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WIND ENERGY SCIENCE DISCUSSIONS

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Interactive comment

Interactive comment on "A comparison study on jacket substructures for offshore wind turbines based on optimization" *by* Jan Häfele et al.

Jan Häfele et al.

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Dear reviewer,

I thank you for your highly valuable and comprehensive comments on the paper. Probably, it took much time to work on this review, which is really appreciated. Most of the comments have been incorporated directly in a revised version, which is attached to this response. The particular responses are given in the following.

On page 1, lines 12-13, you write: "The approach shows reasonable and promising results, ..." Even for the abstract, this is a little vague. You should say something about how the method seems to succeed in finding global minima with reasonable

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Discussion paper

computational effort and how the method allows for a systematic way of comparing cost and structural performance for optimal designs with different topologies.
I agree, this was too vague. In the revised version, the abstract tells more details at this point.

On page 2 you have a fairly comprehensive literature review. You might also want to consider adding a note about Sandal et al (2018, Marine Structures) where the effect of varying leg distance (functionally equivalent to what you call leg radius) was studied to a certain extent.
When the state of the art was surveyed, this paper was not published yet. Of course, I added it to the literature review. In addition, I added the reference Oest et al. (2018) for the same reason.

On page 4, lines 2-4, you write about the limited effect of changes to eigenfrequencies for the design. This would likely be a larger (if not necessarily critical) issue if the soil and foundation were modeled in more detail than what you have done in this study. Though you correctly note that any design close to resonance would fail the fatigue check, there are practical reasons (related to the numerical behavior of the optimization process) that can make it beneficial to include explicit constraints on the eigenfrequency rather than relying exclusively on the fatigue constraints.
I agree, the reason why it is not required to incorporate eigenfrequency constraints is that the foundation is maintained in any case. I extended the sentence to clarify this.

On page 7, line 10, you define c7. This is constant for all designs and hence has no effect on the optimization process. While it may be instructive to note the existence of such additional costs in practice, for the optimization problem being solved such a term is irrelevant. Hence, you should at the very least state whether this term is part of the implemented computations or if it is merely added at the end of the optimization.

For the optimization process, the constant terms are excluded from the objective function, but I believe that the results are easier to compare, when they show total costs including the constant terms. I added this information in section 5, subsection 5.4.

On page 9, lines 3-4, you describe a "space-filling sampling of the input space." As far as I can see, the details of this sampling is not given either here or in the previous paper about the surrogate modeling. It would be instructive to have some more details here. At the very least, specifically what kind of sampling method are you using?
It is a Latin hypercube sampling, I added this information,. I don't think it is necessary to go deeper into details here, as the focus is not on the surrogate model.

On page 9, lines 13-14, you write "A number of 128 turned out to be a good compromise between accuracy and numerical effort." The details of this was given in previous work, but for the sake of the reader of the present work it would be instructive to give a quick summary of what level of accuracy 128 load cases represents.
I agree. The description was extended.

On page 9, lines 16-17, you write "The output value h_{FLS} *is the most critical fatigue damage among all damage values of the entire jacket..." Considering the results you present later, this does not seem to have been a problem here, but note that using such a constraint (the maximum of a discrete set) could lead to discontinuities that affect your gradients (sensitivities) adversely."*

and

On page 9, lines 28-29, you write "... evaluated with respect to the extreme load of the member, where the highest utilization ratio occurs." Here you have the same potential issue as with the fatigue constraint.

I agree to both comments. Prior to this study, I experimented with the constraints. I know that it is better to incorporate each fatigue damage as a constraint, which is not a big issue and from the numerical point of view the better solution. However, the idea of the surrogate model was to provide a solution for simple and quick fatigue evaluations.

On page 9, line 27, you write "Extreme load parameters are derived by the block maximum method according to Agarwal and Manuel (2010)." This needs a little more detail. Does this include a statistical extrapolation to a 50-year (or n-year) return value?
Yes, it does. I added a short description, what "block maximum" means and how the load extrapolation is performed.

On page 12, lines 18-21, you summarize the different fixed integer design variable combinations you study. Note that term c6 in your previously defined cost function is constant for each of these combinations and hence has no (computational) effect on the individual optimization problems (merely adds a constant term to the solution).
Same as above. The information was added to section 5, subsection 5.4.

On page 12, lines 27-33 [also on page 16, lines 3-6], you describe the behavior of your optimization routines. It would be instructive to plot an example of the convergence. For example cost (and maybe feasibility) vs number of iterations. How many function evaluations is typically involved per iteration? The number of function evaluations (in terms of both objective and constraints) is a more direct measure of computational speed/effort than the number of iterations (and is more easily generalized to different machines). It also says something about how the algorithm is behaving.
I've added a figure (Fig. 2) showing the convergence behavior of both optimization methods and all six subproblems with the OC4 jacket as starting solution. Subsection 5.4 has also been extended.

On pages 13-14, you discuss the properties of the optimal designs. What do the initial designs look like (especially in terms of cost and feasibility with respect to code checks)? How are the optimal designs compared to a "typical" initial design, maybe compared to the OC4? Is the most optimal design topology (N_L, N_X) = (3,3) also the one with the most improvement compared to "initial" designs? How is this for the various other topologies? Obviously, the results here are only meant to illustrate the method, so the specifics of the optimal designs are not so important. However, it would give more insight into the effect of your cost model and your chosen design variables if you explain in more detail what kind of optimal designs your methodology tends to produce, with more clear reference to initial designs. What would you say are the design driving variables?
I agree that these questions are important. From my perspective, Fig.2 also answers most of these questions. Moreover, I addressed these points in the text, subsection 5.4.

