Wind Energ. Sci. Discuss.,
https://doi.org/10.5194/wes-2019-7-AC2, 2019

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Interactive comment on "Improving mid-altitude mesoscale wind speed forecasts using LiDAR-based observation nudging for AirborneWind Energy Systems" *by* Markus Sommerfeld et al.

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Received and published: 29 June 2019

Dear Prof. Schmehl,

Thank you very much for your helpful review of our manuscript, "Improving mid-altitude mesoscale wind speed forecasts using LiDAR-based observation nudging for Airborne Wind Energy Systems", wes-2019-7. We have modified the manuscript accordingly, removed or consolidated several figures and adapted the text.

Please find our response to individual comments below.

Changes are highlighted in the "Supplementary Material" pdf. Text and figures marked in red were removed from the original submission and replaced by text and figures marked in blue. Following are our replies to your comments and a description of modification to the manuscript.

Sincerely,
Markus Sommerfeld

Comments by the Referee

0.1 General comments

This paper about an airborne wind energy resource assessment is a valuable contribution. The focus is clearly on the improvement of the wind speed forecast at higher altitudes using LiDAR data. A relatively small part is about the use of this wind data for the prediction of power production from AWES.

The description of the simplified power production model in Section 4.7 is unclear and inhomogeneous. On the one hand, very specific derivation steps of the original derivation are mentioned (geometric relation of aerodynamic force components and apparent wind velocity components) that are not of interest within the scope of this paper and would require proper illustrations and more background information. Other aspects that would be important are however not discussed, for example assumptions and specific choices. I recommend to carefully revise this part of the paper.

- The sentence: "Additional losses caused by gravity, tether sagging and tether drag are neglected" summarizes some of the assumptions
- steady state assumption, constant c_L and c_D are mentioned in the text
- added tether sagging and point-mass assumption
- removed equations and simplified the derivation.

The original model of Schmehl et al (2013), that was also used as a basis for many other studies, is independent of tether length, as it is also apparent from your Equation (5). What was then the reason for you to choose a constant tether length of 1500 m? And how does the tether length come into play? This should be clearly described.

- Agreed.
- The equation is a function of elevation angle. The optimal elevation angle is calculated from the constant tether length and operating altitude.
- Added additional sentences:
  - Optimal elevation angle (ε_{opt}) and operating altitude (z_{opt}) are geometrically related to the assumed to be constant tether length (L_{tether}) of 1500 m ($\sin \varepsilon_{opt} = \frac{z_{opt}}{L_{tether}}$).
  - The tether length of each estimation is assumed to be constant and used to calculate the optimal elevation angle.
- Do you think the equation $\sin \varepsilon = \frac{altitude}{L}$ is necessary?

If you would account for tether drag, the performance of the AWES would decrease with increasing tether length (compared to the idealized case of no tether drag). Tether drag could, for example, be taken into account by an additional drag contribution and lumping this to the kite, as some authors do. A possible reference could be van der Vlugt (2019). But I assume that this was not done in the paper, for the purpose of simplicity? If so, please state this, as it is important when considering large ranges of tether length. For a implemented real AWES it makes generally sense to fly on a shorter tether when flying at lower altitudes, to reduce the effect of tether drag.

- Yes this is out of scope for this paper.
- The following sentences already address the additional losses associated with a longer tether:
  - All estimates show diminishing benefits of a longer tether. These incremental gains would probably be negated by additional drag and weight associated losses.

For a pumping AWES, which is considered here, the tether length continuously varies. Assuming a constant tether length is seemingly in contradiction with this and should thus be motivated better. Just "Here we assume a constant tether length" is not sufficient in my opinion. I would also like to know, if the choice of the constant tether length could possibly influence the results displayed in Fig. 13 (for this is must be clarified how tether length actually enters the modeling).

