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Interactive comment on "Adjoint-based Calibration of Inlet Boundary Condition for Atmospheric CFD Solvers" *by* Siamak Akbarzadeh et al.

Anonymous Referee #4

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The manuscript employs gradient- and adjoint-based method for calibration of inlet velocity profiles for ABL simulation. To that end, cost function for the optimization problem is defined such that the simulated velocity and the target velocity at the observation point in the simulation domain show maximum agreement for the optimized inflow profile. As a test case, the authors verify their method by applying it to a domain located close to Kassel in Germany. Accurate prediction and estimation of inflow condition is of great interest for wind farm simulation as well as for ABL research in general. Like many other methods (e.g. precursor simulation with re-scaled velocity field), gradient-based optimization of inflow profile can also be one of the approaches for inflow generation. Also, if this method works perfectly, it will have advantage over other methods in terms

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of accuracy, as it does not make any big assumption. However, adjoitn-based methods have their own limitation; for instance, they are not very stable for realistic problems. The author may discuss this and other issues that they may have experienced during their simulation in the manuscript.

Although the issue addressed in the manuscript is very interesting, the manuscript need a lot of technical as well as editorial modification before it is ready for publication. First of all, it seems you do not have convective terms in the adjoint equation. If you have a reason for removing the convective terms please discuss that, else if it is a mistake then I suggest that you redo all the simulations. Next, as I have also put in the specific comments, LESs have become more common for ABL simulations and generation of time-dependent inflow data is more of the issue. Therefore, my concern is why did you choose RANS in this study instead of LES? On the editorial side, you have too many small sections and subsections, and information are spread all over those sections (in particular in section 2, 3, 4 and 5). So, I suggest that you further organize the manuscript.

Specific comments:

1. The purpose of section 2 is not clear. Do you want to give general description of inflow boundary condition and forest effect, or are these the techniques you will use in your study? If it is the former then they should go to Introduction, if it is the later, then you should explain them together with the discussions in Section 3 and Section 4.

2. You have Section 3, subsection 3.1 and sub subsection 3.1.1, but no following subsection (e.g. 3.2 etc) or following sub subsection (e.g. 3.1.2 etc). So, I suggest that you put all the contents in this section under a single section heading without any subsection. But my main concern for this section is once again, it is not clear whether you are trying to explain a general method for gradient

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evaluation in optimization or is it for your specific problem? The section lacks explanation that may be necessary for some one not familiar with Gradient-based optimization. Therefore, some more discussion will be required. For example, what is the role of design variable α, what will your algorithm do to optimize it and why can you write Eq. (11). Furthermore, last sentence in section 3.1 (line 28 and 29) will not come as obvious to many readers.

3. Section 4, 1st Paragraph: The purpose of this paragraph is not clear. Summarizing the differences between your work and that of earlier work is not really necessary. Because the two works deal with different optimization problem, all three differences stated in the manuscript are obvious.

4. The forest model, Eq. (18) should be a part of original Lagrangian and should also appear in Eq. (15) and (16).

5. Why do you not have convective (and cross-convective) terms in Eq. (19)? I still see them in Eq. (16). If it is an error, then please correct it. If you have a proper reason why they can be neglected, you must explain that.

6. It is not clear what ω_j is.

7. It has become more common (at least in academic researches) to use large-eddy simulations (LES) for ABL and wind farm simulations. But authors preferred to use RANS in their work. Can you please discuss why you chose RANS? Was it because RANS is cheaper or was it because it is easy to implement adjoint equations for RANS problem? In reality both inflow profiles and measured velocity at the measurement points V_{M} will vary with time. Furthermore, dynamic behavior of flow field as well and wind farm are receiving more interest in wind energy community (e.g. farm level controller). So, it seems LES would have been preferred simulation method.

8. Pg. 12, Line 14–15: It is not clear why you used velocity profile from a reference simulation instead of the velocity measured by the met-mast. Also, if your reference simulation and actual simulation were performed in the similar condition, then you will obviously get good optimization result. Please provide further information regarding this issue.

9. Pg. 12, Line 16–18: You need to provide more information about conjugate-gradient algorithm.

10. Pg. 12, Line 19: Are you sure that the run-time of adjoint equations is 60% of the primal equations? For most work that I am aware of and from my personal experience, adjoint equations always took longer time to simulate.

11. Pg. 13, Line 4–10: You are fitting the inlet boundary condition from the optimization to a logarithmic or a power law. This may not be a good idea, if you want to exploit the full potential of your optimization scheme. Therefore, instead why do not you add some sort of constrain to your system or add appropriate penalty term to the cost functional?

12. I do not think you have presented sufficient result to consider this manuscript as a technical paper. You only have figure 8 as the results for one simulation case. Please define and perform optimization for more simulation cases. Also, you need to provide further discussion of your result.

Editorial suggestions:

1. As I have mentioned earlier, you have too many sections and subsections and discussions are mixed up in sections 2–5. For example, the discussion about optimization package will be more suitable in section 4. Section 5 and Section 4 can be combined.

2. Pg. 1, Line 16: . . . direction and etc. → . . . direction etc.

3. Pg. 2, Line 2: cost function evaluations → function evaluations
   Just for consistency

4. Pg. 2, Line 12: . . . line-by-line differentiating of . . . → . . . line-by-line differentiation of . . .

5. Eq. (3): I do not think capital V is a commonly used notation for velocity. In major books of fluid mechanics as well as in papers, I usually see $u(z)$.

6. You may want to add schematic for cylindrical domain discussed in section 6.1. First of all definition of x, y, z is not clear. Next, cross sectional (x-z) view in figure 3 is not clear?

7. Pg. 10: If wind blows from east to west then wind direction is 90° and not 270°.

8. Pg. 11, Line 9: higher that w.r.t. → higher than w.r.t.

9. Pg. 13, Line 13: the end the of optimization → the end of the optimization

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