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Atmospheric Measurement Techniques
Discussions

Interactive comment on "An LES-based airborne Doppler lidar simulator for investigation of wind profiling in inhomogeneous flow conditions" *by* Philipp Gasch et al.

Anonymous Referee #3

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The article details about the use of LES data to create an airborne Doppler Lidar simulator. The article is aiming to account of the accuracy level of the retrieved wind and investigate the effect of flow in-homogeneity on the VAD scans. It is a well-known fact, that the flow in-homogeneity will cause significant errors in a simple LSQ or SVD type algorithms. There has been sufficient literature and studies on this aspect, using real simulated data. A lot of the sentences have been repeated in the article, one of the reasons for making it such a long article. I should be honest and say that I could not review the entire paper, as I had noted several issues with the article. I would request the authors to re-evaluate, revise and edit the paper significantly before sending the revised version for another round of reviews. But overall I feel its an interesting pa-

per/topic, and has the potential to be published. Some major and minor comments for the authors to address are provided below.

Major comments:

1. The LES domain is too short for this type of study. In the 5 km x 5 km x 1.8 km domain, the authors only use 2 points within to do the analysis for a given trajectory. And approximately 20 trajectories are used in their analysis. The authors also mention that they do not have enough statistics, which is a big concern in such a study. The authors base their methods based on Stawiarski 2014 2015, which is for a ground-based Doppler Lidar and who were looking at detection of coherent structures and the ability of Lidars to resolve those structures. But here, the authors are trying to assess the accuracy of the in-homogeneity, and more simulations and larger domains are a necessity. I can understand that the authors want to have high-resolution LES model data to validate their simulator, but statistically these results cannot be taken seriously. Again, it is very well known that flow in-homogeneity causes errors in Doppler Lidar retrievals, but quantifying the amount for airborne Doppler Lidar measurements is key. And the reviewer feels, that the quantification part is missing here, which is key.

2. The optical effects of the laser beam due to motion has not been considered in the results. It is just demonstrated that the simulator can do motion correction. It would complicate the analysis and make it more realistic and interesting to study such realistic effects (Kavaya 2014 Hill 2008) rather than only flow in-homogeneity (again its a well known issue). Maybe the authors can take motion data from a real aircraft and simulate the effects of wind retrieval accuracy.

3. The article has several repetitive statements all over the document and is extremely long. I feel a large portion of this article can be moved to the supplement section or deleted. The sentence formations are very abrupt, which sometimes I feel are unnecessary and not adding any additional information. I have noted a couple below, but I would urge the authors to take a deeper look at the entire article.

4. The Figures are not very helpful in understanding the results. They all are dumped into one figure with results from several simulations and different type of statistics. Figures 6-16 are very confusing and do not make it obvious without reading the winded text. I can understand the authors sentiment in showing a lot of information, but they need to use smarter approaches such as Taylor's diagram or similar.

Specific comments:

Page 5 Line 10 – The flight temporal sampling is not clear. 1-minute temporal sampling, does that mean the aircraft takes 1 minute to travel the 5 km domain? Isn't that a function of the scan trajectory? Its not very clear. Is the wind assumed to be stationary over that 1 minute?

Page 8 Line 7: Instead of saying "their" weighting function ... please mention the type of weighting function. Was is a Gaussian or rectangular or? Similarly, other things in the paper.

Page 8 Line 8 – What is deltaX? Please use lidar technical terms or variables to be consistent with the literature.

Page 8: I feel a short table of these characteristics would make it much more legible and easier for the readers.

Page 8 line 19: "can be added to the simulated wind field"? Please refine, as it has been added in the flow field, is my understanding.

Page 8 line 9 – What is FME? The acronym comes doesn't make sense with the sentence before.

Section 2.3 – A motion correction algorithm is introduced, but the author mentions that they have not used any of that in their paper. I feel this section is not useful for this paper, if not used. Please cite a reference, such as Kavaya et al., 2014 or Hill et al., 2009, which has similar motion correction algorithms implemented to real lidar data. If its different that these algorithms, then please state only the difference. Please move

this the next section to supplement.

Page 3, Line 26... Remove "These include" and just list the references.

Page 5 Line 6... Gravity waves being present above what? – Sentence structure is wrong! The sentence structures sometimes are too abrupt and unnecessary. So maybe the authors have to give it another read and remove some of the abrupt sentence structures. Such as Page 4, Line 26, it is not necessary to have the first statement "The simulator is tested with a set of underlying wind fields". There are other instances such as this and needs to be addressed. I understand that it's a personal choice of the reviewer.

Page 27 line 11: why bold number of retrieved parameters (VAR)? And similar other locations. Please change.

Since there were too many issues with the paper, I feel like the authors need to revise the paper carefully and provide a better and shorter version for the reviewers.

References: Kavaya et al., 2014 - DOI: 10.1175/JTECH-D-12-00274.1 Hill et al., 2008 - https://doi.org/10.1175/2007JTECHA972.1

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