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Interactive comment on "Analysis of Control-Oriented Wake Modeling Tools Using Lidar Field Results" *by* Jennifer Annoni et al.

Anonymous Referee #1

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This is an interesting paper, which compares several wake models to LIDAR measurements. The measurements are performed at a distance of 2.35D downstream of the turbine, and consider varying turbulence intensity and yaw angle. I believe that this paper is suitable for publication in WES, provided the authors can address the minor comments below.

1. Page 1, abstract: It would be useful for the abstract to specify that the wake models are compared to the LIDAR data at a downstream distance of $x = 2.35D$;
2. Page 2, last line: there seems to be an unnecessary parenthesis before 'Bastankhah. . .';
3. Page 4, limitations of the Jensen model: The authors should mention that, as

noted by Frandsen et al. (Wind Energ 2006; 9:39–53), the Jensen model does not conserve momentum.

4. Page 4, wake models: is there a reason to not consider the model introduced by Frandsen et al. (Wind Energ 2006; 9:39–53), which does conserve momentum? It appears that the "gaussian model" discussed here might reasonably be considered a generalization of the 'Frandsen' model. It would be interesting to test this model as well, as it might provide a way of discerning the effects of conserving momentum versus using a gaussian profile;
5. Page 5, limitations of the multizone model: are the parameters in the model constrained in a manner ensuring that the wake model conserves momentum overall?
6. Page 5, Gaussian model, before equation (6): '. . .is computed using a Gaussian wake based on self-similarity theory often used in free shear flows, (Pope 2000).'. A minor clarification – note that the solution shown for example in Pope (2000) is proven (not assumed) to be gaussian, based on the assumption of self-similarity. Perhaps a slight re-wording to clarify this would be '. . .is computed by assuming a Gaussian wake, which is inspired by self-similarity theory often used in free shear flows, (Pope 2000).'
7. Page 6, equation (9): It's not clear how this is incorporated into the model – should this affect the leading term on the right-hand-side of equation (10)?
8. Page 7, capabilities and limitations of the gaussian model: the authors should mention here that this model conserves momentum.
9. Page 13, Para. 2: the models are tuned to a subset of the data with low turbulence. What are the values of the coefficients?

10. Section 4: All LIDAR data are collected quite close to the turbine, just 2.35D downstream, whereas turbines are usually separated by far larger distances. Could the authors please briefly explain the value of this comparison (perhaps in the light of testing yaw models, since yaw effects are immediately discernible in the near wake)?

11. Page 14, first line: the Jensen and multizone models are said not to have turbulence intensity as an input. Could the authors nevertheless please report what values of the fitting parameters would be required to get good agreement? This would be helpful for any researchers who might wish to apply the Jensen/multizone models in high turbulence conditions.

12. Are there plans to make the data from this LIDAR campaign available, in some form, through an online repository?

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