- See previous comments above

0.2 Specific comments

0.2.1 Authors

- *I believe that the Fraunhofer IWES location at Bremerhaven, Germany, is meant, and not Oldenburg?*
  - While the headquarter of Fraunhofer IWES is in Bremerhaven, IWES has several other locations in Oldenburg, Bremen, Hannover, Bochum and Hamburg. Gerald Steinfeld and Martin Dörenkämper work from Oldenburg.

0.2.2 Abstract

- *I would spell out WRF once, as you do with AWES.*
  - A definition of WRF was added to the abstract.

0.2.3 Introduction

- *Add a reference to Bechtle et al (2019). This could for example be done on p. 2, l. 14, just after Archer and Caldeira (2009).*

  - Reference added.
- *Uwe Fechner (2016) describes in his dissertation and a later book chapter a turbulence model for AWES, based on the Mann turbulence model. As you shortly mention conventional spectral wind models (Burton, 2011) this might be worth a discussion point. (https:// doi.org/ 10.1007/ 978-981-10-1947-0_15)*
  - Reference added.
- *p. 2, l. 23: You state "No mid-altitude measurement device can reliably gather long term, high frequency data." but do not give any reason for this. This statement should also be better embedded in the surrounding text.*
  - Added a sentence and references
- *p. 2, l. 25: Your reference to future work (complementation of TI estimates with LES data) is better for the conclusions section.*
  - Sentence removed and added to conclusion section.
- *p. 2, l. 28: Add a reference to the Onkites II project report, available from https://doi.org/10.2314/GBV:1009915452 Can the measurement data of OnKites II be made publicly available, as a data reference to complement this and the earlier paper? This would increase the value of this research tremendously (reproducibility!).*
  - Reference added. I can not make the decision to publish the data myself and have to refer you to Adrian Gambier and Julia Gottschall.

0.2.4 Mesoscale Modeling Framework

- *p. 4, l. 16: For the non-experts of this specific technique it would make sense to elaborate on the "non-physical forcing term". Why non-physical? Why not*

physical?

- The additional term is added to the conservation equations that guide the simulation. It is non-physical in nature since it is not based on any physical principle in contrast to the conservation of mass and momentum for example from which the conservation equations are derived. This additional term which is driven by the difference between measurement and simulation nudges the simulation closer to measurement without creating discontinuities in the simulation.
- Added a subordinate clause: "...non-physical forcing term which is added to the governing conservation equations of the simulation to gradually nudge..."

- *p. 4, l. 18: It is unclear what the use of 3 nested domains is. Please clarify. What is η-pressure? (also "η-levels" in l. 23)*
  - The sentence explains the benefit of nested domains which is that the inner domains have higher spatial and temporal resolution.
  - added: ... along the terrain following vertical hybrid pressure coordinate η.
- *p. 4, l. 25: Again for the non-experts: what is the difference between "observation nudging" and "analysis nudging"? Maybe a pointer to the respective subsections, where you explain this, is sufficient.*
  - Added an additional sentence explaining the difference between analysis and observation nudging:
    - For analysis nudging each grid point is nudged towards a time-interpolated value from gridded analyses of synoptic observations whereas observation nudging directly drives the simulation towards observations.
  - Reference: In analysis nudging, the model fields are nudged at every grid point toward an analysis of the observations on the model grid in a manner such that the nudging term is proportional to the difference between the model

and the analysis at each grid point (ref: https://pdfs.semanticscholar.org/1c94/a18e5ce2edd5fa5189dc293d8d33fe46b7c7.pdf)

- *p. 5, l. 4: What is the meaning of "qm interpolated"? And what means "(q0)"?*
  - This sentence explains equation 1 which defines the additional forcing term introduces by observation. This forcing term is driven by the difference between observation q_o and model q_m
  - q_m is the modeled quantity (e.g. wind velocity component, temperature, humidity etc...) which has to interpolated to the location of the additional observation due to the large grid size of mesoscale simulations
  - q_o is the observed quantity
  - No changes to the manuscript
- *p. 5, l. 9: "hydrostatic"? This paper is about atmospheric flows.*
  - Correct. This is part of the model.
- *p. 5, l. 13: The time expression in the bracket is not correctly written. It is not the mathematical constant 2.71828 that is meant here, because this would lead to 9 seconds.*
  - replaced by: $1/6\ 10^{-4}s$

0.2.5 Results

- *Elaborate on how unavailability of LiDAR data is handled for the nudged simulations.*
  - Added sentence in subsection 'Observation Nudging': Nudging could not be performed at times and altitudes where LiDAR data was not available.

