Review of
Title: CFD based design of Diffuser Augmented Wind Turbines (DAWT)

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Short résumé
CFD on diffusor augmented wind turbines…

Specific comments:

1. The DAWTs are suggested as promising in regards to the cost of energy. The arguments for this are not strong and I think the author could be more skeptical in presenting the economic future for DAWT. Overall I disagree with the statement and I have not seen data that suggest otherwise. The cost of the DAWT should in my opinion also be compared to the cost of an extended wind turbine blade solution having the same power as a DAWT. Extending the blades compared to the expense of the diffusor would not be to the advantage of DAWT considering LCOE. Comparing DAWT to large modern wind turbines is also problematic as upscaling of DAWT to comparable size or MW power is in my view not realistic and misleading not to underline. On the other hand, the DAWT could have a future in its own right and could supplement the marked for small to moderate size wind turbines.
2. The introduction and abstract both starts motivating with reference to loose economic arguments, e.g, lowering energy cost, a trend seen now days in too many papers related to renewable energy. I am not a big fan of this development. I prefer a strong technical motivation for why this paper is relevant. DAWT has been around since the 1960-70 and in that aspect it is also important to underline what is new in the present paper.
3. The intro could include some status on what has been achieved by others doing CFD on DAWT. Many studies with CFD on DAWT have been carried out the last 2 decades, what is the status and what is new in the present paper?
4. Foil selection: this sections needs more work, what are the main achievements reported by the 3 referenced works?
5. The physical definition of the diffusor coefficient is unclear in the text. Eq.1 states it is the mean speed up increase at the most narrow point… coefficient for mass flow increase? More like a measure than a model…
6. CFD setup: some key details should be included e.g. axisymmetric or 3D grid, turbulence model used, number of cells, etc…
7. The data in table 1 has mixed info: The foils mentioned should include the lift at zero AOA to make the table more meaningful for selecting/evaluating foil to choose.
8. The sections headlines in are not well thought through
9. How is chord/radius ratio chosen initially?
10. It is not clear that the diffusor coefficient should be maximized for the no load situation. Why?
11. Figure 3. Why is the curve for S1223 not on this graph?
12. Section 3.1: what does V_{disc} look like?
13. Eq.2, divide with Vo or Vo^2?
14. Sec.3.2, this section is unclear. Fig.4 shows comparison with axial momentum theory for integral CT, CP, local velocities could be shown also. Table 2, Max CP=0.9? Bare rotor or with diffusor?
15. More details on the CFD used introduced in sec.4. This is a bit backward since the CFD was used previously with no details. The aspect ratio of 0.2 is first mentioned here, it could be more chronological.
16. Swirl is omitted, Ok simplification, on the other hand, the BEM Eqs. in app. B includes a'. Including tangential loading should be manageable and provide a shaft CP.
17. A figure explaining the geometry definition of designs clearly is preferred, sec.4.1 is unclear.
18. Augmentation factor could be explained e.g. as a power factor for DAWT. Computing Cs is also interesting I think.

19. The area A in Eq.4 is not defined. Some dispute is associated with the choice of A as choosing the rotor area, Ar, gives a very high CP. On the other hand, the largest area may also be used as reference e.g. the exit diffusor area Ae. In my opinion this area should be used as this is the area the turbine effectively covers. As minimum, the area ratio Ae/Ar should be stated. Likewise the size of a bare rotor that can produce the same power, as stated earlier.
20. The gap investigated, what is the recommended gap? Is the Jet effect wanted or should the gap clearance be a small as possible?
21. The optimization method is characterized more like a parameter variation. No gradients are computed…
22. Sec.4.1.3: It is not easy to follow changes and choices of designs in this section, it is multiple changes and some ordering would help, e.g. a table or figure.
23. Figure 8. Figure text is missing, as for most figures throughout the paper… what is 1^{st}, 2^{nd} and 3^{rd}? Reference to App. needed.
24. Fig. 9. The chord distribution for the bare rotor seems bumpy. Why is the modification to the optimized Glauert design resulting in this for the bare rotor?
25. What does the loading look like?
26. Sec. 4.2. Also a confusing section. The gamma function is introduced in this section, but this should have been explained earlier.
27. Figures 11 and 12 need comments.
28. Conclusions: The diffusors design can exceed the Betz limit, yes, but what is new? Hjort et al. and others has shown this before,

App.

1. A1. Is figure A2 a NACA4412? Please plot geometry in scale 1:1 for x-y cords. Also fig. 5
2. B1. The Eq. in app. B needs comments.

Concluding remarks:

The present text is not easy to follow. The introduction should include a bit of review I think. Stating that the purpose of the paper is to "maximize diffusor power output" is somewhat too simplified, as the intro does not reference prior efforts to do this and what has been achieved. In what way has this not been addressed in prior work? What has been achieved? Is a simple parameter variation enough to get a better design?

The text is in general not clear in my opinion, the paper needs restructuring and clarity. It would be difficult for anyone to redo what has been computed by the author, it is difficult to follow chronological what has been done. All figures are without sufficient explanatory text about what exactly is shown.

I cannot recommend the present text for publication.

Wording:

Betz-Jawceskesy - Betz-Joukowsky

Gorney - Gurney

Many wording issues, the written English needs a careful read through.