Review of: Klaas and Emeis, The five main influencing factors on lidar errors in complex terrain.

The authors would like to thank the referee very much for the valuable and concise feedback provided and the effort he put into this. We appreciate the comments and suggestions very much. They are very helpful to increase the quality of the manuscript and add relevant information for the reader.

In the following, we answered all of the referee's questions, comments and suggestions. We state how we use the feedback in the revised version of the manuscript and provide additional information and explaination for the referee whereever needed.

General

I applaud the perusal of insight and simplicity clearly demonstrated by this paper. Particularly the idea to split the error source into two parts, curvature and speed-up, is well seen and well demonstrated. Similarly I like the structuring with models of increasing complexity and the opportunity this gives to draw insight and identify limitations.

I am a little disappointed however in the low amount of insight that is drawn and lack some deeper reflections on the implications of the findings. My main suggestion is therefore to add a 'Discussion' section (not necessarily with this name and (please) separate from the Conclusions) where the authors reflect on (suggestions):

- That the errors (on hill tops?) are always negative. Is this a general result, why and can we expect lidar measurements in positions where it wouldn't be true?
- The reasons for the trends that are seen (this is already tackled to some degree in the text). For example (one only that caught my eye), why does increasing stability give larger errors where increasing roughness (and forest) gives smaller errors?
- How do the parameterizations combine? (e.g. if you have strongly stable flow and tall trees, are the predicted errors even lower?)
- (Why) Should we believe these results? To what extent is there experimental corroboration?
- Would other 'hi-fi' models give similar results?
- It's a bit alarming to see how much the errors change with the parameterizations? What does all this say about the uncertainty of the corrections?

The elegance of the paper would be heightened by a corresponding lift in the quality of the writing. Not that this is bad but some of the explanations are more difficult to follow than necessary. Perhaps use more examples (measurement situations) in explaining the trends shown in the different plots.

Make the text shorter where you can.

We would like to thank you very much for your detailed and well structured general feedback on our manuscript. We are pleased to read your general appreciation on the topic and how we tackled it. Beyond that, we find your suggestions on a 'Discussion' section very helpful. After having studied all of your feedback and suggestions and reviewed our submitted manuscript, we agree with you that it would highly benefit from a separate 'Discussion' section. We have therefore added that section between the 'Methods' and 'Conclusions' sections. In the revised manuscript we have moved already existing discussion paragraphs to the new section. We have extended our discussion according to the following points, which also include your above given feedback:

- Discussion on general findings for all parameters, which includes explainations on when the lidar error is negative (or positive) and about the implications of measurement locations on a hilltop, on the flanks of the hill etc.
- Discussion and interpretation of the error parts and their importance
- Discussion of the role of the half-cone opening angle, including references to relevant literature on that topic
- Discussion and interpretation of the influence of roughness, forest and atmospheric stability including explaination of the reason for the influence whereever possible
- Discussion of the implications of the sensitivity of the lidar error estimation to model parameterization and what this means for applications
- Discussion of combinations of different parameters (roughness and stability or forest and stability etc.)
- Considerations regarding the magnitude of the lidar error in comparison to e.g. cup anemometry in complex terrain
- Considerations on the flow models used, including possible limitations and discussion about other 'hi-fi' models
- Discussion on available (and needed) experimental validation of the flow modelling results

We have included additional literature references where needed to support our discussion. We have also included additonal plots with streamlines from the RANS flow model that help discussing and interpreting the influence of roughness (forest) and atmospheric stabilty on the model results.

Specific comments and answers:

Abstract

First para:
Wind lidars (or Doppler lidars) don't have much to do with "light detection and ranging" – they don't detect or range!! It's just a name so please don't labour this. Is it the 'measurement principle' (what is this?) that is the problem – or the assumptions inherent in the processing of the raw data?

We agree that in the relevant literature it is common to simply use the term „lidar" or „wind lidar". We have therefore changed the first sentence and have removed the explaination of the abbreviation.

The second sentence was meant to briefly state that wind lidars are prone to errors at complex terrain sites, which is then elaborated throughout the manuscript. To be more precise here, we propose to write: „However, because of the assumption of homogeneous flow in their wind vector reconstruction algorithms, common Doppler lidars suffer from errors at complex terrain sites."

We have rephrased this in the revised version of the manuscript.

L15: suggest remove e.g.

We removed „e.g." in the revised manuscript.

L20: suggest remove *manifold* (I had to look it up and it's my mother tongue.

We have replaced „manifold" by the more common term „various" which we think fits better here.

