_{eq} = \phi$. This doesn't strictly need to be true, since h_{eq} and ϕ could differ by a gradient plus a constant, so the authors should clarify the logic that allows them to define ϕ. Subsequently, at line 293, the authors assume partial $\partial h_{eq}/\partial t = 0$. Taken together, these arguments seem to assume that phi is time invariant. The assumptions underlying these approximations should be justified more carefully.

Authors
Referee #1 is correct in requesting clarification of the assumptions $-\nabla^2 h_{eq}=\nabla^2\phi \Rightarrow -h_{eq}=\phi$ and $\partial h_{eq}/\partial t = 0$. These assumptions are crucial to our reasoning and are key to understanding why we have decomposed the wind stress using the Helmholtz decomposition.

However, before we continue, we would like to clarify the terminology used. Please see also point 6b, "other concerns" of Referee #1. In geophysical sciences, the term "hydrostatic" often refers to the approximation $\rho g=-\partial P/\partial z$, where P is pressure, ρ is density, and g is gravity. This approximation is valid when $W/U \ll 1$, where W and U are the magnitudes of vertical and horizontal velocity, respectively (Pedlosky, 1987). However, in classical textbooks of fluid mechanics, the term "hydrostatic" has a much more restrictive meaning. In Chapter 2, titled "Pressure distribution in a fluid," White (1994) states: "When the fluid velocity is zero, denoted as the hydrostatic condition, the pressure variation is due only to the weight of the fluid". Similarly, Batchelor (2000) (the entire book can be downloaded for free at https://elmoukrie.com/wp-content/uploads/2022/04/g.-k.-batchelor-an-introduction-to-fluid-dynamics-cambridge-university-press-2000.pdf) defines the hydrostatic state as "the condition of a fluid being completely at rest relative to some inertial frame of reference." Batchelor (2000) formally proves that if $\mathbf{u}=0$ everywhere, the Navier-Stokes/Euler equation naturally simplifies to the scalar balance $\rho g=-\partial P/\partial z$. Additionally, the Glossary of Meteorology of the American Meteorological Society (AMS, 2000) defines Hydrostatics on page 385 as: "That part of fluid mechanics restricted to fluids in which the velocity (linear or angular) of mass motion does not vary from point to point". This definition aligns with Batchelor's (2000) definition, as a fluid with no variation in velocity from point to point can be described in an inertial frame of reference where the velocity is zero everywhere.

When we mention "hydrostatic state", we use the definition used in classical textbooks of fluid mechanics as White (1994) and Batchelor (2000), which appears to be implicitly adopted by the AMS in the Glossary. So when we mention "hydrostatic" we imply $\mathbf{u}$ equal to zero everywhere in the fluid.

Perhaps, the manuscript clarity could be improved by substituting the term “Hydrostatic” with “motionless” and/or “fluid at rest”.

After this clarification, it is important to note firstly that h_{eq} and ϕ cannot differ by a gradient, as they are both scalars and the gradient is a vector. In principle it is possible that, if $\nabla^2 h_{eq}=-\nabla^2\phi$, ∇h_{eq} and $\nabla\phi$ differ by a the curl of an arbitrary vector field **b**, i.e. $\nabla h_{eq}+\nabla\phi=\nabla\times\mathbf{b}$. However, this is not the case. To demonstrate this position, consider the momentum equation

$$\frac{1}{g'}(\partial \boldsymbol{u}/\partial t+2\boldsymbol{\Omega}\times\boldsymbol{u})=\nabla h+\boldsymbol{\tau}/(g' H\rho)$$

After decomposing the dimensionless wind stress (DWS) into its irrotational ($\nabla\phi$) and solenoidal ($\nabla\times\mathbf{A}$) components (i.e. $\nabla\phi+\nabla\times\mathbf{A}=\boldsymbol{\tau}/(g'H\rho)$), <u>we define h_{eq} as the interface between the surface and bottom layer in equilibrium with the irrotational component of the DWS with the fluid at a hydrostatic or motionless state ($\mathbf{u}$=0).</u> This implies, according to the momentum equation, that $\nabla\phi + \nabla h_{eq} = 0$. In other words, with our definitions, the curl of the arbitrary vector field must be zero (i.e. $\nabla\times\mathbf{b}=0$). Additionally, the continuity equation shows that, given $\mathbf{u}=0$, $\partial h_{eq}/\partial t = 0$. So, h_{eq} can be identified directly from ϕ unless for an arbitrary constant. However, in this research, we are interested in interannual and longer-term variability. As a result, we analyze anomalies and remove any steady signals through anomaly computation.

