Based on feedback from reviewers and in consultation with the editor, the manuscript type has been changed to a regular research article. In response, several improvements have been made, including enhancing the abstract and introduction, rearranging the methodology section, adding three appendices with accompanying figures, and including three additional figures to address reviewer comments. Notably, the introduction now provides a more detailed literature review (two new paragraphs added) and a clearer explanation of the main objectives, the relevance of coherence, and the decomposition method.

Specifically, the approach in this paper aims to unfold following key aspects:

– Identifying and assessing swell-related wind-wave imprints on atmospheric velocity and two-point coherence structures during specific swell-dominated conditions (proposing a theoretical model for representing wave-induced coherence).

– Removing wave-induced peak effects from wind velocity spectra using a spectral technique and reconstructing turbulence and wave time series from the wave-affected sonic measurements, during stable atmospheric conditions.

In the Methodology section, efforts have been made to improve clarity and cohesiveness. A new figure has been introduced to illustrate the concept of observed coherence and how the method utilizes data from two sonic anemometers at different heights to remove wave contaminations. Specifically, the manuscript now discusses the impact of waves on power spectra, coherence, and structure functions.

The Results section has been refined to incorporate all the suggested clarifications and modifications. Additionally, two new figures have been introduced, and three appendices have been included to address reviewers' comments more comprehensively. All comments have been meticulously addressed in this version.

Reviewer 1

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.

Reviewer 2

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 2

Wave-Turbulence Decomposition holds significance for both the wave and atmospheric communities. However, it is a considerable challenge for the decompositon. Within this study, the author introduces a method for decomposing wave and turbulence fluctuations. The concept is intriguing and certainly warrants publication. Prior to the manuscript's publication, I have outlined several comments that the author may wish to consider addressing.

Thank for your thoughtful feedback. I'm committed to addressing these invaluable comments to enhance the clarity and quality of the manuscript. In particular, the brief definition of the wave boundary layer is provided to clarify how wave-induced orbital velocities influence velocity fluctuations in the lower marine atmospheric boundary layer, especially in stable conditions. The suggested decomposition method is compared to three wave-turbulence decomposition methods that rely solely on the spectral information of the timeseries. I will elaborate on the observational coherence by available sonic data at two different heights (with separation distance of 5m – I specifically use our sonic data at 20m for this purpose). This study also introduces a theoretical coherence method crucial for generating the cross-spectral density matrix to simulate fluctuating wind velocity and wave height. These contributions emphasize the model's importance and its effectiveness in analyzing aerodynamic and structural loads on offshore wind turbines.

Specific comments

1. **Several established methods for wave-turbulence decomposition have been utilized in previous studies. To provide context, I recommend that the author furnish an overview of these methods in the introduction. This should encompass the work conducted by Hristov et al. (2003, 2014), the spectral method outlined by Veron et al. (2008) and**

Grare et al. (2013), as well as the interpolation method described by Rieder and Smith (1998) and Högström et al. (2015).

I appreciate this feedback. Your suggestion to include references to established wave-turbulence decomposition methods such as those by Hristov et al. (2003, 2014), the spectral method by Veron et al. (2008) and Grare et al. (2013), as well as the interpolation method outlined by Rieder and Smith (1998) and Högström et al. (2015) will enhance the context and comprehensiveness of the manuscript. In response to another reviewer's comment regarding this manuscript's suitability for being considered as a regular manuscript, I have room for further elaborations and will improve the introduction section to provide a more detailed overview and background on the methods (by including references and background). Additionally, I'll include an appendix to explain a few decomposition methods with a figure to compare them with the suggested spectral method of this manuscript, ensuring a more thorough description of the methodology and its effectiveness.

2. In light of these established techniques, it would be beneficial for the author to address whether their proposed method has been compared to these prior approaches. Specifically, have the results obtained using the author's method demonstrated good agreement with those generated by the aforementioned methods?

   In my research, such processing tool, and its application, has evolved over several years, involving the application of various decomposition methods to high- frequency oceanic and atmospheric sensor data, such as shear probes data and ADVs for oceanic studies and sonic anemometer data for atmospheric studies. Notably, these decomposition methods differ, with some relying also on high- frequency surface wave elevation data (beside to wind and current time series) and others solely on high-frequency wind (current) velocity time series. I have conducted thorough comparisons between the suggested spectral method and a

couple of established approaches. These comparisons consistently demonstrated promising agreement, with the spectral method standing out as robust and efficient, acting as a statistical physics-informed gap-filling technique.

It is noted that selecting the appropriate decomposition method can be a complex task, depending on factors such as data characteristics, the nature of wind-wave

interaction (like misalignment, stability conditions, etc), advection of wave orbital velocities across a broad frequency range, and more [1]. I've found that the spectral method and its associated technique for deriving wave and corrected turbulence time series from wind speed (ocean current) frequency data are robust and valuable for various atmospheric flux studies over and under the wavy air-sea interface.

I will clearly present and discuss in the manuscript, affirming the effectiveness of the approaches, by comparing the technique with few other ones I made using solely the spectral information, specifically I add an appendix and detail them there.

