Review of the paper entitled: “Load Reduction for Wind Turbines: an Output Constrained, Subspace Predictive Repetitive Control Approach”, by Liu Y. et al.

General comment:

The paper deals with a novel control strategy aimed at reducing the oscillating loads on rotor blades.

The algorithm is based on a predictive control implementing constraints on a particular output. The formulation is based on techniques already present in literature (receding horizon control, recursive identification, constrained controls), that are here used in synergy for generating a particular control action.

The constraint is used to trigger the control when a selected output (here out-of-plane moment) exceed a selected threshold. In the end, the control is able to reduce (not cancel) the loads only when it is necessary, i.e., when the loading is too high. Because of that, the impact of the control on actuator duty cycle is extremely limited.

The performance of the proposed control is compared with that of a widespread technique aimed similarly at reducing oscillating loads, that is the standard IPC based on the Coleman transformation. In this regard, I have to say that the two controllers have similar but substantially different scopes. Standard IPCs can target and remove completely the oscillating loads thanks to the integral feedback. To do so, the use of the actuators results high. The proposed control, on the other side, acts so as to “simply” limit the oscillations under a desired threshold. This latter represents an extremely interesting control philosophy, which may find applicability outside the goal of the present work.

Although the comparison performed in the work (cSPRC vs MBC-IPC) is clearly important, I hope that the Authors in an amended version of the manuscript may acknowledge that the scope of the two controllers is “similar but different”, and that, eventually, cSPRC is not the “winner” but rather a good alternative to MBC-IPC with specific pros and cons.

The paper is well-written and represents a good piece of research. I recommend the acceptance of the paper with minor corrections. The suggestions are all listed here in the following. The list in “Minor comments” refers to corrections that can be regarded as “minor” but worthy of a particular attention.

Minor comments:

1. RLS problem requires a persistent excitation (as also reported by Authors, see lines 168-169). This point deserves additional explanation. I may pose some questions to emphasize the issues which could be connected to that:
    a. How is this excitation practically imposed? Pitch actuation? Atmospheric turbulence?
        i. If the persistent excitation is achieved by pitch, this will have a great impact on ADC, which will be detrimental in light of the main aim of the work (see line 8 “actuator activities can be significantly reduced”). Moreover, what is here the role of the actuator bandwidth? Is it possible to have a pitch-driven persistent excitation within the frequency band of interest?
        ii. In case of excitation provided by turbulence, can it be considered “persistent” in the frequency band of interest? What one can say for very low turbulence inflow typical of offshore winds?
    b. Since the system is periodic and made by three rotating blades, the requirement of persistent excitation is to be carefully considered. One cannot simply check the excitation

made by a collective pitch motion, but also in the other two component (sin and cos, see the Coleman transformation).

i. If my doubt is well-founded, this can have an important impact on loads, as the rotor will be always subject to an excitation which could modify nodding and yawing rotor moments.

2. Line 230: the whole methodology is attractive as it is fully automated and adaptive. Apparently, it does not need a dedicated tuning campaign, as it can be expected for standard PID controller. Unfortunately, once the constraint is enforced in the problem, one needs to minimize a cost function depending on two user-defined matrices Q and R. Some indications for the selection of these matrices are important. Consider also that the number of entries in both Q and R can be high even if one assumes them diagonal.
3. Section 4.1: Are the pitch actuator dynamics considered in the model? If not, is it possible to infer how much the presence of actuator dynamics may affect the outputs for all analyzed controls, especially at the multiples of the rotor frequency (if considered in the controllers)?
4. All in all, I may imagine that one may force MBC-IPC (even if integral feedback is present) to have limited ADC, through the imposition of upper boundaries in the cosine and sine control variables (the signals before the anti-Coleman transformation) and through suitable anti-windup filters. If Authors share my opinion, they could note it somewhere in the text (e.g., introduction).

Technical corrections:

1. Abstract. I would smooth the sentence "… the current IPC design not economically viable". It is hard to demonstrate that IPC, considering the entire turbine, is not economically viable.
2. Line 55: change "to the best of author's knowledge" in "to the best of authors' knowledge".
3. Line 62: There is the repetition of the author's name inside and outside the parenthesis. Probably using \citet in latex may give a better output.
4. Figure 2: I believe that the arrow between the "10 MW Wind Turbine (Plant)" and the "Basis functions" is there to emphasize the fact that the sinusoidal are based on the actual azimuth angle coming from the plant. If so, the symbol φ can be associated to the arrow itself.
5. Line 144: change one δu in δx
6. Line 224: The meaning of $\bar{T}$ is not defined. Could it be $\bar{Y}$? Moreover, in many places it is written that the constraint is defined according to the IEC standards. Those sentences seem a bit vague. How is it practically defined?
7. Section 4.2.1: During the simulation, does the wake travel from left to right as it can be inferred by Figure 3? If so, this should be specified in the text to ease the comprehension.
8. Figure 4: could it be interesting to show also in this case the yawing moment?
9. Figure 4: the caption reads "The steady-state values of MOoP have been removed". This is ok for clarity of the picture, but, at the same time, I may say that in a real environment this cannot be done and, in turn, one has to define variable bounds. In fact, the limitation should be different for different wind speed (or even TI). This should be noted during the mathematical treatment as it could represent an additional complexity in the control scheme.
10. Section 4.2.2: It is not clear whether within the turbulent flow also non-null shear layer is considered.
11. Conclusion: I would expect a comment on multifrequency IPC. Is it possible to easily extend the cSPRC algorithm to multifrequency IPC? Such an extension may be useful for reducing loads in the fixed system, for example the 3p loads at hub, which can be mitigated through 2p and/or 4p IPC.