- *p. 8, l. 3: RSME is missing in legend.*
  - added RMSE to the legend
- *p. 8, l. 5: The reduction of the spread of the bias is hard to observe by eye*
  - The reduction in bias spread (visualized by the lengths of the horizontal blue and red lines, also called whiskers) is clearly visible in the PDF.
- *p. 8, l. 9: Doesn't nudging reduce the error? So, reduced nudging would result in larger error?*
  - Good point. Subordinate clause removed.
- *p. 9, l. 14: Please elaborate on this sentence.*
  - Need more information. page 9 is referenced n this and the following comments. Probably page 8
  - added: ... as can be seen in the right box plot in figure 3
- *p. 9, l. 11: Bechtle et al (2019) have used a similar representation as the one described here, using dots to show the optimal altitude for operation of an AWES. A reference should thus be added, and possibly also a discussion of the usefulness of this measure added (i.e. an AWES will generally sweep an altitude range, which means that this single point characterization is only a very rough measure.)*
  - While we agree that a discussion is useful, we disagree that the usage of dots to visualize optimal altitude justifies the added reference. The same reference as been cited earlier in the paper.
  - Added sentence: A single point is only a rough measure of operational altitude since AWES generally sweep a range of altitudes.

- *p. 9, l. 14: How do you see that the LLJ and the .... are weaker? I can hardly see anything.*
  - The color of the contour plot in the upper subplot, which is refers to the horizontal wind speed, is significantly different between OBS and NoOBS.
- *p. 11, l. 7: You write "remain the same". Shouldn't ΔV be zero?*
  - That is right. While the boundary condition ($\Delta U = 0$) is applied on the outward facing surface of the cubic grid cell the wind speed values are stored and interpolated to the center of the grid cell. This leads to $\Delta U \neq 0$ at the boundary grid cell.
  - Added sentence: $\Delta U \neq 0$ because wind speed values are interpolated to the center of each grid cell.
- *p. 11, l. 8: You write "change in wind speed": is this observed by the gradient?*
  - can be seen by the spike of the red line close to the vertical black line.
  - Added subordinate sentence: ... measurement location which is highlighted by the black vertical line ...
- *p. 14-15, Figs. 7 and 8: Why are contour plots of Fig. 7 not as smooth as respective plots of Fig. 8. The caption mentions "filtered": aren't these "unfiltered"?*
  - As mentioned in the text: these contour plots are filtered by LiDAR availability. As a results, WRF data is discarded at times when LiDAR is not available. Therefore, the WRF results are skewed, but similar to LiDAR measurements.
- *p. 20, l. 3: You write "We chose": how do you control this?*
  - This has been addressed together with comments from the second Referee.

- *p. 21, l. 8: "Misalignment" is between TETHER and wind direction. It should be clear that the azimuth and elevation angles describe the angular position of the kite or aircraft with respect to the ground station. Renaming of Θ as elevation angle is dangerous, because it is generally use for the polar angle.*
  - Changed elevation angle to ε
  - Changed sentence to: Losses associated with mispositioning of the aircraft relative to the wind direction, expressed by azimuth angle ϕ, elevation angle ε relative to the ground station, are included in the model.

0.2.6 Conclusions

- *I am missing some conclusions of Section 4.7 on the AWES power estimation.*
  - reworked conclusion paragraph on AWES.

