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# Review of “Optimization of a dynamic absorber with nonlinear stiffness and damping for the vibration control of a floating offshore wind turbine toy model”

© **Pierre-Olivier MATTEI**<sup>1</sup>, © **Renaud CÔTE**<sup>1</sup>, © **Kevin DEKEMELE**<sup>2,R</sup>, and © **Olivier THOMAS**<sup>3,E</sup>

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<sup>R</sup> Reviewer

<sup>E</sup> Editor

## Review of version 1

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**Authors** The authors are very grateful to the reviewers’ critical comments and thoughtful suggestions. Based on the comments and suggestions, the authors have made careful modifications on the original manuscript. The following are the responses to the reviewers’ comments.

### Reviewer 1 (Anonymous)

**Reviewer** The paper addresses important and acute topic – optimization of the nonlinear energy sinks in order to avoid the disastrous detached resonance branches. The authors offer very reasonable optimization strategy (albeit its computational price remains unclear). It would be instrumental to understand whether such dangerous branched might appear beyond the used 1:1 resonance assumption – some of them can remain unattended. My other major concern is a language – the grammar in many places is poor, and professional help is definitely required to make the paper readable. I recommend the revision with subsequent additional review.

**Authors** By construction, we have never observed detached resonances when solving numerical equations once nonlinear damping have been optimized to cancel it. The 1:1 resonance assumption have been simply used to construct analytical solution. This analytical solution have been used to verify that accounting for nonlinear damping does not change the very essence of Targeted Energy Transfer. The fact that obtained values for the optimal nonlinear damping are almost identical for the analytical and numerical solution is an indication that the 1:1 resonance assumption is suffices to describe well Targeted Energy Transfer. The computational cost of the optimization had been indicated in the text and the English grammar had been carefully revised.

### Reviewer 2 (Kevin DEKEMELE)

**Reviewer** This paper studies a nonlinear energy sink for wind turbines. In particular, the goal is to cancel the detrimental ‘detached’ resonance curve and as such make a nonlinear energy sink efficient over a large energy range. The authors attribute previous work that achieve this by proposing a nonlinear damping. The research is well done, and the paper deserve publication after some modifications.

**Reviewer** The authors should better stress how their paper is novel compared to the previous work that

proposed this damping.

- Authors** The main originality have been proposed in the last lines of the abstract and conclusion.
- Reviewer** The context of this paper is a wind turbine. However, except for the introduction, the discussion is quite generic. Why stress the wind turbine application? Does it come in certain forcing terms?
- Authors** We agree with this remark, but as this study deals with parameter and forcing range characteristic of FOWT and because it addresses an important and actual problem of renewable energy we wanted to keep this application.
- Reviewer** Content: how much control does the mechanism of "Andersen 2012" allow in the variable  $v_N$ , the nonlinear damping? If I look at that paper, it seems that  $v_N$ , and the nonlinear coefficient of the stiffness are not independent. How does this affect performance limits? Are there other mechanisms that allow separate optimization of the  $v_N$  and nonlinear spring?
- Authors** All NES parameters are independent, we added the figure 1 to show it more clearly. While not obvious because we explain the nonlinear damping coefficient as a product between the linear damping coefficient and a multiplicative factor, the two parameters are independent. This way to proceed allows very interesting results such as the particular value of that factor  $v_N^0 = 3/2K_N/(1 - \delta_N)$  that keep the SIM shape independent of the linear damping.
- Reviewer** Section 3: Equation 6 takes into account 0 to maximal forcing amplitude. A minimal forcing amplitude would also make sense, as a small amount of vibrations is acceptable. Would the result of the optimization in 3.2 be different if a  $A_{min}$  is incorporated into the optimization functional?
- Authors** We have mistaken, we have not precised that the minimal value can not be zero, as without forcing, no motion is been possible. This is corrected.
- Reviewer** Section 4: An analytical 'frequency response' is obtained. Could you obtain the same result from this analytical plot as the 'brute force' methods of section 3 ? This would save a lot of numerical optimization steps.
- Authors** The results obtained are comparable over the whole frequency range except in the vicinity of the main resonance where they significantly differ. This is shown in the new figure 9 where we compare analytical and numerical FRF.
- Reviewer** Section 4: Figure 5 only shows the stable parts of the frequency response. Why are the unstable not included? Unstable does not mean there is an infinite amplitude, there is just no fixed value for the amplitude, it is quasi-periodic/SMR. If the authors include them, the detached resonance curve would be clear. Now this is not the case, yet this seems to be a very important aspect of this paper.
- Authors** This figure becomes figure 6 and we added the unstable branches. Please note that the curves had been normalized by  $\sqrt{2}$  to keep the same amplitude as the numerical values (+3 dB shift).
- Reviewer** Section 4: An important figure is missing, in where a frequency response obtained from simulation is compared to the analytical frequency response. This should be done in section 5 to compare an optimal configuration numerically and analytically.
- Authors** This comparison had been added in section 4.3, figure 9.
- Reviewer** Language: Sometimes 'optimisation' is used, other times 'optimization'. The first one is British English, the other one American. Please choose one of the two.  
Abstract & intro: p2 End of 4th sentence first paragraph: 'by the community and industrial' => 'by the academia and industry' ?  
In the abstract you call the system a 'mass-stiffener-damper' and in p3 3rd paragraph 'mass-spring-damper'. One should be chosen (mass-spring-damper) is more common.  
p3 5rd paragraph: "2 passes procedure" => "2 step procedure", in the same paragraph 'pass' should be replaced by 'step'
- Authors** All suggestions have been taken into account.