5. Referee #1. The methods presented in section 3 are described without citations, as if this is brand-new material. In reality, the approach appears to be a fairly textbook approach to least-squares fitting and to empirical orthogonal functions. The work should be described in the context of previously published literature. I’ll also note that the presentation would probably be easier for readers to understand if the equations used matrix notation.

Authors

Referee #1 is correct: the approach used is a standard methodology based on least-square-fitting and EOF analysis, as described in detail for instance by Bretherton et al. (1992), von Storch and Zwiers (2001), and Bjornesson and Venegas (1997). Since these are quite standard methods widely known, we wrongly did not quote specific references. We apologize if this caused possible misunderstandings and in the revised version of the manuscript we will cite in section 3 the references cited above.

Regarding matrix notation, it is our belief that the decision to use either scalar or matrix notation is a matter of personal preference. The readability of the notation is dependent on the reader's individual tastes and background. In our previous publications, such as Eusebi Borzelli et al. (2024) and Zhong et al. (2025), we consistently utilized scalar notation. In order to maintain continuity and consistency with our earlier work, we have chosen to continue using scalar notation in this paper. However, if Referee #1 deems it really necessary, we are willing to rewrite equations (1) through (6) in matrix form.

6. Referee #1. Figure labels and legends are too small to be read. As a courtesy to readers, authors should assume that readers might have printed out the manuscript on paper and need to be able to read it without hunting for a magnifying glass.

Authors

We agree with Referee #1 and apologize. In the new version of the manuscript, figure labels, ticks and legends will be magnified in order to allow easier reading.

Other concerns

1.b Referee #1. The first few lines of the introduction (lines 45-51) provide a general introduction to El Niño that would be appropriate for a popular science article but seems out of place in this analysis. I think the authors can start with their concerns about the Bjkerknes feedback and the Walker circulation.

Authors.
We agree and thank for the suggestion. The new version of the manuscript will start as:

"The El Niño Southern Oscillation (ENSO) is the strongest year-to-year climate fluctuation on our planet. A crucial aspect of the ENSO cycle is the positive feedback relationship …."

2.b Referee #1, Lines 171-172. Formally the drag coefficient should probably be a constant for very high wind speeds---in typhoon conditions, a foam layer is established at the ocean surface, which is thought to limit further growth of the draft coefficient. That detail is probably not important for this study. However, the authors should be aware that there has been further research on drag coefficients since 1981.

Authors.
Yes, we are aware that there has been research on drag coefficients since Large and Pond's study in 1981. However, typically, for winds lower than 11 m/s, the drag coefficient remains constant at $1.2 \cdot 10^{-3}$. For winds between 11 m/s and 25 m/s, the drag coefficient increases linearly with wind intensity. Therefore, for winds ranging from low to moderate, the description of the drag coefficient by Large and Pond is sufficient, despite the existence of other formulations (such as Smith, 1980). However, as Referee #1 correctly points out, the problem arises when wind speeds exceed 25 m/s, such as in typhoon conditions. It should be noted that this study uses monthly wind data, which rarely captures wind events with intensities higher than 25 m/s. In Figure L.3 of this letter, we illustrate the percentage of points over the observation domain where monthly ERA5 data captures winds with intensities between 11 m/s and 25 m/s, as well as intensities higher than 25 m/s. It is evident that the number of events captured by the monthly ERA5 wind data set with intensities between 11 m/s and 25 m/s is always lower than 1%. Furthermore, the ERA5 data set does not capture any events that leave a signature on the monthly mean wind intensity stronger than 25 m/s throughout the entire observation period. This demonstrates that the application of the Large and Pond (1981) representation of the drag coefficient is accurate for this study.