Last sentence:
*'When planning a measurement campaign, an accurate estimation of the prospective (predicted?) lidar error should be carried out in advance **to decrease measurement uncertainties** and **maximize the value**.':*
Knowing what the error isn't enough of course – you need to make the correction in order to reduce the uncertainty. What are you maximizing the value of?

Thank you for your comment.

Of course, you are right, that besides the estimation of the lidar error it is neceassary to apply a correction to the actual measurement data. However, the idea of error estimation **before** the measurement campaign is to choose reasonable measurement locations where lidar errors are relatively low. E.g. in the German FGW Technical Guideline 6 – „Determination of wind potential and energy yields", the additional uncertainty due to lidar error correction is assumed to increase proportionally to the magnitude of the lidar error.
Because of this, it is beneficial to choose a measurement location with low predicted lidar errors that will then only add small additional uncertainties to the overall wind ressource assessment.

We agree that „predicted" fits better to the context than „prospective" and have changed this in the revised version of the manuscript.

We rephrased the last two sentences to clarify why lidar error estimation should be carried out before the measurement campaign in order to optimize the choice of the measurement location and that the overall aim is to maximize the value of the measurement data by decreasing measurement uncertainties.

1 Introduction

L32: *Due to their principle of measurement*,.. – Please be more specific here

Similar to the Abstract, we have rephrased this sentence to make it more specific.

L32: profiles = profilers

Thank you, we have corrected this in the revised manuscript.

L45: *equivalent* – change to identical?

We agree that „identical" is the more appropriate term and have changed it in the revised manuscript.

L59: *there is no significant increase in the* magnitude of the *errors* (add magnitude of the)

Thank you, we have corrected this in the revised manuscript.

L77: delete between

Thank you, we have corrected this in the revised manuscript.

A general comment here is that most, if not all, of the literature fails to recognize that traditional instrumentation (i.e. cups) in complex terrain have a fairly high uncertainty.

Thank you for your comment.

Indeed we agree that flow in complex terrain also has an influence on cup anemometer (or sonic anemometer) uncertainty. E.g., increased turbulence and flow inclination differ from those in flat terrain, which leads to higher uncertainties. A procedure to account for the acutal ambient conditions is therefore implemented into the IEC 610400-12-1 standard, Appendix I. From relevant studies we learned that class indexes significantly increase for class B sites (e.g. J.-Å. Dahlberg, Friis Pedersen, T., and P. Busche: ACCUWIND - Methods for classification of cup anemometers, Denmark. Forskningscenter Risoe. Risoe-R, 2006).

In the revised manuscript we have added a short paragraph to the introduction that discusses calibration and classification uncertainties of cup anemometers in complex terrain. We have also added relevant literature about this. Additionally we are discussing the magnitude of the lidar errors from our parameter study in the context of measurement uncertainties in the newly introduced 'Discussion' section.

L134: Do you expect (or not) that stability directly affects the lidar accuracy? (I think this is most unlikely). Understand your explanation that stability indirectly affects the error through changed profile, turbulence(?) and flow patterns. Are there other flow parameters that might be relevant- e.g. Froude number?

Thank you for your comment.

We expect atmospheric stability to change the flow patterns above complex terrain. Changed flow patterns will then affect the lidar errors, which makes the lidar error indirectly dependent on atmospheric stability. We support this with relevant literature on flow over complex terrain in neutral and stratified atmospheric conditions (e.g. Ross, A. N., Arnold, S., Vosper, S. B., Mobbs, S. D., Dixon, N., and Robins, A. G.: A comparison of wind-tunnel experiments and numerical simulations of neutral and stratified flow over a hill, Boundary-Layer Meteorol, 113, 427–459, https://doi.org/10.1007/s10546-004-0490-z, 2004.).

We have added this to the newly introduced 'Discussion' section. To explain better, how flow patterns change due to changes in atmospheric stability, we have added a plot with streamlines over a hill from our simulations.

The Froude number might be used to classify atmospheric stability. In our study we use the Obukhov length as a measure for atmospheric stability. This measure is applied by the CFD model Meteodyn WT that we use in our study. It is also possible to calculate the Obukhov length from high frequency sonic anemometer measurements, which often makes it the method of choice to classify atmospheric stability. We have not done any investigations on other flow parameters such as Froude Number or Bulk Richardson number and their influence on flow pattern and therefore lidar error. A more detailed discussion of stabiliy classification methods is beyond the scope of our study as we think and might be part of future work.

L139: *Courtney et al.* ***were*** *(not* ***are****) proposing…* They probably got wiser from the subsequent experiments…;)

We are certain about that and have rephrased the sentence in the revised manuscript.

