Reply to reviewer: for “**Brief communication: Impact of swell waves on atmospheric surface turbulence: A wave-turbulence decomposition method”**

Reviewer comments are presented in black text using the "Calibri" font format with a size of 12. My responses are displayed in blue text using the "Calibri" font format with a size of 13.

**Reviewer 1**

Thanks a lot for your manuscript. I found it quite interesting, and I would like to suggest that this should not be reviewed as a “brief communication” but as a “research article”. I am not an editor of the journal, so I do not know the formal distinctions, but I think the manuscript has sufficient material to become a paper and it is not brief (its current version has 13 pages). It actually has the size of the papers I like to read.

Thank you for your thoughtful review and valuable suggestions. I agree with your suggestion to consider the manuscript as a 'research article.' I've already discussed this with the manuscript's editor. Moving forward, I plan to expand the paper slightly by carefully addressing the specific comments provided by the reviewers. Your comments are greatly appreciated as they have already helped refine the quality and presentation of my work.

**Other main comments**

1. As I said the manuscript is quite interesting but right now it is difficult to read/follow because there is a part (I think) in which the method is applied in simulated wind fields and another part in which the method is applied to observations (I think). So it is not clear if the wind field simulations are actually used within the analysis of the observations or not.

   Thanks for your valuable feedback. It's important to note that the original format of the manuscript was a letter, which contributed to its concise nature, resulting in some limitations in providing comprehensive information and structural clarity. I understand and appreciate your concern regarding the unclear explanation. To address this, I revise and reorganize the manuscript to ensure a smoother flow and better delineation between the sections where the method is applied to simulated wind fields and where it is applied to observations. As an example, I modify the methodology section by renaming "2.2 Synthetic turbulence" to "Wind-wave interaction: Coherence and synthetic turbulence" and positioning it as section 2.1. This adjustment, coupled with improved explanations in the methodology section, enhances the overall coherence between wind field simulation and observations, making clear their significance and interconnection.

2. This is not clear neither in the abstract nor in the results. So I think that the author should make an effort to explain shortly and clearly the steps of the method, and clarify whether the results are divided into “simulations” and “observations” or if there is some combination: e.g., around lines 124-132 simulations are only used but it seems that after line 133 observations are used

I appreciate your attention to detail and for highlighting the need for clarity here. I acknowledge that the organization of the method and results sections could be improved for better understanding in order to be more accessible to readers. I will then take your suggestions into consideration and make the necessary revisions.

In this paper, I present two key developments. Firstly, I propose a representation for the wind-wave coherence spectrum crucial for creating turbulence data to be used in structural load analysis models and also creating synthetic wind-wave interaction data during swell wave conditions. Secondly, I introduce a method to separate wind and wave components using only sonic anemometer high frequency observational data. This method is implemented for both the synthetic model data and observational data. In the light of these two developments and along with your comment, following changes are made:

- I will revise the abstract and introduction to provide a more concise and clear overview of the method's steps, ensuring that the distinction between 'simulations' and 'observations' is explicitly stated.
- In the results section, I will modify further to provide a step-by-step explanation of the method, clarifying how simulations and observations

are utilized, and where any combinations occur (I will add two extra figures to address this concern).

- Specifically, I will revise lines 124-132 (and this section in general) to provide a clear explanation of the use of simulations and ensure that the transition to the use of observations after line 133 is seamless and well-explained.

To bridge between the theoretical model and observation, I will add one figure by including two sonics data. This helps further clarification of method.

3. The decomposition you are presenting in Eqn. (1) is generally known as “triple decomposition” (see e.g., Buckley and Veron, 2017). As most people working in air-sea interaction perform a decomposition like that in the latter study, it would be nice you describe what the differences are between yours and their type of decomposition. Also why not use their type of decomposition?

Various methods exist for decomposing wind-wave interactions, including phase averaging, linear transformation, and orthogonal projection of the wind onto the Hilbert space to estimate the wind-wave coherence signal, etc. Many of these techniques rely on complex cross-spectra between horizontal u and v fluctuating air velocities and vertical w fluctuating air velocities, along with sea surface elevation, to isolate the direct wave influence.

The choice of decomposition method in this manuscript, as outlined in the methodology section, is based on specific considerations related to the research objectives and the nature of the data I am working with (i.e. sonic anemometer data at 15m height above the mean sea level). I plan further to add a figure comparing between the suggested method in this manuscript with one or 2 other decomposition methods. My approach differs in the following ways that I will clarify in the manuscript:

- In summary, the approach solely utilizes sonic wind velocity data, omitting the need for concurrent high-frequency wave measurements in the decomposition process. It neglects velocity fluctuations within the wave band, assuming turbulence field stability during transformation into wavenumber space.

