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WIND ENERGY SCIENCE DISCUSSIONS

Interactive comment on "Do Wind Turbines Pose Roll Hazards to Light Aircraft?" *by* Jessica M. Tomaszewski et al.

Anonymous Referee #2

Received and published: 20 July 2018

This reviewer is familiar with classical aerodynamics and flight mechanics, but less educated in the fluid mechanics and the industry development of wind turbines. Therefore I find the introduction useful as written, since it well describes the relevance of the topic and it provides the readers an overview of the wake computing methodologies and literature in the field.

Overall I am supportive of the publication of this material, but would like the authors to be more explicit on certain findings. I therefore suggest that the manuscript be further revised, and would recommend this manuscript be conditionally accepted at this time. I would like the authors to provide some additional insight or share more of their experience in the following:

1. Do the authors have evidence that the helical tip vortices generated by the wind tur-

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bine can be meaningfully captured at the source, as well as its downstream evolution, in the computational methodology employed in the paper?

2. Do the authors have evidence that these helical vortices either dissipate quickly (i.e., physically instead of numerically) or is not a flow feature that contributes to worst case encounters?

Other Specific comments: Because this reviewer is not very familiar with the wind turbine industry, I do not know the significance of the phrase “utility-scale wind turbine”. Is that supposed to represent the upper range in the spectrum of wind turbine power extraction (therefore the upper limit of the wake generator)? If so, it would be helpful to point that for the general readers.

Related comment: just because GE 1.5-MW SLE is a common model, it does not necessarily bound the risk. This reviewer uses the word “risk” in the context of the FAA Safety Management System (SMS), in that risk is a combination of likelihood and severity of a hazard. If the turbine being simulated were a common model (at least stated so in the manuscript), it is at least meaningful in a likelihood/frequency perspective. However, it may not be as meaningful in a potential severity perspective. It would thus be useful to provide the additional insight in terms of where this wind turbine resides in the spectrum of characteristics like physical size, power extraction level, etc., in wind turbine industry. As an example, in the area of aircraft wake turbulence, the most common aircraft in commercial aviation is Airbus A320 (a single isle commercial transport). But that airplane, although most common, does not represent the upper bound of the wake turbulence severity. Airbus A380 for example, is seven times heavier than that of the A320 and generates far more significant wake turbulence.

This reviewer supports the choice of the actuator-line approach, as it is claimed in the manuscript that this representation of the wind turbine can capture tip vortices. It would be useful for the authors to more explicitly illustrate that the computed flow field did indeed capture the tip vortices (and the associated initial circulation is considered

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reasonable).

This reviewer supports the inclusion of the neutral atmospheric condition in the simulation. It is likely to contribute to near worse conditions for turbine wake evolution. However, the decay of tip vortices is also influenced by turbulence level, and it would be useful to comment on the turbulence level both in the incoming flow as well representative locations in the wake. EDR is often preferred (only because it is more often used in aircraft wake turbulence), but TKE would be meaningful as well.

This reviver is not familiar with SOWFA, nor OpenFOAM. It would be useful to comment on the Reynolds number associated with a real life utility class wind turbine vs. what is used in the simulation. It would be also insightful to comment on how the incoming boundary layer flow is handled (or at least reference it if it is too lengthy). It would be preferred if the results in 8D are less impacted by numerical noise. However, if the CFD results are considered realistic enough, encounter scenarios in regions prior to 8D should help argue that conservative estimates have been made within the current framework.

Equation 5 appears to be dimensionally inconsistent.

