Response to Referee Comment #1, wes-2024-83, 16 Sep 2024

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We sincerely thank the referee for reviewing our manuscript and providing constructive feedback to improve our manuscript. We have revised the manuscript accordingly. Below are the original comments from RC#1 in black and our responses in blue.

In this paper, the authors present a system to measure the operational deflection shapes (ODSs) of the ring gear of a gearbox using fiber optic strain sensors. The shapes then have application to the measurement of mechanical torque in the gearbox, currently not typically available, for the purposes of remaining useful life estimation or even gearbox condition monitoring. In general, the article is interesting and well-written. I have provided the following comments and some grammatical suggestions at the end.

In the Abstract, gear tooth root strain gauge measurements are described in comparison to the fiber optic ODS and torque measurements. Although I generally understand the comparison, I think most readers might be a bit confused here as Abstract speaks to torque measurement up to this point. It might be simpler just to delete "Compared to conventional gear tooth root strain gauge measurements," as the deflections measured by them relate to the sun-planet and/or planet-ring tooth load distributions rather than the deflection of the ring gear from torque. Nothing is lost from the sentence by deleting this and it is much simpler and direct. Additionally, I might recommend changing "research the deformations caused by gear mesh events" to "measure the deformations caused by planet gear passage events". The primary content measured by the fiber optics is the planet passage, as evidenced by Figure 8, not the gear meshing that occurs at a much higher frequency.

We agree to delete "Compared to conventional gear tooth root strain measurements" as the resulting sentence conveys the desired meaning in a more direct way. We also agree that "planet gear passage event" is more precise than "gear mesh events" because several teeth engage in meshing actions throughout the deformation peaks shown in Figure 8.

Introduction, line 50: A small point, but I recommend deleting "substantial" an just saying "impact on the main frame, tower and foundation". The implication is that tower-top mass (and specially from the drivetrain) substantially impacts these costs - unless a reference can be given, we have found that the rotor loads are the main driver. Reducing drivetrain mass was not a particularly strong driver as described in https://www.sciencedirect.com/science/article/pii/S0306261923006360.

We agree that the use of "substantial" is probably disproportionate. We wanted to convey that torque is the main driver for sizing the drivetrain (line 49), and the weight of the drivetrain has a cascading effect on the main frame, tower, and foundation.

Introduction, line 62: Another minor suggestion - an additional reference might be nice at the end of this paragraph. I recommend https://doi.org/10.5194/wes-7-387-2022.

Citation added, indeed the trend to replace roller element bearings with journal bearings is discussed in the article "Wind turbine drivetrains: state-of-the-art technologies and future development trends" from Nejad et al..

Introduction, line 70: Unmentioned here is that estimating mechanical torque from electrical currents also contains inherent uncertainties in the converter, generator, and gearbox efficiencies. I can't think of a reference that quantifies this, but I believe this to be generally accepted. It would be nice to add this point here. In terms of the impact of load on at least bearing fatigue, since the fatigue is roughly proportional to the cube of the load, then even a 10% error results in a 33% error in fatigue - this even in normal operating conditions.

I will admit I don't know wether this uncertainty or the omission of torque fluctuations in dynamic events is more important, but I wonder.

We agree and have added the sentence "Even in normal operation, relatively large errors are expected when using generator currents because the power losses in the generator and the gearbox vary with torque and other operating conditions and are generally unknown". Currently, we are unable to anticipate which of these two factors, measurement uncertainty or the lack of information from torque fluctuations, is more important.

Introduction, lines 109-111: Initially, when I read the stated third contribution of the paper itl, at least to me, read as the most important contribution. So much so that I was going to suggest it is important enough to merit being "elevated" as part of the Title. After reading the remainder of the article; however, it feels like this bullet point is a bit of an overstatement. Certainly such measurements could be used in a framework, but the framework itself is only mentioned rather than being proposed (and certainly described) in this paper. I'm not quire sure what I'd recommend here - to leave this as a third contribution and "soften" the contribution, or put this in text as the usefulness of such a system and analysis. I simply ask the authors to reconsider how this third bullet is written with respect to how the article itself is written.

This comment aligns with Referee #2, and we agree that the proposed framework to track operation deflection shapes over time for fault detection is more a proposed potential application of the results of the paper rather than an actual contribution of the paper. We have reworded the contributions accordingly. The conclusions section and the abstract have also been slightly modified to emphasize this remark.

Section 2, line 150: Could you add a short mention of what Ab is? I take it that the periodic and structural modes are Aper and Asys, but I don't see mention of what Ab represents.

We have added definitions of the variables A^{sys}, A^{b} and A^{per} to the revised manuscript. As this comment aligns with another from Referee #2 we have modified this section with an explicit explanation of how the influence of the unknown input $u(t)$ is modeled inside the extended system matrix $\bar{A}$.

Section 4.1, line 265: A similar comment as before regarding "mesh events versus "planet passage events. Here in this line I believe the phenomenon being described is better represented as "mesh forces as each planet passes the measurement point on the ring gear occur at different times. That is, in Figure 8a, one only really sees 7P content, not 83P content.

We agree and have reworded several sentences in this section to clarify that within each stage, deformations caused by the mesh forces as the planets pass close to the measurement locations occur at different times. Additionally, each stage has a different spacing between deformations because each stage has a different planet carrier rotational frequency.

Section 4.1, line 279: Similar to the Abstract, I recommend "gear mesh events be changed to "planet passage events.

Corrected.

Figure 10: In an earlier figure, red and blue were used as 2 different sensors, but here the two colors are not labeled. Could a legend be added here, or other description? I don't think the colors have the same meaning.

Indeed, blue and red don't relate to the sensor number as in Figure 8. The mode shapes involve all sensors on the different stages, and an explanation has been added to the caption to clarify how the mode shapes are represented. Each shape is defined by two conjugate vectors with a size equal to the number of sensors; the two different colors in the plot represent the real and imaginary parts. Green and orange have been chosen to differentiate from Figure 8. This comment has been applied to the rest of the plots showing mode shapes in the revised manuscript.

Figure 13 and associated text: I'll admit I don't see much point in this figure, but maybe I'm really missing it? Maybe just say that the modules were not found to be time variant. Or maybe the relative magnitudes of each mode could be listed in Table 5 - I think the main point is that mode 2 representing 7P has the highest magnitude, just reinforcing that the signals are primarily comprised of this as Figure 8a already shows.

We agree and have erased this figure from the revised manuscript. As suggested, we have added the average values of state modulus to Table 5.

Minor grammatical comments:

Line 44: I believe the year for "Stehly et al." is missing. It would typically look like "Stehly et al. (2016)" or "Stehly et al. (2021)".

Corrected.

Line 86: A comma is needed here "... or a shaker, EMA relies...". Similarly on line 90: "... with OMA, because in OMA...".

Corrected.

Line 115: I might suggest "described" instead of "shown" here.

Corrected.

Line 158: I believe this inline citation style should be "Verhaegen and Verdult (2007)".

Corrected.

Line 174: I believe "it" is missing and should read, "it has a sustained oscillation".

Corrected to "the system exhibits a sustained oscillation" to avoid confusion.

Line 176: I believe the "i" in "ith" should be italicized.

Corrected.

Line 243: "barking" torque should be "braking" torque.

Corrected.

In the References, Veers et al. 2022 can be updated from https://doi.org/10.5194/wes-2022-32 to https://doi.org/10.5194/wes-8-1071-2023.

Corrected.

Again, we thank the reviewer for the positive feedback.