Dear Referee 1,

Thank you very much for the time and effort invested in conducting such an exhaustive review of the submitted manuscript. All of your feedback was greatly appreciated, as it raised very interesting points that helped us to improve our work. We present here our authors' comments to your feedback. Please find our responses and manuscript changes described in this document.

RC1: Comments on wes-2025-195, 19 Nov 2025

A) First, the paper does not provide all data/information required to reproduce the results.

A1- Authors' response: Thank you for bringing up this point. To be more transparent with the information and allow for better repeatability of the results, the input task lists used in the model are now based on the tasks and durations used on a study conducted by Jalili et al. in the Interreg North Sea Region report (2022), Project Number: 20180305091606. Therefore, all information required for independent validation is now easily available.

A2- Changes in manuscript: Table 1, and Appendix Tables A1-4 adjusted to reflect the tasks and durations from the report by Jalili et al. (2022).

B) Second and even more important, the entire uncertainty assessment seems to be based on very vague assumptions. I hoped for some data provided by industry. However, data is either taken from literature (suitability is sometimes questionable (see comment 3) or parameter values are just assumed without sufficient justification.

B.1- Authors' response: Thank you very much for the question, it touches on a very important point. The values presented in the uncertainty assessment, although sometimes based on subjective assumptions, are largely a result of previous work conducted by a prior project at our institute. Looking into the work conducted by Garcia Munoz et al. (2023), it is mentioned that these parameters were derived from interviews with industry experts, compiling industry knowledge concerning the consequences of uncertainty in offshore logistics projects. That being said, the focus of our work is centered on developing the methodology in such a way that when further data becomes available, the model can be easily adjusted to yield updated results.

B.2- Changes in manuscript: The sentence in line 77 was rephrased this to give more transparency to the assumptions used for systematic uncertainty:

Derived from expert judgment, this uncertainty was modelled following a PERT distribution that ranges from a 10 % time reduction to an increase of 50 % (Wiggert, 2022).

Additionally, the following paragraph was added for clarity in line 61:

Given that a central idea of the project is to be able to give numerical values to hard-to-quantify uncertainties, a systematic review of relevant literature and existing risk assessments in the offshore wind industry is carried out for risk identification and parametrization. The sources were screened to keep only those risks that are relevant to the turbine decommissioning phase, as well as general project risks that have a direct impact on decommissioning duration. Additionally, the risk parameters are based on interviews with industry experts conducted by Garcia Munoz et al. (2023). The interviews served to compile industry knowledge concerning the probabilities and consequences of relevant risks, asking the interviewees for subjective estimations where no data was available. The values for the risk catalog also consider analysis on publicly available data on standards, accident statistics, and structured risk analysis methods like Fault Tree Analysis and FMEA.

C) And third, the overall innovation seems to be quite low. The methodology is relatively similar to what has already been done for OWF installation. Surely, installation and decommissioning are two different things. However, if the underlying data is not based on real decommissioning data or at least industrial experience/expert knowledge but on the same assumptions made for installation, a case study will not yield very relevant findings. Hence, methodological innovation must be even higher, which is not the case here. Therefore, I cannot recommend the paper for publication without substantial improvements and extensions.

C.1- Authors' response: Thank you very much for sharing your perspective on the innovative aspect of the work. While it is indeed true that similar approaches have been presented in other papers focused on risk management for offshore operations, for example by Garcia Munoz et al. (2023). One of the key novelties of this study is to conduct the full implementation of the methodology, which has not yet been conducted. In addition, the project partners did provide new data derived from their experience with offshore operations, which helped build the risk catalog used to model delays and sources of uncertainty to a level which previous work did not achieve. In addition, a full sensitivity analysis was carried out to expand on the uncertainty study of the model. We hope that under this new light it is easier to see the innovative aspect, and hopefully deem it worthy of publication.

Subsequent results coming from this project will expand on new data coming from project partners, allowing the study to include a more comprehensive list of risks. Further scenario analysis would also allow further insights related to parameters that have not been analyzed in similar offshore wind farm decommissioning studies.

C.2- Changes in manuscript: rephrasing last part of the methodology section to stress the novelty of the work conducted in line 43:

In addition to these steps, built after the steps laid down by previous researchers focused on offshore logistics, this work also includes a novel implementation of the detailed methodology in a reference wind farm. For this, all sources of delay are considered in a single model, which simultaneously assesses the key sources of uncertainty in a full scale OWF decommissioning project.