- Additionally, the method stands out as a physics-informed statistical approach that employs a turbulence spectrum model to effectively bridge the gap between high- and low-frequency sections in the observed spectra. This enables us to estimate the variance attributed to turbulent velocity fluctuations within the wave frequency band by learning solely from the energy spectrum of the corresponding wind component.
- Notably, this method uniquely provides wind-corrected and wave time series, a critical data component for structural analysis that is not accessible through other known methods in my knowledge (the link to structural response is an ongoing almost completed work that I am planning to submit).

In short, the decision to employ this decomposition method is rooted in the specific nature of the datasets I am working with, and my extensive experience in motion compensation of moving sensors in both atmospheric and oceanic environments in the presence of a wavy air-sea interface. Through this experience, I have found that filling spectral gaps using a well-established spectrum is an effective approach across a broad spectrum of atmospheric stability and sea state conditions, on both sides of the sea surface.

**To enhance clarity, I will include pseudocode for each method in the manuscript.**

4. In section 2.1 it is not quite clear why you start with a Kaimal wavenumber spectrum and not with a Kaimal frequency-based spectrum, which is much more known and popular.

I acknowledge that the majority of the analysis in this manuscript relies on the frequency spectrum. However, in the "Wind-wave decomposition" section, I incorporate the wavenumber spectrum. Although the Kaimal frequency-based spectrum is widely recognized and used, my choice of the wavenumber spectrum aligns with my approach to solving this specific problem (all my codes on this problem have been developed over time based on wavenumber spectrum). This approach draws inspiration from the one-dimensional wavenumber spectrum of turbulence as described by Hannoun et al. (1988), Kaimal et al. (1972), and Fung et al. (1992).

5. Also you mention that \sigma_\beta is an adjustable parameter, but is it? Is it not the standard deviation of the variable? If so then it is not adjustable but computable from the time/spatial series. The one that is adjustable as k_0\beta should be A, or am I missing something?

   In the manuscript k_{0\beta} represents the spectral roll-off wavenumber for the *beta* component of velocity. I employed a two-parameter least squares fit of this model spectrum to our observations to allow estimation of k_{0\beta} and \sigma_{\beta}, which describe the variance and the spatial scale of the energy-containing eddies. I will try to clarify this further in the manuscript.

6. (7): it is not clear if this is a suggestion made by you based on something or if it is already in the literature. It kind of comes suddenly and you need to provide a background for it.

   I appreciate your point about the need for background on the suggestion made for the theoretical coherence function. In general, this study aims for two primary objectives to develop:

   - a novel wind-wave coherence spectrum representation, pivotal for generating turbulence data in structural load analysis models and synthesizing wind-wave interactions during swell wave conditions.
   - a method for distinguishing wind and wave components solely based on high-frequency sonic anemometer observations or synthetically generated data. This method is successfully applied to both synthetic model data and observational data.

   The idea presented in the manuscript is the suggestion made by the author based naturally on prior existing research in the field of wind coherence. Building on these foundations, I proposed this theoretical relationship for the wind and wave coherence. To enhance the clarity and provide better context, I will include a more explicit reference to the relevant literature and explain a little more the concept.

7. Section 3.1. It is important to state which sonic anemometers are you using, I mean which type of sonics. You mentioned you do not filter for mast shadow (by the way you do not mention which is the direction of orientation of the sonics) but are you using sonic-specific corrections for probe distortions? If so, please tell us which.

   As the manuscript is reformulated from letter to regular research, I found this comment indeed important to be addressed appropriately. During the OBLEX-F1 campaign, we deployed two sonic anemometers at the FINO1 offshore meteorological mast. These sonic anemometers were positioned at heights of 15 and 20 meters above mean sea level, with a measurement frequency of 25 Hz. Their orientation was set at 135 degrees, which means that the wind shadow zone extended approximately above 300 degrees. I will include a detailed explanation on this matter in section “3.1 Datasets”. I may add a new figure.

## Specific comments

I have carefully reviewed all of the specific comments raised by the reviewer and they will be precisely incorporated into the revised version of the manuscript. Additionally, I include the reference you suggested.

Buckley M.P. and Veron F. (2017) Airflow measurements at a wavy air-water interface using PIV and LIF. Exp. Fluids. 58:161

## References

Kaimal, J., J. C. Wyngaard, Y. Izumi, and O. R. Cote, 1972: Spectral characteristics of surface-layer turbulence. Quart. J. Roy. Meteor. Soc., 98, 563–589.

Fung, J. C. H., J. C. R. Hunt, N. A. Malik, and R. J. Perkins, 1992: Kinematic simulation of homogeneous turbulence by unsteady random Fourier modes. J. Fluid Mech., 236, 281–318

Hannoun, I. A., H. J. S. Fernando, and E. J. List, 1988: Turbulence structure near a sharp density interface. J. Fluid Mech., 180, 189–209.