At least in terms of the aircraft wake vortex community, there is no universal acceptance on a severity criteria (a web based, not most authoritative but easily accessible reference would be the power point material from “van der Geest, WakeNet3 Europe, Feb 2012 – “Wake Vortex Severity Criteria, the Search for a Single Metric””). However, it is commonly accepted that roll moment coefficient (a static severity matric) is a reasonable predictor of the aircraft response. However, it is also argued that roll moment coefficient is more relevant when it comes to Large aircraft (when the aircraft roll response is slow in the presence of a wake) and most applicable to Heavy category aircraft. As the authors have correctly pointed out, severity is better described in terms of aircraft response. However, pilot reactions and perceptions are perhaps, at the end of the day, form the most relevant hazard definition. And pilot perception involves a complicated and subjective set of criteria that include the aircraft energy state, altitude and performance. This reviewer is not expecting the present authors to resolve a topic that the wake vortex community has not been able to resolve for 40 years in the US (and 20 years in Europe). Instead, it should be recognized that roll moment coefficient has certain limitation. However, if roll moment coefficient were used in a relative sense against a recognized safe baseline, even though the response is not characterized, it leads to a better argument. It is for this reason that this review does not believe the roll moment coefficient based boundaries developed by Mulizanni and Zheng (2014) are very meaningful. In addition, roll moment coefficient has various levels of approximations in its formulation and the computed value can differ by a factor of close to two for the same flow input. It is believed that the formulation used by the authors is to represent the wing of a typical GA aircraft as a rectangular lifting surface, and this treatment is conceptually consistent with the formulation used in developing a set of baseline roll moment coefficient exposures in wake turbulence (see Fig 5 of AIAA-2016-3434, and its reference 10). If the roll moment coefficient exposure in wind turbine wakes are not anywhere close to the wake turbulence baseline exposure, then an argument can be made that the exposure from turbine wakes are acceptably safe, or just as safe or safer than the exposure due to the ICAO wake turbulence separation, however aircraft respond to those levels of roll moment coefficients.

This review is very interested in the following feedback from the authors: It is not clear that all of the features that potentially pose roll hazards are properly captured. I am particularly interested in knowing if the authors have confidence in their computed field in terms of the proper capturing, as well as the proper decay of helical tip vortices. Mulinazzi and Zheng (2014) used the near field turbine tip vortex data from wind tunnel measurements and assumed the scaling relationship that governs the decay of vortices to be the same as that of the aircraft wake vortices. The decay of the vortices is driven by the ambient turbulence in their formulation, which may not be as realistic since conceptually the vortex decay should be most influenced by the turbulence field surrounding the tip vortices (or in this case, turbulence level in the wake of the wind turbine itself). The decay of the tip vortices in Mulinazzi and Zheng (2014) may therefore represent a conservative scenario. The encounter scenario used in Mulinazzi and Zheng (2014) is then for the small aircraft to hit those highly coherent swirling structures that have sharp velocity gradients in the transverse direction relative to the wingspan of the aircraft. This treatment is considered reasonable and conservative (not necessarily a bad thing for commenting on safety), especially if tip vortices have been shown in the wind turbine literature to persist longer than other velocity deficit or turbulent features. However, once again, the computed tip vortex decay may be too conservative in Mulinazzi and Zheng (2014), since the source of wake decay is taken to be the ambient turbulence instead of the turbulence within the velocity deficit region of the turbine wake. If the LES computation by the authors were shown to be capable of capturing the initial generation of the blade tip vortices with the correct range of strength/circulation, and the computational technique is capable of preserving vorticity without artificial numerical dissipation/diffusion of the vortex, and the encounter scenario involves entering the properly computed / surviving tip vortex, and the conclusion is still that roll moment coefficient is considered low relative to a safe baseline, then it would completely satisfy the current reviewer. This reviewer would like to have some assurance that the results are truly due to all of the possibly relevant flow features being properly captured, and the results are not biased by the computed flow field that cannot capture the critical features that may be important for this specific problem. The tip vortex feature may not be as important in traditional wind turbine wake applications such as siting optimization, but it is considered potentially more important than other features in terms of roll upset. If the numerical scheme and associated modeling of the turbine cannot meaningfully duplicate the tip vortex flow field, then this review would suggest that the wording in the conclusion be modified along the phrases used in Wang, White and Barakos (2017). The aforementioned reference essentially pointed out the flow features their model and LIDAR measurements can reveal, and estimated the risk is only based on those features that their flow field data can resolve.

This review once again, thank the authors for the effort to advance the knowledge in this field.

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