General comments:

"Floating wind turbine motions signature in the far-wake spectral content – A wind tunnel experiment" addresses the important question of the relationship between floating offshore wind turbine (FOWT) motions and wake meandering. Using an actuator disc model subject to idealized and realistic motions in a wind tunnel, the spectral signature of the wake is probed and related to the frequencies of the imposed motions. The authors find that the signature of strong, idealized motions appear in the wake, while more realistic motions do not have an apparent impact. While these findings present a step towards understanding wake meandering for FOWTs, the authors could have elaborated on their results to show what impacts realistic wave motions do have on the wake, even if the expected spectral signature was not observed. In addition, some of the details of both the methods and results were not clearly presented. Please see below for specific comments and some minor technical corrections.

Specific comments:

1. Page 2, lines 44-46: Can you elaborate on how your work compares with Belvasi et al. (2022)? For example, page 16, line 288 states that none of the pitch regimes tested in the current study leave a detectable trace at 8*D*, but figure 13 of Belvasi et al. (2022) shows a clear peak for the case that is similar to P5 in the current study.
2. Page 3, lines 61-62: Camp and Cal (2016) do not show wake spectra in their study. Is there evidence showing that the rotational frequency signature does not persist into the far wake?
3. Page 4, lines 100-101: What does the velocity scaling factor of 3.2 correspond to in terms of the full-scale and model velocities used?
4. Page 5, lines 121-129: The use of "first order" and "second order" is not clear in this context.
5. Page 8, figure 3: Why don't the normalized velocity profiles reach 1 at the top of the plot?
6. Page 8, line 175: Does the proposed range of 0.08 to 0.12 come from the VDI Guideline? Please clarify.
7. Page 8, lines 176-177: Why is the flow slightly skewed to the left?
8. Page 8, line 181-182: The theoretical data is for $z_0 = 0.01$ m, which is still multiple orders of magnitude larger than the roughness length calculated in this experiment ($z_0 = 5.5 \times 10^{-6}$ m). How is this comparison justified?
9. Page 10, figure 6: There are no blue dots (shown in the legend) plotted in the rightmost panel of this figure.
10. Pages 10-11, lines 209-210: This sentence is not very convincing as figure 7 shows only one point from Orcaflex below (or at) the cutoff frequency of 0.1 for each plot.
11. Page 12, lines 227-229: Why do the OL and OR points have more energy for the moving turbine than the fixed turbine (i.e., $\varphi_{max} > 0$), and why does this signify the lateral limits of the wake?
12. Page 13, lines 235-237: It does not appear that the results of this study are consistent with those from Li et al. (2021) which show maximum receptivity for reduced frequencies between 0.2 and 0.3. In the current study, the peak frequencies are all above 0.3. Were any higher frequencies tested? Would φ_{max} be expected to continue to increase with frequency?
13. Page 15, line 270: Does the realistic motion affect the total energy of the wake?
14. Page 16, figure 12: This figure is very unclear. Are three cases shown on each plot? If so, different symbols/colors should be used to differentiate them.
15. Page 17, lines 305-306: Does the realistic motion have any impact on the wake? Can you compare, for example, a time series of the wake centerline? Or could you refer to previous studies to put this finding in context?

Technical corrections:

1. Page 1, line 22: A word is missing here: "…both constructive and destructive *interference* is possible…"
2. Page 5, line 123: If figure 7 is the first figure referenced, maybe it should be figure 1.
3. Page 7, line 168-169: Make sure you are consistent with your use of φ_{max} vs. ϕ_{max}.
4. Figures 9, 10, 11, and 12: The labels on these figures should read φ_{max} rather than φ.