Reviewer #1: The authors have responded to the comments, however, it should be noted that there are no expermental or laboratory validations of the proposed approach. The following comments are provided:  
  
1) No experimental studies are conducted to verify the proposed appraoch, which may not meet the HIGH standard of MSSP.

We acknowledge the reviewer’s concern regarding the absence of experimental or laboratory validation. At this stage, our study was intended as a proof-of-concept based on high-fidelity numerical simulations using industry-standard software (DNV’s Bladed) with realistic offshore wind turbine models and environmental conditions. While numerical studies are widely used in the early development of new SHM methodologies, we fully agree that experimental validation is crucial for elevating the study to meet the highest standards. We have clarified this limitation in the revised manuscript (Section 6 - Conclusions) and emphasized that future work will focus on experimental campaigns, including scaled model tests and field data validation, to further demonstrate the applicability and robustness of the proposed method.

2) Even for the numerical studies, no validation or calibration of the modelling technique is conducted to show the comparions of the proposed modelling technique with existing methods for OWTs.

We appreciate the reviewer’s comment regarding the lack of validation or calibration of the numerical model against existing methods for offshore wind turbines (OWTs). We wish to clarify that the primary contribution of this paper lies in introducing a novel conceptual approach for effectively removing environmental and operational variations (EOVs) by leveraging the relative vibration characteristics between adjacent OWTs within the same wind farm. The specific choice of Gaussian Process Regression (GPR) was made to demonstrate the feasibility of this approach clearly and intuitively, rather than suggesting that GPR itself represents the novelty. Indeed, GPR could potentially be replaced by other regression or machine learning methods without altering the fundamental idea of our approach.

Thus, the emphasis of our work is the innovative concept of utilizing inter-turbine relationships to mitigate EOV effects, rather than developing or validating a specific numerical modelling technique. Nonetheless, we fully agree that comparisons with other numerical methods or regression techniques would further enrich our study. Future work will explore alternative modelling approaches and include direct comparative analyses to strengthen confidence in the general applicability of the proposed concept.  
3) It is noted that the link between two OWTs is used for damage detection. However, it should be noted that when vibration characteristics of two OWTs are different, the effect of EOVs to vibration characteristics is different for different OWT. This may make the proposed appraoch not valid for damage detection.

We thank the reviewer for highlighting this important point. In practice, minor differences between adjacent OWTs (e.g., variations in soil conditions, foundation stiffness, and water depths) indeed exist, potentially influencing their individual vibration characteristics under environmental and operational variations (EOVs). However, our proposed method specifically leverages the relationships between adjacent OWTs, as captured by the Gaussian Process Regression (GPR) during a training phase under known undamaged conditions.

Even though the individual responses of OWTs to identical EOV conditions may differ, the relationship between the vibration characteristics of adjacent OWTs is consistent and stable in undamaged conditions. It is precisely this stable relationship—not the absolute values—that the GPR model learns during training. Consequently, any deviation from this established relationship during monitoring clearly indicates potential damage, rather than differences in baseline properties.

4) Significant errors in Figs. 9a and b may introduce the errors in damage detection.

We appreciate the reviewer’s concern regarding the observed discrepancies in Figs. 9a and 9b. However, we would like to clarify that the differences between the predicted and actual frequencies shown in these figures, particularly after the damage event, indicate the effectiveness of our proposed approach rather than introducing errors. Before damage occurs (16 May to 31 May), the predictions closely match the observed frequencies, demonstrating accurate modeling and prediction performance. After damage occurs (1 June onward), the prediction significantly deviates from the observed data due to changes in structural properties, which is exactly the intended outcome. Such deviations are essential to trigger alarms within the control chart-based monitoring framework, thus accurately detecting and localizing damage.  
  
  
Reviewer #2: I acknowledge the authors effort to answer and clarify my concerns, and I understand that some points can be left for future works, as per authors suggestion. However, I still feel some points should be further discussed:  
1. The authors have rephrased the tide/waves discussion to ""In this study, a parked OWT operating under cut-in wind speed and calm sea conditions was assumed. Under these conditions, both tidal levels and wave heights primarily affect the natural frequency through added mass effects. To simplify the analysis, only the tide level was used to represent the combined influence of tidal levels and wave heights.". I am still not convinced this is the case. Why would the waves height change the natural frequencies of the system? Note that the tide level effectively changes the global volume of water, but the waves height, in average, does not. This implies that the tide level will mostly impact the natural frequencies of the system, while the waves will mostly impact the damping level, but not necessarily the frequencies (assuming a linear structural model in both cases). Once more, I understand that the authors want to focus on a single case, but I think one of 2 approaches can be used: either the authors show evidence that the impact on the natural frequencies is similar (which I don't think is the case), or rephrase to something like "since tide level impacts the system natural frequencies, only this scenario will be analysed".

Thank you very much for your insightful comment and for highlighting the distinction between the impacts of wave height and tide level on the dynamic properties of offshore wind foundations. We agree with your assessment and have rephrased the relevant paragraph as follows:

“Wave heights can influence the resonance frequency by affecting the damping characteristics of the system, but they do not directly alter the natural frequency, assuming a linear structural model. In contrast, tide levels effectively change the submerged length of the monopile and therefore the overall added mass of the system, which directly impacts the natural frequency. Consequently, in this study, only tide level is used to represent environmental variations, as it has a more direct influence on the natural frequencies of the structure.”

2. I understand that extending the analysis to new damage scenarios may be out of scope for this work, but I think the answers the authors gave regarding multi-input prediction (Figure C3 and C4) and the damage in OW3 (Figure C4 - I think there are two figures with the some label in the responses) should be summarised and included at the end of Section 4.

Thank you for the helpful suggestion. We have now incorporated a summary of the multi-input prediction results (Figures C3, C4) and the conceptual analysis regarding damage in OWT 3 in Section 5 (Discussions). This addition highlights the robustness and scalability of the proposed approach, as well as potential future directions.

3. Please check the fontsize of the tables. For instance, Table 3 seems to have a fontsize bigger than the rest of the document

We have standardized the font size across all tables to maintain consistency throughout the manuscript. Thank you for pointing this out.  
4. Please check the figures size for the final document. For instance, in Fig. 9 and 10, I think it would be better to have the 3 plots side by side on a single line.

We appreciate the reviewer’s suggestion regarding figure size and layout. Figures 9 and 10 will be carefully arranged during the editing and proofreading stage, ensuring they are clearly presented side-by-side on a single line for optimal readability and clarity in the final version of the manuscript.  
5. Please check the final position of the figures. For instance, Fig 12. and 13 should be incldued before ending the corresponding sections.

We thank the reviewer for pointing out the positioning of Figures 12 and 13. We will carefully reposition these figures before the final submission to ensure they are appropriately placed within their respective sections.