

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

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Dear reviewer,

I appreciate your effort for this comprehensive review. I did my best to address all comments and revised the manuscript (see attachment) based on the first revision, which addressed the first reviewer's comments (RC1). The particular responses are given in the following.

Generally, the modeling section should be more descriptive and the language in especially the result section should be revised.

I extended the modeling section, especially the cost modeling part. In addition, more detailed descriptions of modeling (also the structural code checks obtained by surro-

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gate modeling) can be found in another work, which is referenced in the introduction of section 3. The shortcomings of the cost model are discussed in more detail in the benefits and limitations, section 6. I also revised the language.

The choices behind the cost-model should be described in much more detail. Also, the limitations of the cost model should be reflected upon. For instance, local content is a large factor in the current market. Thus, designs can to a certain extent be driven by the locally available production facilities. To name an example, this can affect the number of bays due to crane facilities or painting facilities. While it is fair not to include all the aspects, more important factors should at least be discussed.

See the previous point. Further explanations and discussions are given in sections 3.2 and 6.

The cheapest structures appear to be the simplest structure, i.e. the fewest bays. This is intuitive, as it is wellknown that welding and potentially grinding of jacket structures is very expensive. Thus, the motivation for implementing this framework is lacking, as you get the expected result, as you also mention in the paper. However, if sensitivities to different terms of the cost function were presented, much more insight into the design drivers would be given, and this would add significant value to the paper. E.g., how much would you need to lower the production cost, for instance by robot-welding of X-braces, before we get a different optimal design?

This is correct. A similar point was annotated by the first reviewer (RC1) and I performed a little sensitivity analysis, where the variation of each cost function term was studied with respect to variations in design parameters.

Lastly, since the cost model aims to replace the more used 'overall mass' model, the overall weight of each of the optimized structures should clearly be stated in the result section. The cheapest four-legged jacket is the lightest four-legged jacket. How about the three-legged? This information is lacking. It would have been very convenient to see a minimization of mass optimization compared to the presented results.

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This is also correct. I already performed an optimization loop using a mass-dependent approach and compared the results in the first revision.

You mention that structural optimization is paramount (I do agree, at least in absence of experts) because it provides cost savings “with low effort”. Low effort in execution, yes, but not necessarily in implementation of the method. More focus should be on how easy or difficult it is to implement the proposed optimization method.

I thought about how to address this comment appropriately. What you say, is absolutely correct. However, it is meant that cost savings can be reached just by improving the design process. Of course, it requires effort in implementing the method, but not much economical effort. Therefore, I decided to write “with low economical effort” and added the remark to the benefits and limitations section (sect. 6).

For clarity, I suggest that you directly mention what is meant by ‘intermediate water depths’

I clarified this.

It is true that thousands of simulations are required for verification, but it should be clarified, that it is not needed during conceptual design phases with or without optimization methods.

This was clarified in a footnote.

You do not mention decision by design ‘experts’ until page 3, but number of bays and legs are normally correctly decided by experienced designers. Consider restructuring/rephrasing.

I rephrased the sentence.

This is an assumption. Pile design can be affected by the design of the substructure.

I added “. . .in this approach” to make clear that this is an assumption of this work.

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You should mention why the cost function is scaled with \log_{10} . If you experienced numerical difficulties without the logarithmic scaling, this should also be mentioned.

I added this information.

'The problem incorporates no nonlinear equality constraints'. This sentence can be removed. This is clearly stated in equation 3.

The sentence was removed.

The last sentence in the figure text lacks a 'respectively' or should be rephrased.

I added "respectively" at the end of the sentence.

It is fair to reduce the design space by always having a mudbrace, but real jacket structures do not always have this. The impact on both the structural response and on the manufacturability/costs of having a mudbrace or not should be mentioned.

I fixed the mud brace flag, because it is not a continuous parameter. This is stated now.

You should mention that the actual weights are presented in section 5.3 or the weights should be listed here.

My intention was to split the descriptions of models and parameters. I put a reference to the unit cost values in a footnote.

Generally, the limitations and assumptions of the equations should be made much more visible. While this part is a large step forward in defining the optimization problem as compared with most previous work, the cost function is still quite simplified.

See my first particular response.

You assume that the transport cost is directly dependent on the mass. This is a very large simplification, and effectively makes the additional constraint obsolete at is just an additional factor on $C1$. I fully realize that it may be too complicated to incorporate many of the governing factors, e.g. crane and vessel availability. However, e.g. deck

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space occupied by a three- or four-legged jacket is very different, and this can have a significant impact on the transportation and installation costs.

I'm aware that this is a large simplification. My idea was to consider transport costs to some extent by a simple approach, because it is difficult for me to get realistic cost values here. I talked with people from industry and decided to select the mass as governing factor, as the mass is on the one hand at least partially influencing the transport costs and on the other hand partially related to other measures of the jacket affecting the transport costs (like, for example, deck space occupied by the structure). From the optimization perspective, C_5 is proportional to C_1 and therefore not necessary, I agree (see my comments to the first reviewer). My intention was to separate material and transport costs, which leads to a more realistic cost breakdown in the results. Someone, who has a better model for installation costs may, however, replace C_5 by a more detailed term.

I think that there should be a difference in the cost function for an optimization problem, and the actual costs. There is no need to add fixed costs to the optimization problem.

For the solution of the optimization problem, fixed terms are excluded from the objective function. This information was missing, see my comments to the first reviewer.

It should be clearer that the Efthymiou SCF's are just one way of determining the SCF's, and they are well-known to be quite unprecise. People that are unfamiliar with fatigue design of offshore structures may believe that this is the standard approach, which is most often not the case.

I clarified this.

Not enough details are given. E.g. what are 'appropriate settings' for fmincon?

I removed the term "with appropriate settings" from this sentence. Instead, I improved the beginning of section 5.4, where I describe some settings of the optimization methods. Additionally, the convergence behavior is shown and discussed in the revised

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manuscript. I believe that this strengthens the (mathematical) optimization aspect.

Can you include the overall mass of the jacket in this table? This is important to compare the results from standard 'mass minimization' to your 'cost minimization'. It looks like the cheapest jackets are also the lightest jackets. At least, the cheapest 4-legged jacket is the lightest jacket. It could be very interesting to see if the cheapest 3-legged jacket is also the lightest 3-legged jacket.

The row with overall masses was added to the table. As C_1 (and C_5) are proportional to mass, the overall masses were given in Fig. 3. However, I agree that this was not very obvious. I discussed it in the text in more detail.

I hope that the revision and my comments are to your satisfaction and would appreciate a recommendation for publishing the revised manuscript 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-AC2-supplement.pdf>

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

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