3.b Referee #1, Lines 230-231. Note that the sign of the transfer function is arbitrary and depends only on the sign assigned to the EOF. To avoid confusing readers, it might be worth emphasizing that the sign applies to the figure but does not convey specific information about the solution.

Authors.
We respectfully disagree with Referee #1 on this matter. It appears that equation (5) in the original text relies solely on the EOFs, which are denoted as Y_n in the text. Referee #1 is correct in noting that the sign of the EOFs is arbitrary, which could make the sign of the transfer function seem arbitrary as well. However, if a specific EOF sign is changed (i.e. $Y_n \rightarrow -Y_n$), then the corresponding PC sign (denoted as α_n in the original manuscript) must also be changed (i.e. $\alpha_n \rightarrow -\alpha_n$). The coefficient c_n in equation (5) of the original manuscript is defined by equation (6), which shows that upon changing the sign of the n^{th} PC, the coefficient also changes sign. As a result, the contribution of a given EOF to the sum (5) does not change when its sign is changed. Therefore, the sign of the transfer function does convey specific information.

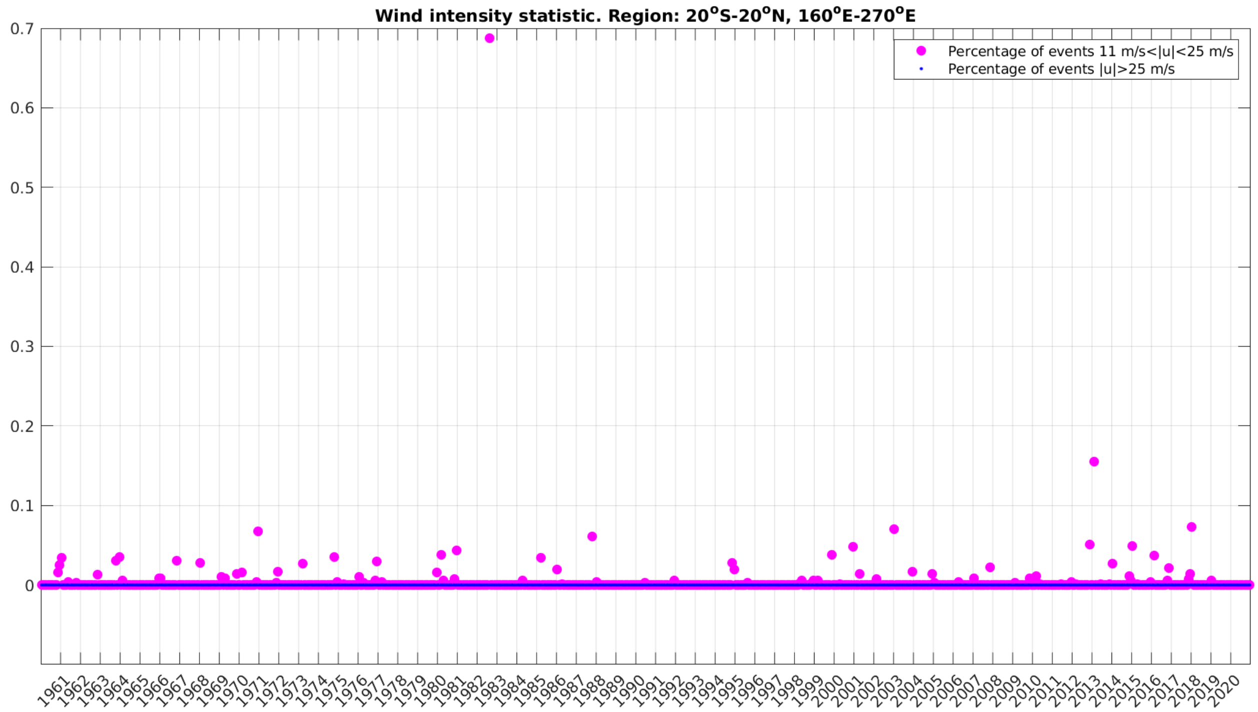

Figure L.3. Number of wind events with intensity comprised between 11 m/s and 25 m/s and higher than 25 m/s recorded by the ERA5 monthly wind data set.