0.3 Language and style comments

0.3.1 General spelling

- *Use of dashes should be checked (e.g. "high-resolution data" or "long-term statistics" would be correct)*
  - hyphen added to the best of my knowledge
- *Do not capitalize abbreviations*
  (see https://www.aje.com/en/arc/editing-tipcapitalization-when-defining-abbreviations/).
  - removed capitalization in abbreviations except for names.

0.3.2 Title

- *p.1: "Airborne Wind Energy" should be "Airborne wind energy".*
  - removed capitalization, changed to “airborne wind energy”

0.3.3 Abstract

- *p. 1, l. 9: I would add an "it" between "but" and "becomes"*
  - pronoun “it” is omitted since there is no ambiguity that the topic of the sentence is the impact of nudging.

0.3.4 Introduction

- *p. 2, l. 4: "Airborne Wind Energy Systems" should be "Airborne wind energy systems".*
  - fixed capitalization
- *p. 2, l. 4: I would say that AWES are a class of renewable energy technologies, and not a source of energy. The source is the wind.*
  - Implemented.
- *p. 2, l. 10: Instead of "marketplace" I would just write "market".*
  - Implemented.
- *p. 2, l. 11: ...none are YET commercially available.*
  - changed to: ...none are currently commercially available

- *p. 2, l. 13: "power" should be "power output" as you list wind energy technologies here.*
  - Implemented.
- *p. 2, l. 23: ... variations to resolved quantities are parametrized. The "resolved" sounds wrong and the meaning of this sentence is also not clear to me.*
  - Corrected the sentence: Sub-gridscale high frequency variations **of** resolved quantities are parameterized.
- *p. 2, l. 25: "here presented" -> "presented in this study"*
  - Sentence removed in process of editing the manuscript.
- *p. 2, l. 27: Year is missing in reference.*
  - Reference updated
- *p. 3, l. 1: "power" -> "power output".*
  - Implemented.

0.3.5 Measurement campaign

- *p. 4, l. 2: Year is missing in reference.*
  - Reference updated
- *p. 4, l. 3: Should be "emphasizes".*
  - Implemented.

- *p. 4, l. 6: Should be "... the WRF-calculated...". The entire expression "WRFcalculated sensible surface heat flux (SHF)" sounds incomprehensible to me. What is the role of the "sensible"?*
  - Added hyphen.
  - Latent and sensible heat are types of energy released or absorbed in the atmosphere.
    - "In meteorology, latent heat flux is the flux of heat from the Earth's surface to the atmosphere that is associated with evaporation or transpiration of water at the surface and subsequent condensation of water vapor in the troposphere." https://en.wikipedia.org/wiki/Latent_heat
    - "In meteorology, the term 'sensible heat flux' means the conductive heat flux from the Earth's surface to the atmosphere." https://en.wikipedia.org/wiki/Sensible_heat
- *p. 4, l. 9: Should be "... the SHF".*
  - Implemented.
- *I would move Fig. 1 to the next section and remove the reference to the white X here. Because in the next section you explain the 3 hierarchically nested domains used for the WRF. Here, in this section, the figure introduces more questions than answers.*
  - Figure was moved to section 2.
  - Reference to white X was kept to show the measurement location and the location where observations were implementation.

0.3.6 Mesoscale modeling Framework

- *p. 4, l. 14: Year is missing in reference. It is also not clear whether the "section 2" in the referenced paper or the present one is meant.*

- Citation updated and section 2 removed.

