Wind Energ. Sci. Discuss.,
doi:10.5194/wes-2015-3-AC2, 2016

eawe european academy of wind energy
Open Access WIND ENERGY SCIENCE DISCUSSIONS

Interactive comment on "Periodic stability analysis of wind turbines operating in turbulent wind conditions" *by* Riccardo Riva et al.

Riccardo Riva et al.

carlo.bottasso@tum.de

Received and published: 23 June 2016

We thank the reviewers for the detailed analysis of our work and the constructive inputs, comments and suggested improvements. A revised version of the manuscript has been prepared taking into account the reviewers' recommendations. A list of point-by-point replies to the reviewers' comments is reported in the following.

Printer-friendly version

Discussion paper

Reviewer *The introduction section is well documented and references the right papers. Clearly, the work presented in this paper builds on the many of the past contributions of the authors to this topic. I believe the introduction should mention in a very explicit manner what the new contributions of this work are. What is incremental and what is new and different. This should allow the paper to have a sharper focus, and also to be shortened. The paper is not a review paper, but many sections of it read like a review paper.*

Authors As noticed by the reviewer, the paper is built on some past contributions of ours on the same topic. The differences with respect to the past contributions are actually quite clearly stated in the introduction: "Goal of the present paper is to expand and formulate in detail the PARMAX-based method originally proposed by [ref:Bottasso-Cacciola-Riva, 2014]. A second goal of this paper is to compare the PARMAX method with the periodic operational modal analysis (POMA) [ref:Allen-Chauhan-Hansen, 2011], which is taken here to represent the accepted state-of-the-art for the stability analysis of wind turbines operating in turbulent wind conditions."

Regarding a possible shortening of the paper, we have carefully considered this opportunity during the preparation of our paper. Eventually, we decided not to reduce further the document, because of two main reasons. First, we consider it useful to have all related theory in one single self-contained paper. In fact, a reader not familiar with LTP systems would have to find the necessary background information by reading several papers, written using different symbols and nomenclature. We are not aware of references that cover all this material in a single unified manner, a lack that makes it hard for readers to enter into the rather complex field of periodic systems. We remark that our point of view seems

Printer-friendly version

Discussion paper

to be fully shared by the first reviewer, who stated: “Although lengthy, it is useful to have the entire theory in the appendix”. Second, each theoretical aspect treated in the manuscript is necessary for a proper understanding of the results. In fact, it is hard to understand the comparison between POMA and PARMAX without a good knowledge of harmonic transfer function theory, which is in turn strictly related to the basic behavior of LTP systems. It is our opinion that having the entire chain of notions into one single paper will dramatically ease the comprehension of our work. Moreover, the fact the all theoretical aspects were grouped in the appendix, allows a reader familiar with the topic to read the paper without interruptions. From this point of view, we believe that our paper strikes a good balance between contrasting requirements: on the one hand, it is comprehensive and self-contained, something that should be appreciated by the novice reader; on the other, the new material is clearly separated from the background grouped in the appendices, so that an experienced reader can skip the latter and concentrate on the former.

Reviewer *The authors mention that the approach should satisfy the following criteria: “First, one would like to account with complete rigor for the periodicity of such systems, without introducing approximations of unknown effects. Second, one would like to formulate the analysis so that it is system-independent...”. The first requirement is used to justify the use of Floquet theory. Unfortunately, the behavior of wind turbines is not fully periodic, nor is it linear. These limitations should be made more explicit from the beginning of the paper.*

Authors We agree with the reviewer in general, although we would like to stress that, at least in the analyzed scenarios, results showed that nonlinearities and rotor speed variations did not play prominent roles. Indeed, we added the following sentences at the end of Sec.2.1 “It should be remarked that the present approach

does not consider the effects of nonlinearities nor of rotor speed variations induced by turbulence. The former potential problem can be checked a posteriori by looking at the matching between predicted and measured quantities. The latter can be partially solved by averaging the rotor speed over the analyzed time window. Typically, because of the large inertia of wind turbine rotors, angular speed variations are not expected to be highly significant, especially within the short time windows required by the proposed approach." The very same concept was further stressed in the result section (cf. Sec. 5.1).

Reviewer *The appendices could be shortened, eliminating textbook material. In summary, the paper is well written and presents interesting material. The paper should be shortened, focusing more directly on the new contributions of this paper.*

Authors Please see the reply to the first question of the second reviewer.

Please also note the supplement to this comment:
http://www.wind-energ-sci-discuss.net/wes-2015-3/wes-2015-3-AC2-supplement.pdf

Interactive comment on Wind Energ. Sci. Discuss., doi:10.5194/wes-2015-3, 2016.

Printer-friendly version

Discussion paper