2 Methods

L184: The uncertainty of all experimental data should be estimated before comparing to models – cups as well as lidars. How should this uncertainty be used?

Thank you for your comment.

The intention of this paragraph is to point the reader to the fact that there are uncertainties in all relevant aspects: The flow model predictions, the lidar measurement data and also the specific aspect of lidar error correction. So either – when no lidar data correction is applied – the uncertainty of the data is considered to be higher at a complex terrain site due to (unknown) lidar errors. Alternatively, after application of a lidar error estimation and correction, the uncertainty can be reduced. Still an additional uncertainty from the lidar error correction has to be considered.

We have slightly rephrased the paragraph in the revised manuscript to clarify our intention.

The uncertainty should be used to evaluate the comparison between e.g. modelled and measured wind profiles at the complex terrain site. If lidar errors significantly contribute to the overall measurement uncertainty, it might not be possible to validate model results without the application of the error correction.

L191: Again, what about the 'mast' measurement uncertainty?

Yes, we agree that also the mast has an (increased) uncertainty at a complex terrain site. We added this into the above mentioned paragraph in the revised manuscript and the newly introduced 'Discussion' section. See also our comment above.

L222: win -> wind

Thank you, we have corrected this in the revised manuscript.

L249: I think you are using 'exemplary' wrongly throughout the paper. Please check its definition. I would write '*are shown as examples within this study*'.

Thank you very much for your advice. We have checked and corrected or rephrased this throughout the revised manuscript.

L268: Are these results only valid for a top-placed lidar? What about the more general case of a lidar on the side of a hill (where the error is usually lower anyway)?

Thank you for your comment.

All results presented in the study are valid for a lidar placed on the top of the hill. We have added a sentence in this paragraph to clarify this. We agree that this is a special case and that real measurement locations will often be on the flanks of a hill, on a plateaus, close to escarpments or in valleys.

However, considering all of these cases would expand the study very much. It is also not trivial to answer the question if errors on the flanks of the hill are generally lower than on the top. E.g. when considering a forested case, the streamlines are shifted downwind. The turning point of the flow (where positive vertical wind changes to negative vertical wind) will be found in the lee of the hill. Having a lidar placed on the downwind site of the hill will then eventually lead to larger errors than on the top.

A valuable tool to analyse lidar errors for different is the lidar error map that is presented in Figure 7 of Klaas et al. (2015) https://doi.org/10.1127/metz/2015/0637 . We have added a reference to this in the newly introduced 'Discussion' section and now provide a more elaborate discussion on other cases in the revised manuscript.

L272: and error -> an error

Thank you, we have corrected this in the revised manuscript.

L283: add *horizontal* after *reconstructed*

Thank you, we have added „horizontal“ in the revised manuscript.

L293 (eq 3) – please state here what u_in and u_out are (you do this later in L310). Are there assumptions (symmetry?, linear change in speed?) in this simplification? I wasn't completely clear about this.

Thank you for you comment.

u_{in} and u_{out} refer to u_4 and u_2 in Figure 3, which have their indices from the clockwise numbering in the three-dimensional case (Figure 2). We agree that it is confusing to use different notations in the figure and the text / equations. We have therefore updated Figure 3 in the revised manuscript and now only use subscripts 'in' and 'out' as in the following equations and text.

There are two main assumption that are used when deriving the equations:

1. The ratio k between outflow and inflow wind speed ($k = V_{out}/V_{in}$) is set to 1, assuming that both wind speeds are the same. This removes V_{out} and V_{in} from the equation.
2. The horizontal wind speed at the lidar location u_L is defined as the mean of u_{in} and u_{out}. This makes it possible to cancel u_L out of the equation, when inserting equation 4 into the lidar error definition equation 1 (left part).

These two assumptions enable us to derive an equation for the flow curvature induced lidar error ε_c that is only dependent on inflow angle α, outflow angle β and half-cone opening angle φ.

With respect to these assumptions, we have slightly changed the resulting equations (5) and (6) and replaced the equality sign by an approximately equal sign.

(!) Please note that we have introduced an additional equation in the revised manuscript – the Gaussian hill definition – that is now equation (1). Following equation numbers have therefore been shifted by 1. Above given equation numbers refer to the original manuscript.

L302 (eq 6) – why, contrary to expectations, does the opening angle still appear here?

As described in the manuscript the error equations are based on a very general approach and only two minor assumptions (see above) are used.

