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WIND ENERGY SCIENCE DISCUSSIONS

Interactive comment on "Updating BEM models with 3D rotor CFD data" *by* Marc S. Schneider et al.

Anonymous Referee #1

Received and published: 17 January 2017

Title: Updating BEM models with 3D rotor CFD data Authors: M. Schneider et al.

The manuscript is focused on the extraction of three-dimensional airfoil data from blade-resolved RANS analyses of an in-house code. The authors have conducted a great deal of work with respect to the number of CFD RANS analyses, extraction of airfoil tables from RANS results, and comparison of sectional blade forces among results obtained by RANS, inverse BEM, and standard BEM methods. The primary conclusion of the work is that airfoil tables (that account for 3D effects) appear to be dependent on the blade pitch angle when extracted with an inverse BEM methodology that does not include any tip or inboard stall correction. In comparison to a TORQUE paper presented by the authors, the present manuscript also includes an introductory treatment of airfoil data extraction for unsteady cases.

General comments:

The study of three-dimensional effects on turbine blades is an important area of research. In general, the Prandtl/Glauert tip correction (or variants thereof) are used in conjunction with a root loss model and accounting for stall delay effects at the inboard parts of the blade. Contrary to this approach, several researchers have addressed the problem by focusing exclusively on adjusting airfoil data without any tip corrections. Here the reviewer would like to point the authors to literature in the United States on the NREL Phase VI rotor, e.g. the works of Tangler (NREL/CP-500-31243, 2002) and that of Gerber and Tangler published in the ASME Journal of Solar Energy Engineering. Another groundbreaking work in this area is that of Guntur and Sorensen (TORQUE 2012) that the authors are aware of.

While the former method is widely used in the wind energy community for rotor design, the latter method lacks predictive capability due to the fact that airfoil data are extracted from higher-fidelity analyses, e.g. RANS simulations, thus allowing efficient BEM analyses only 'after-the-fact' instead of being an accurate predictive tool for rotor design. The primary purpose of airfoil data extraction methods is to gain further physical insight into a given rotor design and/or to suggest modifications to existing tip corrections towards a more accurate predictive design tool. One has to be cautious here with generalized statements and conclusions because extracted airfoil data are very specific to one particular flow condition on a given turbine blade design.

It is hence not surprising that the extracted airfoil tables may not be applicable to any other rotor designs or even flow conditions (e.g. different blade tip pitch angle) as noted by the authors as their primary conclusion; it is further not surprising that the inverse BEM method agrees very well with the RANS analyses when it is (in reality) a simple verification study that the inverse operations are computed correctly. The authors' main conclusion is that "... 3D rotor polars depend on the blade pitch angle", a result which is (again) really not surprising as the polar extraction method described in section 3.3 does not consider a tip correction which is (in general) a function of the blade flow angle (and intrinsically includes the effect of blade tip pitch) as is well known to the wind energy community.

As such the results are only of FAIR scientific significance because, in the reviewer's opinion, they only represent a minor contribution to scientific progress in BEM modeling. Furthermore, there are no substantial new concepts, ideas, and methods brought forward by the authors. On the contrary, the description of BEM can be simply referenced by available textbooks (Manwell or Burton) as is the description of the extraction methods documented in Guntur and Sorensen. There are no overall hypotheses, questions asked, or objectives for this work. The reviewer cannot apprehend the actual purpose/objective of this work and how the community can use the results obtained. The scientific quality is also only FAIR because of several statements on rotor induced flow that are erroneous, see specific comments below. The presentation quality is GOOD overall with minor suggestions to rewording.

In summary, the reviewer thinks that this work is of conference proceedings quality but does not merit publication in WES as a selected work from the TORQUE 2016 conference. The reviewer hopes that the authors understand the many technical comments as constructive criticism, and the reviewer would hope to see further work by the authors at future technical meetings.

Specific comments and technical corrections:

Page 1, Line 8: Double "the the . . ."

Page 1, Line 15: A better word choice may be "attempted" (rather than "tried")

Page 1, Line 23: "models" and not "model"

Page 2, Figure 1: Not sure if this figure is needed as it does not provide any new (or more clear) information as presented in the narrative.

Pages 3-4: These are very basic BEM relations that can be simply referenced from Hansen, Burton, or Manwell. No need to go through this.