4.b Referee #1 Lines 241-242. "solenoidal (i.e. divergence-free) and irrotational (curl-free) vector field". These words seem to be in reverse order relative to the terms in Equation (7), which is likely to confuse readers.

Authors.
We agree and thank Referee #1 for the catch! In the new text the order will be inverted.

5.b Referee #1 Line 242. Morse and Feshbach, 1953. While Morse and Feshbach might be the definitive reference for this material, there have been extensive publications in the last 10-20 years, and readers would likely be helped if they had pointers to the more recent literature as well.

Authors.
The Helmoholtz decomposition is well described in Morse and Feshbach, which is available in most libraries worldwide. This is why we referenced this book in our discussion. Our goal was to provide readers with a widely used handbook that explains the decomposition. However, we agree with Referee #1 that there have been several oceanographic studies in the past 10-20 years that have utilized this decomposition. We are familiar with the works of Forget and Ferreira (2019), Watterson (2001), and Han and Huang (2020), and will include them in the revised version of our manuscript.

6.b Referee #1 Line 258. Why is the equation in the text set equal to zero. It seems to be the righthand side of Equation (8), but I do not see a clear justification for assuming that the lefthand side should be zero in a hydrostatic state. Maybe the authors mean a word other than "hydrostatic"?

Authors
Yes, indeed. We believe there is confusion in the terminology. Please see authors comment to point 4 of Referee #1.

<u>Style and grammar</u>

1c Referee #1 Line 74. “is described” -> “are described”

Authors: Yes, sure.

2c Referee #1 Line 97. “in to” -> “into”

Authors: Yes, of course this will be changed

3c Referee #1 Line 154. “as a” -> “to be a” [You might not think that the subjunctive exists in English, but it does….]

Authors: thanks for that! No, actually we did not think that this subjunctive could exist in English. Thanks for making this point.

4c Referee #1 Line 179. “data includes” -> “data include”

Authors: thanks, yes.

5c Referee #1 Line 194. “is `optimal’” -> “be `optimal’”

Authors: yes, done.

6c Referee #1 Line 195. “(RMS) between” -> “(RMS) difference between”

Authors: yes, of course. Corrected

7c Referee #1 Line 195. “is minimum” -> “be a minimum”

Authors: Done,, thanks.

8c Referee #1 Line 196. “the bra-ket symbol”. This could be worded more succinctly (and more consistently with the oceanographic literature) as “the angular brackets”.

Authors: yes, agree. “Angular brackets” sounds better.

9c Referee #1 Line 243. Since the sentence following equation (7) defines the terms in the equation, it should probably not start a new paragraph. No indent.

Authors: sure. Referee #1 is right, “no indent”.

10c Referee #1 Line 254. “seek for” -> “seek “ or “look for”

Authors: yes. “for” was removed

11c Referee #1 Line 257. “to as quasi-equilibrium layer” -> “as the quasi-equilibrium layer”

Authors: thanks, done.

12c Referee #1 Line 293. “in to” -> “into”

Authors: yes. Corrected

13c Referee #1 Line 370. “by the equilibrium dynamics, which encapsulates” -> “by equilibrium dynamics, which encapsulate”

Authors: yes. Done. Sorry for that: we have often this problem because in Italian dynamics is singular. Thanks for this point.

14c Referee #1 Line 507, 687. “Wusch” -> “Wunsch”

Authors: of course. Corrected.

15c Referee #1 15c Line 514. Need a line break.

Authors: yes, indeed. Need a line break. The original text was written in LibreOffice and then converted in Word. Something went wrong in the conversion. Sorry for that.

References

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