Further concrete comments are:

1) In line 21/22, you state that there is a lack of risk models that integrate task-specific process risks. This might be correct for the decommissioning. However, for the installation of OWF, there are such approaches and you use them. Hence, as installation and decommissioning involve quite similar tasks, the innovation of your approach is somehow limited. At least, you must discuss the available approaches for installation and highlight the differences.

1.1- Authors' response: Thank you for your comment. The authors agree that explaining the differences between approaches in installation and decommissioning approaches might be beneficial for the paper's justification.

1.2- Changes in manuscript: The section on available approaches for installation was added in line 20, highlighting the differences between installation and decommissioning:

Recent advances in simulation-based planning tools for offshore wind projects have primarily focused on the transport, installation, and operational phases. Approaches such as Monte Carlo simulation, discrete-event simulation, and weather-window analysis are widely used to quantify uncertainty and assess schedule risks. Monte Carlo simulations typically evaluate probabilistic variations in weather or resource availability at an aggregated level, whereas discrete-event simulations model operational sequences and vessel logistics with higher temporal and process detail. Weather-window analysis concentrates on identifying feasible periods for marine operations based on environmental thresholds. While these methods offer a solid foundation for installation planning, their application to decommissioning remains limited. Decommissioning campaigns face higher uncertainty and less studied risks, particularly related to the removal of components where the reverse-installation method is not feasible. Existing models also tend to focus on weather-driven delays and rarely incorporate task-specific process risks such as cutting operations, seabed intervention, or component degradation, which are more prominent during decommissioning.

2) Table 1: Tasks for the substation, cables, monopile etc. are missing. You should list all tasks. If there are too many tasks to be listed here, think about an appendix or some supplementary material. Furthermore, where did you get the data for the other tasks from?

2.1- Authors' response: Thank you for pointing this out. Indeed, space was the original reason why task lists for all components were not included. The authors agree that it is worth including them in the appendix, where they can now be found. Regarding the precedence, the source for the task data was the decommissioning strategy for the Lincs Limited OWF, as detailed by Jalili et al. (2022).

2.2- Changes in manuscript: Tables with the remaining component's activities were added in the Appendix tables A1 to A4, and a clarification on the precedence of the data was added in line 50:

For the dismantling of the OWT, the task list is shown Table 1, together with the target duration for each individual step based on the calculations by Jalili et al. (2022). The tasks for the remaining components can be found in Tables A1 to A4.

3) L. 58: You state that the values and the distribution for the "systematic uncertainty" are taken from Garcia Munoz et al. (2023). However, it seems as if they have just chosen these values without any data foundation. Hence, is there any data or at least industry experience/expert knowledge? If not, the values are just educated guesses making your quantitative results less relevant. Furthermore, Garcia Munoz et al. (2023) investigate installations of OWF. Hence, the question is whether the same approach and the same values can be applied in case of decommissioning. If yes, the question is: how innovative is your work. If no, you should not use the values.

3.1- Authors' response: Thank you for your comment. You are right to point out that the ranges for systematic uncertainty were taken from the work by Garcia Munoz et al. (2023). The numbers were estimated based on expert interviews with industry leaders and researchers with solid experience in the field, this particular value was obtained in an interview with Wiggert M. (2022). For further information on the foundation to the values taken from Garcia Muñoz et al. (2023), please refer to authors' response B.1. For more information on the innovative aspect of the work, please refer to C.1.

3.2- Changes in manuscript: the source of data for the systematic uncertainty range was specified in the same sentence that it is presented in line 73:

Since their impact is broad and hard to quantify precisely, they are modelled by applying a variation to the baseline duration of each activity to account for variations in actual durations that can make actual tasks durations shorter (caused by overestimation in *baseline durations and learning curves) or longer (increases derived from missing process steps, underestimated task durations). Derived from expert judgment, this uncertainty was modelled following a PERT distribution that ranges from a 10% time reduction to an increase of 50% (Wiggert, 2022).*

4) Table 2: Where does this data come from? And how reliable is it? Later, you conduct a parameter study for one of the values. However, as the uncertainty in these values is probably quite high, you should conduct a full sensitivity analysis for all parameters.

4.1- Authors' response: Thank you for your input on this topic. The values presented in Table 2 come from previous work conducted in partnership with IWES (such as the expert interviews by Garcia Munoz et al. in 2023). Regarding the sensitivity analysis, the authors agree that a full sensitivity analysis could be beneficial, therefore all risks in the catalog are now included.