- *p. 4, l. 15: Year is missing in reference.*
  - Citation updated.
- *p. 4, l. 17: Why discussing here spatial resolutions when this is all given in Table 1?*
  - Spatial resolution is kept in the sentence and table removed.
- *p. 4, l. 23: "Turbulent Kinetic Energy" should be "Turbulent kinetic energy". Add "(TKE)" here and use the abbreviation in the next sentence.*
  - Replaced by “turbulent kinetic energy”.
- *p. 4, l. 29: Maybe a footnote link with the URL is better? This bibliographic reference looks strange.*
  - Removed citation and replaced with footnote: “EDDY: HPC cluster at the Carl von Ossietzky Universität Oldenburg, see: https://www.uni-oldenburg.de/fk5/wr/hochleistungsrechnen/hpc-facilities/eddy/”
- *p. 5, l. 20: Something is wrong after* W_{xy}.
  - Fixed equation variable

0.3.7 Results

- *p. 6, l. 5: Should be "differences".*
  - Implemented.

- *p. 7, Fig. 2, legends: text and number should be separated by a space and also a comma.*
  - Updated legend to: linear regression, slope: 0.985.
- *p. 9, l. 2: Replace "bias" by "error".*
  - Implemented.
- *p. 17, l. 3: Reference missing (?).*
  - Citation updated.
- *p. 19, Table 2: last three columns in % of time would be better readable.*
  - Implemented.
- *p. 20, l. 4: "additional two" -> "two additional"*
  - Paragraph updated while editing.
- *p. 20, l. 5: Figure reference is missing (??).*
  - Paragraph updated while editing.
- *p. 21, l. 6: Why do you use a subscript "air" for the density? This study is only about atmospheric flows, so the index can be safely omitted.*
  - That is correct. However, we chose to keep the subscript “air” for clarity.
- *p. 21, l. 7: Set equation in displaymode.*
  - kept equation in text. Could be changed when typesetting the manuscript.
- *p. 21, l. 10: "... are assumed constant are ...": something is wrong here*

- Changed sentence to: " ... and drag coefficient (c_D =0.06), which are assumed to be constant, are geometrically related to ..."

0.3.8 Conclusions

- *p. 23, l. 7: Six months OF LiDAR .....*
  - Implemented.
- *p. 23, l. 13: Dot behind "decreases" is missing.*
  - Paragraph changed while editing the manuscript.

0.3.9 Appendices

- *p. 25, Figure A1: what means the question mark at the end of this caption? "U profile" -> "velocity profile".*
  - Removed figure.
- *p. 25-26, Table A1: Include in the caption to which software & version, possibly also model, these settings refer.*
  - Caption updated to: "Namelist parameters for WRF 3.6.1 observation nudging"

0.3.10 References

There are many references for which the DOI is occurring twice, as "doi:..." and as URL "https://doi.org/...".

removed URL and kept DOI where applicable.

- *p. 28, l. 13: what is this oCLC number? I would use either ISBN or DOI.*
  - removed oCLC and replaced with DOI.
- *p. 28, l. 19: Publisher or standard-issuing organization is missing.*
  - updated reference to ISO 2533:1975
- *p. 29, l. 15: Correct the URL.*
  - Citation replaced with footnote.
- *p. 29, l. 23: Add DOI.*
  - Citation correct, long, long list of contributers followed by a DOI and URL
- *p. 29, l. 27: Insert comma/dot and space between "2016" and "At".*
  - Not sure where. No 2016 in this citation.
- *p. 30, l. 3: "Statistik" should be starting with a capital "S", according to German spelling. Consider choosing an English textbook as reference.*
  - Replaced reference.
- *p. 30, l. 8: This is a contributed chapter in a book. Accordingly the reference should be Schmehl, R., Noom, M., van der Vlugt, R.: Traction Power Generation with Tethered Wings. In: Ahrens, U., Diehl, M., Schmehl, R. (eds.) Airborne Wind Energy. Springer, Berlin Heidelberg, 2013.*
  - The citation is taken directly from: https://link.springer.com/chapter/10.1007/978-3-642-39965-7_2 . Had to change @inbook to @incollection to make it work.
- *p. 30, l. 14: Update this*

- Updated.

Please also note the supplement to this comment:
https://www.wind-energ-sci-discuss.net/wes-2019-7/wes-2019-7-AC2-supplement.pdf

Interactive comment on Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2019-7, 2019.