Contrary to other simplified approaches of lidar error correction (especially Bingöl, F., Mann, J., and Foussekis, D.: Conically scanning lidar error in complex terrain, Meteorologische Zeitschrift, 18, 189–195, 2009.), the cone-opening angle does not cancel out in our approach. This is because we do not assume the vertical wind speed to change linearly, which is the main assumption in Bingöl's considerations.

We have added this to the newly introduced 'Discussion' section to emphasize the difference to already available literature.

L315 mostly -> usually

We agree and have changed this in the revised manuscript.

L315: why does this comment appear here and not in the results section?

We agree that this should be moved to the results section as it describes the way the results are presented. We have changed this in the revised manuscript.

L343: Several comments regarding the -2% threshold for cup-anemometers and the comparison with this. Firstly is 2% realistic (the reference is in German, but it is easy enough to find typical values for cup operational uncertainties (class B in complex terrain) and add in mounting and calibration uncertainties. I would suspect higher values than 2% (was this using class A?). And is this standard uncertainty (k=1)?

Thank you very much for your comment. We agree that measurement uncertainty with cup anemometry at complex terrain sites might have higher total uncertainties than 2% (standard uncertainty, k=1). The intention of drawing the -2% and the -10% lines was to raise awareness about several aspects:

1. Cup anemometers as a reference or standard sensor, which is mostly applied in wind energy, do also have uncertainties (of course) and we wanted to roughly indicate their magnitude.
2. With complex terrain induced lidar errors in an order of 2 %, the total uncertainty of a wind ressource assessment will not benefit much from a correction.
3. The overall uncertainty of wind ressource assessments should stay below approximately 15% for complex terrain sites. Lidar errors in the order of 10% will add too much to the uncertainty, even if a correction is applied (see next comment). Therefore, lidars are not a good choice at such complex sites.

However, based on your comments we have decided to remove the -2% and -10% lines from the plots, because we think fixing these values will not be helpful for the discussion of the results in the manuscript. Instead, we have added a more elaborate discussion about mast / cup anemometer uncertainties at complex terrain sites (class B) in the newly introduced 'Discussion' section. We have also added relevant (and English) literature for this discussion We hope that this will help the reader to better interpret the magnitude of the complex terrain lidar errors.

But this would be the total uncertainty for the cup. Wouldn't it be fairer to compare with the total uncertainty of the lidar and not just the uncertainty due to the non-homogeneous flow?
The -10% seems very arbitrarily chosen. Do we expect an uncertainty of 10% if the error is 10%?

Thank you very much for your comment. Several aspects of this comment are already answered above. We have removed the -10% line in the revised version of the manuscript.

Nevertheless, we would like to explain why we have chosen -10%: From literature (which we have now added to the revised version of the manuscript) we found that total uncertainties of about 15% at complex terrain sites can be seen as an upper limit for a reasonable (bankable) wind ressource assessment. Without consideration of the additional uncertainty due to lidar error correction, we fixed the total uncertainty of a wind ressource assessment at a complex terrain site to 12 %, which is based on literature research and rough estimations of the specific uncertainties within a wind ressource assessment in complex terrain. Additional uncertainties caused by lidar errors will now increase the total uncertainty of the wind ressource assessment. Following the relevant FGW Guideline 10, we expect an additional uncertainty of 50% of the lidar error (5% uncertainty at 10% error). For a magnitude of 10% of the lidar error the total uncertainty will increase beyond 15%.

Therefore, we think that lidar measurements are no longer feasible at such sites. We have added a more elaborate discussion on this in the revised version of the manuscript.

L351: All of this section takes some digestion. Try and make the text more straightforward, E.G (L351) *For the least complex hill, ….* I would suggest something like *For the least complex hill (H/L=0.1), the lidar error is below 2% until a non-dimensional height (z/L) of 0.16 is reached. The error reaches a maximum of 3% at z/L=0.5 and falls below 2% again above z/L= 1.5.* Maybe even set this in context, e.g. with a hill L=1000m and H=100m.

Thank you very much for your suggestion. We have considered this and rephrased the section in the revised version of the manuscript. This discussion is also moved to the newly introduced 'Discussion' section.

3.4 Influence of surface roughness

L427: *reference* – which reference?

Please excuse the confusion. What is meant here is the potential flow model, which – as the simplest of the three models – is used as a „reference" or „baseline" simulation (compare lines 221-224). In order to avoid confusion we have decided to always refer to it as the „potential flow model" and have rephrased the sentence in the revised version of the manuscript.

L436: The -2% dashed line is missing in Figure 7 (Right)

As explained above, we have completely removed the -2% dashed lines in the plots in the revised version of the manuscript.