4.2- Changes in manuscript: added the following explanation in line 82:

The research work, built upon the expert interview data gathered by Garcia Munoz et al. (2023) and input from the project partners, resulted in 15 risk events that might occur during OWF decommissioning projects.

Additionally, section 3.3 was expanded to include the expanded sensitivity analyses.

5) Table 2: Are the values the same for all vessel types? What about failures when decommissioning the substation or the cable?

5.1- Authors' response: Thank you for raising the question. The model works by linking a risk (or set of risks) to each task based only on the task description, in that sense, it is independent of the vessel type. On the other hand, the specific parameters for each risk, i.e. minimum delay, most likely delay, and maximum delay, already take into account the most common vessel types for carrying out each task, so in that sense it is accounted for.

Regarding your second point, the possible risks related to the decommissioning of cables, offshore substations or scour protection are currently being developed. Future planned publications derived from this project are expected to cover these stages, using project partner's data to characterize and measure risk related to these operations. An important contribution of this work is to build the framework and describe the methods, so that later risks can be added in the same way once they are properly characterized.

5.2- Changes in manuscript: n.a.

6) Section 2.4: Stating the precise site of the reference wind farm would be nice to make your work more reproducible. I assume that you use the environmental conditions for this site.

6.1- Authors' response: Thank you for the suggestion. Yes, the environmental conditions for this particular location were used. The authors agree that adding the location data helps to increase the replicability of the work.

6.2- Changes in manuscript: the paragraph describing the reference wind farm at the start of section 2.4 was expanded to contain more precise information:

Although similar methods have been developed, this work includes the full scale implementation of the model and analysis of the results. To demonstrate the potential of the methodology, an exemplary analysis of a reference wind farm is thus presented. The reference offshore wind farm selected for this study is an average representation of the German OWFs currently installed in the North Sea. The wind farm comprises 80 turbines, each with a rated power of 5 MW, reflecting the typical scale of wind farms commissioned in the early-to-mid 2010s. Hub heights of 106 m and rotor diameters consistent with 5 MW class turbines are considered. The turbines are mounted on monopile foundations, as the selected site is situated at an average water depth of 30 m. The inter-array cable grid covers a length of 116 km. Eemshaven was selected as the base port for decommissioning operations, resulting in a travelling distance of 54 km from the OWF location (54°00' N, 6°30' E). The wind farm is assumed to have been fully commissioned in 2015, with a planned decommissioning start date on March 1st, 2040, to be able to carry out the majority of the foundation cutting activities during the summer. Vessel speed for the JUV was set at 14.45 km/h.

7) Section 2.4: Crane and vessel speeds are not mentioned, but needed for the simulation.

7.1- Thank you for pointing out this issue. The authors agree to add this missing information to make the paper more reproductible. The transit speed used was 14.45 km/h. Given that the updated task list is based on the work detailed by Jalili et al (2022), the crane speed is no longer a factor that influences task duration, as a fixed value for the task duration is assumed.

7.2- Changes in manuscript: added the following note in line 120 of section 2.4

The sailing speed for the JUV was set at 8 knots, replicating the value used by (Jalili et al., 2022)

8) Section 2.5: It remains unclear, what you mean by “internally tested”. Furthermore, you state that you want to enable cross-validation. However, if you want that other researchers can cross-validate your work, all data must be included (e.g., in the appendix or as supplementary material).

8.1- Authors’ response: Thank you for raising this point. As mentioned in the paper, the software was previously benchmarked with other tools to validate its functionality, and the new implemented method was applied on the already developed tool. Regarding the second part of the comment, modifications have been made to make the work more reproductible, by including all tasks lists as described in the study by Jalili et al. (2022) and making available parameters like exact coordinates of the reference wind farm and vessel speeds.

8.2- Changes in manuscript: Please refer to comments 2.2, 6.2 and 7.2 for the description of changes related to increasing the reproducibility of the work.

9) Section 3 and conclusions: How relevant is the entire case study if it is only done for one reference OWF and based on mainly “guessed” uncertainties? You should somehow show that the results are, at least to some extent, generally valid, i.e., for other sites, OWF and uncertainties.

9.1- Authors’ response: Thank you for the comment. Regarding the basis of the uncertainty values, please see author response B.1. In terms of general validation, a more detailed sensitivity analysis was conducted to check the stability of the model, as well as a benchmark with the results from Jalili et al (2022)

9.2- Changes in manuscript: Please see comment B.2 for changes related to input uncertainty values, and comment 4.2 for changes related to the expansion of the validation aspect.