Page 4, Line 17: Is 2014 the correct publication year ?

Pages 5-6: Again, section 2.3 is documented in earlier work [Guntur and Sorensen]. The reviewer does not see the purpose of presenting the analysis/methodology if the reader can simply look at the original publication.

Page 6, Line 12: ". . . but it is used the other word round" is non-scientific language.

Page 7, Figure 3: It is difficult to discern from the contours but it seems that the axial induction factor a is increasing toward the tip, at least in parts (a) and (c). This is consistent with classical BEM and vortex methods and rejects some of the strong statements made by the authors, see below.

Page 7, section 3.3: Confusing terminology of 'Glauert correction'. The authors refer to it as the high CT correction used in BEM methods, while other literature refers to the classical tip correction. The authors may reconsider to be more clear on this in the future.

Page 7, section 3.3: Reviewer statement: "If no tip correction is used in the inverse BEM analysis, then the 3D polar data MUST depend on the blade pitch angle". In the reviewer's opinion, this is a trivial result and one that is really not useful for BEM-based efficient design tools. The reason why tip corrections exist is because they can account (at least to some extent for lightly loaded rotors) for the effects of solidity (blade number) and blade pitch angle (through the blade flow angle). The reviewer does not see any scientific merit here.

Pages 9-10, section 3.5: The higher the tip pitch angle, the lower the rotor thrust coefficient (which are not documented). It would have been useful to include integrated rotor thrust and power coefficients and to quantify differences beyond qualitative comparisons of sectional blade loads.

Pages 9-10, section 3.5: The authors do not provide a physical reasoning as to the behavior observed in Figure 5 rather than a plain description of what can be seen in the corresponding figure. The reviewer feels that this is a missed opportunity of providing some further physical insight into the aerodynamic behavior of the given turbine at that particular operating condition. Again, what is the purpose of this work ?

Page 10, Lines 7-9: "It is noted that in the common BEM with 2D airfoil coefficients, the lift and drag polars are always assumed to be independent of the pitch angle. However, the findings of the present work indicate that this is wrong for 2D polars as well as for 3D rotor polars". This is correct but an artifact of the extraction method in the sense that the inverse BEM methodology does not include any tip correction, see also earlier comment. While this is a correct finding by the authors, it is not surprising and, more importantly, does not add much scientific merit to the community as it is very unrealistic to use pitch corrections for airfoil tables in a general predictive sense, particularly when the tip loading is such that the flow is attached (pitch angles of 0deg, 10deg, 20deg). Again, it is not clear what is the actual purpose of this work and what the results should be used for ?

Page 12, Discussion of Figure 7: It would have been really useful to document integrated rotor thrust and torque coefficients to better understand the differences between the various methods. Again, no physical explanation is given; the authors merely discuss what the reader can see in the figures anyhow. Are there any convergence issues for the inboard blade sections ?

Comment on Figures: Vertical axes would be best presented in [kN]. The behavior at the blade root is very concerning; the reviewer suspects that BEM solutions might not be fully converged.

Page 13: Why is load case E2 presented prior to E1 ?

Page 14, Figure 10: The behavior at the blade root is very concerning with respect to numerical accuracy and solution convergence of computed results in general.

Page 14: "... reduction at the tip by increasing the induction factor, which is physically

wrong." and "... , which is physically wrong and can be considered an inconsistency in the tip loss model by Prandtl/Glauert." Unfortunately, these strong statements are both very wrong and concerning as to the authors' understanding of basic blade aerodynamics. 1) The increase in the axial induction factor a toward the blade tip is very consistent with vortex methods (from which the Prandtl/Glauert tip correction is derived) and finite-wing lifting-line theory in general; the reviewer also wants to mention section 3.4.3 in Burton (Relationship between blade circulation and induced velocity) which can serve as a simple counter example to the authors' statements. 2) The reviewer does not understand why the 'a' distribution(s) from Figs. 3 and 6 cannot be used for comparison; in fact, they actually indicate an increase in 'a' toward the blade tip (maybe with the exception of Fig. 3b). This really raises questions as to the scientific quality of this work.

Page 18, Figure 14: What is going on at the blade root ? Are the solutions converged and to what accuracy ? This is very concerning.

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