3. Furthermore, it would be pertinent to explore the strengths and limitations of the author's method in comparison to the existing alternatives. Within the scope of this study, a comparison has been made between the method developed by Hristov and the approach outlined by Veron et al. in 2008. It is noted that the results obtained from these two methods show an acceptable level of agreement, as previously established by Wu et al. in 2008. This comparison adds credibility to the validity of the author's method. Incorporating this comparative analysis would provide a more comprehensive understanding of the novelty and effectiveness of the author's proposed approach in relation to the existing methodologies.

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:

- **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.

In summary, 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 address this comment, I've added an appendix comparing three methods for isolating wave motions from the vertical wind velocity data. These methods, the stopband filter, the intrinsic mode function, and linear interpolation in frequency domain don't rely on wave elevation time series but solely on sonic data. The stopband filter is employed by knowing the wave peak frequency or dominant wave frequency band. I use an estimation for the frequency band as 0.6fp to fp+0.1. The SB filter method significantly reduces energy within the wave- dominant frequencies, resulting in an associated underestimation of turbulent energy; this is approximately the same for the intrinsic mode functions that can be

further improved while the method alone may not completely eliminate the wave velocities. The linear interpolation in the spectral domain may be sensitive to the choice of the wave frequency band.

4. The presence of multiple layers of sonic sensors introduces an intriguing opportunity for validation. It would be highly compelling to ascertain whether the wave coherence contribution as discussed in Section 2.2 aligns with the findings derived from the methods

detailed in Section 2.1 across the various sensor layers. This comparative analysis could yield valuable insights into the consistency and reliability of the outcomes.

Thank you for this insightful comment. The idea of a comparative analysis across the various sensor layers is indeed intriguing. In the revised version, I explore the possibility of aligning the wave coherence contribution discussed in Section 2.2 with the findings derived from the methods detailed in Section 2.1 across these multiple sensor layers. This analysis has the potential to provide valuable insights into the consistency and reliability of method outcomes. Furthermore, I will incorporate time series data from another sonic anemometer operating at a 20- meter height. This addition will allow to estimate the observational coherence between the 15-meter and 20-meter sonic anemometers, bridging the gap between the theoretical coherence function proposed and real-world coherence data.

The following figure illustrates coherent structures at two different heights (15m and 20m). In Fig. 1a, a 20-minute sonic data time series at these heights is displayed, while Fig. 1c shows the observed coherence and the theoretical model results. This also sheds light on why the theoretical coherence formula incorporates the wave-induced bump. To address this concept and establish a connection between the observed coherence and my proposed theoretical formula, I will include the fitting of the theoretical coherence to this data. A more detailed explanation will be provided in the methodology and results sections.

Figure 1. The observational and theoretical coherence representations for two sonic anemometers at 15m and 20m heights.

5. It is not easy to follow the connection between sections 2.1, 2.2, and 2.3. Please consider restructuring it.

Thank you for your suggestion regarding the restructuring of methodology section/subsections. I will improve and enhance the clarity and comprehensiveness of both the introduction and methodology sections of the paper.

In response to restructuring, I will undertake the following steps to address your comments:
Introduction section: I will restructure the introduction to provide a more comprehensive overview of the established methods for wave-turbulence

decomposition. This will include references to the work by Hristov et al. (2003, 2014), the spectral method by Veron et al. (2008) and Grare et al. (2013), as well as the interpolation method outlined by Rieder and Smith (1998) and Högström et al. (2015). By enhancing this section, I will offer readers a stronger foundation for understanding the context of this research.

Methodology section: I will revise the methodology section to explicitly address the comparison between the proposed methods and the established approaches (I use specifically three wave-turbulence decomposition methods as explained in reply to comment 3). This comparison will be presented in a more structured and detailed manner, highlighting the consistency and agreement observed in the evaluations, by emphasizing on the robustness and efficiency of the suggested spectral method as a statistical physics-informed gap-filling technique.

Result section: I will correspondingly revise the result section by adding two new figures (Figure 1 is a sample plot).
I believe that these changes will significantly enhance the quality and clarity of the work.

6. For section 2.3: Eq. 11 is only valid for the surface. Thus, it should not have the dependent on z which is confused the readers.
   I agree and the text is enhanced by defining the wave boundary layer as the region where the non-static pressure distribution on the surface layer becomes apparent, with a height of impact corresponding to several significant wave heights (Hs). For medium waves, the typical WBL height is a few meters, while for larger waves, it can extend up to say 20 meters. The WBL interacts with the wave surface below and merges with the Monin-Obukhov stratified boundary layer above. Within the WBL, surface wave movements influence the structure, which is shaped by the specific characteristics of the wave field.
7. In the manuscript, you use many "we". Since there is only one author, it should be "I" instead.
   Thank you for your feedback. The use of "we" in my manuscript is my impression that this is a common convention in academic writing, even when there is a single author. It can make the writing more formal and objective. However, I can certainly make the change to use "I" instead if it is the preferred style.

References:

[1] Bakhoday Paskyabi, Mostafa.

Applied Sciences

Springer Nature **A wavelet-entropy based segmentation of turbulence measurements from a moored shear probe near the wavy sea surface.**