On page 14, line 5, you write "Altogether, this is meaningful and not far off from structural designs that are known from practical applications." Do the authors mean that the designs are close to that seen in practical applications or that the costs are? Please elaborate a bit on this statement.
Both the designs (in terms of number of legs, which shows a trend to three-legged jackets) and the costs are close to practical applications. I've modified the sentence to make it clearer.

On page 16, Figure 3, you show a cost breakdown of the optimal designs. Since C_1 and C_5 both depend on the mass, note that the mass is associated with the largest proportion of the cost, but this is not immediately obvious from the figure. From a practical point of view, these two points indeed represent different aspects of the production and installation process, but since both these terms directly contribute to lighter designs, their effects in an optimization context are the same (and in the figure, *these two terms are clearly just scaled versions of each other). While* C_6 *does not impact the optimization directly, its presence in the cost breakdown is justified by how it clearly shows where a significant portion of the difference in total cost between 3- and 4-legged designs come from. The importance of* C_7 *is more questionable, as it just shifts the total cost of each design by a constant value. It may make this cost breakdown more "realistic" in a practical sense, but does it impact how the authors' proposed design approach would be utilized? Were these fixed costs larger or equal in size compared to the other terms, one might conclude that differences between the topologies were negligible and therefore not worth (or to a lesser extent worth) pursuing, but this is not the case here. In any case, this term needs more justification by the authors.*

This is a very good point. I compared the cost function to a pure mass-dependent one at the end of subsection 5.4. The result is that one obtains similar designs (I've added Table 3 with new results), when only the mass is considered as objective. The reason is obvious (as written in the comment). C_1 and C_5 are proportional to mass, C_6 and C_7 do not impact the optimization problem, C_2 and C_3 are in some way proportional to tube dimensions. The only remaining cost term, C_4, is affected by topological variables. However, these variables have a greater impact on structural resistance than on costs.

What is the sensitivity of the optimal designs to each term in the cost function? If possible, what would the optimal designs look like if some/certain terms were neglected? If that is too comprehensive, what is the contribution of each term in the cost function to the gradient of the objective function? Especially those elements of the gradient corresponding to the most design driving variables. Evaluate this at, e.g., the initial design, an intermediate design and close to the optimal design. Comparing terms that are more highly dependent on tube dimensions to the ones that depend more strongly on topology, does the inclusion of the latter terms change the direction of the gradient or does it merely reinforce the steps the algorithm would otherwise take?

Given that C_1, C_2 and C_3 (and hence also C_5) all depend in some way on effective tube dimensions (changing mass, weld volume, outer areas), does the different weighting of the design variables induced by the inclusion of all three terms in the cost function have a significant impact on the optimal design? In other words, since several of the cost terms are in a sense "proportional" (partially or otherwise) to the total mass, how much is changed by the inclusion all of these terms (rather than just the mass)? Clearly, these terms contribute significantly to the actual cost. However, given the correlation with mass, do they have a large effect on the solution of the optimization problem?

Similarly, since many of the design variables controlling topology also enter into the cost terms related to mass, does the inclusion of cost terms entirely related to topology have a significant impact on how these variables are changed by the optimization process? For example, it seems like the costs related to the transition piece is almost completely determined by the number of legs, since the values of Rfoot and ξ are the same (or almost the same) for all design with the same number of legs. One then wonders if the values of Rfoot and ξ are actually determined (or at least significantly affected) by the inclusion of the transition piece cost term, or if similar behavior would be seen without this term. If so (and if this was seen to be a more general result also outside the scope of the present study), this would mean that the cost of the transition piece would not need to be included in the continuous optimization problem, but only added in along with the installation cost as an additional cost related to the number of legs.

Shedding some light on these issues would considerably strengthen the proposed costmodel methodology compared to previous studies using just mass optimization.
As proposed, I computed the sensitivities of each cost function term that is impacted by the design variables, shown in Fig. 4 at the initial design, an intermediate design, and

the optimal design. The results are discussed in subsection 5.4. Also, the evaluation of the mass-dependent approach (described above) sheds light in the cost model methodology. Using the comprehensive cost model does not yield completely different results. However, from my point of view, it is a good result that mass-dependent approaches are actually more accurate than expected.

On page 16, lines 6-7, you write "The number of iterates may be decreased, when using finite differences of the objective function to obtain gradients ..." What exactly do the authors mean here? Having precise analytical gradients of the objective function would generally tend to improve the behavior of optimization routines, since this is less prone to numerical error."
I extended the sentence to make it clearer.

On page 17, lines 24-26, you write "... these approaches assume that the structural topology is always optimal, even in case of significant variations in tube dimensions. However, when combined with the approach presented in this work, state-of-the-art tube dimensioning may be much more powerful." If possible, please comment on how the inclusion of variables related to design topology changes how the structure is optimized compared to pure tube size optimization. I.e. to what extent is the reduction (or change overall) of tube size "replaced" by changes to topology?
It is not easy to quantify this (for instance, because the OC4 jacket is just a derivation of the UpWind jacket, which is a detailed design), but it is replaced to some extent when comparing the topologies. I modified this paragraph to address this point.

Technical corrections
I adopted all suggestions. Thank you for these corrections.

I hope that the revision and my comments are satisfactorily. I would appreciate a recommendation of the reviewer for publication in Wind Energy Science.

Best regards,
Jan Häfele
(on behalf of all authors)

Please also note the supplement to this comment:
https://www.wind-energ-sci-discuss.net/wes-2018-58/wes-2018-58-AC1-supplement.pdf

Interactive comment on Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2018-58, 2018.