**Reviewer 3 (Anonymous)**

**Reviewer** This paper focus on the optimisation of a NES (cubic nonlinearity and nonlinear damping). State of the art is very well presented and analysed. The paper is detailed and educational, authors have done a very good job. The main contribution is about the influence of the nonlinear damping, with very interesting conclusion.

I will recommend the paper for publication in international Journal of Theoretical, Computational and Applied Mechanics (JTCAM), if all the following corrections are incorporated in the manuscript.

**Reviewer** The entire manuscript should be thoroughly proofread, because it contains some typing mistakes, and References will be checked. For example: "which is the best way yo dissipated", "NES constituted by of a mass", on the references replace "pages=" by "pp.", remove [SW]...

**Authors** We corrected the mistakes, Please note that [SW] can not be removed as it depends on the bibliographic style chosen by JTCAM

**Reviewer** What seems a minor matter, but one that distinguishes a very well-presented paper from a moderately good one, is the use of 3rd person (impersonal). Could you remove the excessive "we" from the text, and replace them by impersonal.

**Authors** It has been done.

**Reviewer** Another detail, a stiffness is by definition linear, but "nonlinear stiffness" is not correct. Replace "nonlinear stiffness" by "nonlinear behavior", or "cubic nonlinearity", or other wording.

**Authors** We prefer to keep nonlinear stiffness as this wording is usual in the literature (see eg the book "The Duffing Equation" by I. Kovacic and M. J. Brennan, 2011, John Wiley & Sons.)

**Reviewer** On Section 2, can you add a figure with a schematic of the model to make the link with the Floating Offshore Wind Turbine.

**Authors** We added figure 1 that depicts the model.

**Reviewer** The use of classical tool (fixed point and SIM), need to add references at the beginning of section 4.1 and 4.2. It's important for the reader, in order to show the contribution.

**Authors** We added a reference for the calculation of fixed points, SIM and principle of CX-A method at the beginning of paragraphs 4.1 and 4.2.

**Editor's assessment (Olivier THOMAS)**

The paper proposes a method to optimize a nonlinear passive vibration absorber called a NES (Nonlinear Energy Sink) in the literature. This kind of absorber received a great amount of attention since the first publication of the concept in 2001. Because of its strongly (and intentional) nonlinear features and dynamic response, its optimization is often intricate and no general results are available as it would be expected for a linear system. In particular, this kind of system can produce detached resonances, meaning that some high amplitude vibratory behaviour can be observed in practice. The present paper proposes a method to optimize a NES in order to avoid and master possible detached resonance using a nonlinear viscous damping element.

The paper was reviewed by three independent experts in the field of NES, who agreed on the quality of the study provided minor revisions. After this first round, the editor readily accepted the paper for publication. However, just before final typesetting, the authors noticed a mistake in some of the computations. In accordance with the editor, they proposed a corrected version of the article. Before finally accepting the article for publication, the editor asked a new review from one of the reviewers, which was strongly positive.

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