10) L. 127: 1000 runs/samples (I would not call it iterations) do not seem to be a lot if your failure probabilities are sometimes 10-4.

10.1- Authors’ response: Thank you for your comment. It makes sense to use the terms "samples" instead of "iterations", and the term used in the manuscript is now adjusted accordingly. Regarding the number, 1000 samples are conducted for each task, so the total number of individual delay calculations is closer to 10e6. To increase robustness, the simulations were re-run using 10,000 samples per task.

10.2- Changes in manuscript: replaced the word “iterations” for “samples”, and added the “per task” clarification in line 151:

By repeating the same process to gather 10 thousand samples for each task, the result is no longer a single DoE estimate, but a distribution of possible values.

11) L. 140/141: You state that the results are similar to what can be found in literature, although the weather delays are not yet included. My question is: why should they be similar? I assume that literature values do include weather delays. Does your model, after including weather delays, yield delays that are too high compared to literature values?

11.1- Authors' response: Thank you very much for pointing this out. The objective of mentioning comparison at that point was meant to reflect that the values were already getting closer to those in the literature, even before adding weather delays. Considering your feedback, the comparison was now updated so that it takes place after including all sources of delay, comparing final results with real values from actual decommissioning processes.

11.2- Changes in manuscript: the comparison was shifted after all sources of delay were included, and is now presented in line 187:

The DoE for the whole project considering both sources of delay is estimated at 1.9 DoE / MW) for the median scenario (P50). The results found are comparable to those from completed decommissioning projects as documented in extensive research studies. (Topham and McMillan, 2017).

12) L. 159: Is it correct that you only use the mean results? If yes, why?

12.1- Authors' response: Thank you for inquiring about this. To process the results of the model, they were grouped in P10, P50 and P90, and mean datasets. The figure referenced in line 159 was created using the mean results because it captures the impact of risk event delays, even those with low likelihood that are not present even in P50 or P90 scenarios. Due to space constraints, the histograms showing the spread of P10, P50, and P90 datasets are not included in the paper.

12.2- Changes in manuscript: expanding the explanation of the histogram in line 159:

Figure 3 displays the resulting histogram of mean DoE across all samples. The mean results are chosen to ensure that risk event delays are appropriately characterized in the distribution.

13) L. 160: How did you select the time period? Year 1-25, 2-25 etc. or randomly selected starting days? In the first case, I agree that you have only a few samples. In the latter case, you could generate more samples. On the downside, it would prevent you from always starting the decommissioning in March.

13.1- Authors' response: Thank you for the question. The former alternative was used to select time period, choosing the same starting date each year where data was available. This strategy was chosen to ensure the simulation prioritized carrying out the majority of the foundation cutting activities before the winter months.

13.2- Changes in manuscript: comment added for clarity in line 185:

For the weather delay analysis, the starting date remained constant for March 1st to ensure the simulation prioritized carrying out the majority of the foundation cutting activities before the winter months. *The results of this distribution include the simulation of 43 years of weather data*.

14) L. 169: With average, you mean the "arithmetic mean"? I would be very clear here, as you have already presented the P50 results

14.1- Authors' response: Thank you for the question, and apologies for the oversight regarding the use of this term. Yes, the arithmetic mean is the proper term.

14.2- Changes in manuscript: Clarification in line 197:

In contrast, fig. 7 shows the arithmetic mean for each component's decommissioning campaign duration

15) Figure 6: Is it somehow included that systematic delays lead to further weather delays, e.g., an early systematic delay shifts the project to the winter which increases the weather delay, i.e., are interactions of the uncertainties considered? If yes, how are they counted in Figure 6? If no, why not?

15.1- Authors' response: Thank you for raising the question. Yes, the model simulates the weather delays on top of the operational ones. In that sense, interactions of uncertainties are considered, given that a longer delay derived from systematic uncertainty leads to longer exposure to adverse weather conditions, raising the total duration of the project in a non-linear fashion as seen in figure 6. That is an additional point of innovation of the work.

15.2- Changes in manuscript: n.a.

16) Table 4: This table is neither discussed nor mentioned anywhere.

16.1- Authors' response: Given that the information in table 4 was also included in table 2, the table was removed, inviting the reader to see table 2 to find the risk names.

16.2- Changes in manuscript: adjusted caption of figure 4:

Risk assessment matrix for risk events, numbered according to Table 2.

17) Section 3.3: Currently, you just conduct a parameter study for one parameter. I think that it would be interesting to see this for all parameters and perhaps even combinations of parameters in a real sensitivity analysis, e.g. based on elementary effects. This is especially important, as your uncertainty values are just educated guesses.

17.1- Authors' response: Thank you for the comment. The authors agree that the work would benefit from an expanded sensitivity analysis. To this end, additional risks within the top criticality band in the risk assessment matrix were analyzed to reach a more complete study.

17.2- Changes in manuscript: Extended sensitivity analysis added as Figure 8 in section 3.3, with explanation starting from line 211:

The results of the analysis, summarized in Figure 8, reveal varying levels of sensitivity to changes in severity assumptions. As the studied risks move from 'low' to 'critical', the DoE rises by up to 9% for risks such as vessel collision and contact with foundation, while change remains under 1% for risks related to availability. In the middle, detachment failure shows moderate sensitivity, ranging from 1% to 3%. This wide range of impacts in DoE highlights the importance of reducing risk uncertainty, and underscores the potential benefits of standardizing the tools and strategies used in this step of an OWF's lifecycle.

Extended conclusion reflecting on sensitivity analysis results in line 225:

A sensitivity analysis was conducted to gauge the impact of uncertainty in risk parameters, finding that the model is most sensitive to variations in risks related to vessel collisions and contact with foundation, followed by detachment failures.

18) L. 193: You did not validate your approach, as it has not been tested for any real application. In line 207, you even state yourself that validation is future work.

18.1- Authors' response: Thank you for the comment. You're right to point out that no testing on real application has been conducted. Due to lack of large scale decommissioning projects, a real life application remains unfeasible to the best knowledge of the authors.

18.2- Changes in manuscript: The phrasing of the line 193 has been updated from "validated" to "applied" to avoid confusion.

19) L. 195: How do we know that the parameters are realistic and probability distributions are expert informed? In line 206/207, you write that future work should address topics like the incorporation of industry expert-knowledge and the validation using real data

19.1- Authors' response: Thank you for your question. As mentioned in authors' comment B.1, the values were derived from extensive expert interviews with industry leaders who were involved in offshore wind installation and decommissioning projects. To address the second part of your question, the sentence about expert knowledge in the future work section refers to the expansion and further refinement of the risk catalogue with more conversation with industry experts and data from decommissioning projects.

19.2- Changes in manuscript: Please see comment B2.

20) L. 206/207: In my opinion, these things should be already addressed in the current work. As the methodological innovation of this work is not that pronounced (see for example comment 1 or 3), at least the data source should be unique, i.e., include some real-world data. Currently, this is a major drawback of the work.

20.1- Authors' response: Thank you for sharing your perspective on this topic. For this paper, the authors aimed to use all available data from project partners and literature reviews, although you're right to point out that it is still scarce. The main goal of the work was to develop the methodology and create a framework that is easily adjustable, so that in the future new adaptations can be swiftly added to account for new risks, and changes in vessels, methods, or strategies for offshore wind farm decommissioning.

20.2- Changes in manuscript:

Please refer to comment C2 for changes highlighting the novelty of the work conducted.

Typos etc.:

21) L. 5: Abbreviation "OWF" has not yet been explained.

22) L. 29 and 31: The cited standards are not on the list of references.

23) L. 32/33: "In the context…": The sentence structure is not correct.

24) Figure 1: "Adapted from [50]": What is "[50]"?

25) Table 1: All durations should be given with the same number of significant figures, e.g., 2.0, 0.50 etc. 26) L. 58 and 151: For "Garcia Munoz et al." the year is missing.

27) Table 4: If I am not mistaken, it is just a repetition of Table 2 and can be removed.

28) L. 169: "Fig. 7" not "figure 7".

29) References: Some references seem to be incomplete, e.g., Garcia Munoz et al. Or

29) References: Some references seem to be incomplete, e.g., Shields et al

Authors' response: thank you very much for pointing out these typos and formatting errors, which have now been fixed.

Dear Referee 2,

Thank you very much for the valuable time invested in conducting your thorough review. In this section, we address your comment regarding our manuscript.

RC2: Comment on wes-2025-195, 15 Jan 2026

As reviewer of the Manuscript wes-2025-195 entitled "Risk Assessment Model for Offshore Wind Farm Decommissioning: Analysis of System Uncertainty, Risk Events, and Weather Delays", I have thoroughly reviewed the manuscript, and I would recommend in **accept the manuscript** based on my review. Thank you!

Authors' response: Thank you again for the comment. We sincerely appreciate your positive recommendation of our work.

Changes in manuscript: n.a.