

Author comments for Anonymous Referee #3

The authors thank the referee for her/his valuable comments and the positive overall evaluation of our manuscript. We carefully addressed all comments and accounted for them in our revised paper as stated below. The document is structured as follows. The original referee comment is provided in *italic*, followed by the author's response and author's change in the manuscript (deleted parts are ~~scored-out~~ and added parts are **bold**).

Referee Comment #1

There are a considerable number of references that are to various documents that are only available on the web or from similar sources. It is not clear whether these documents are peer reviewed and these documents may not be available in the future. The authors should be encouraged to substitute peer-reviewed papers as references wherever possible.

Author' response #1

Yes, we are aware of the problem if peer-reviewed papers are not (yet) available for all aspects to be cited. We confirm we used peer-reviewed papers wherever possible. Favorably, also the content of the key ESA report that introduces the new Abel transform, and that is therefore cited a number of times, is meanwhile submitted to the Journal of Geophysical Research (JGR) (Syndergaard and Kirchengast, “An Abel transform for deriving line-of-sight wind profiles from LEO-LEO infrared laser occultation measurements”) where it is currently in review. So we can add it to the references and in this way people will easily find the JGR paper in a couple of months after its publication.

This ESA report itself, as well as any other scientific report cited, is part of an international report series of the institute and it is Wegener Center's policy to put all such cited institute reports permanently online under the publications webpage, so that convenient web access is ensured on a long-term basis. Many peer-reviewed papers include a few such citations, if found unavoidable, as is the case here.

Author's change in manuscript #1

We added a reference to the JGR paper in review, i.e., we changed all relevant citations in the text from “Syndergaard and Kirchengast (2013)” to “Syndergaard and Kirchengast (2013; 2015)” and added the JGR paper in review to the Reference list (in line with the AMT “Manuscript preparation guidelines for authors”

new citation added in References section:

Syndergaard, S., and Kirchengast, G.: An Abel transform for deriving line-of-sight wind profiles from LEO-LEO infrared laser occultation measurements, J. Geophys. Res. Atmos., in review, 2015

Referee comment #2

Since the Abel transform is considered foundational to this study, a short review of the Abel transform and its relevance to this study would have been appropriate.

Author's response #2

We carefully reconsidered this, but given we now have the JGR paper in the pipeline, which we co-cite throughout with the ESA report as mentioned above, we prefer not to include more details also into this AMT paper. It would be quite redundant and increase the complexity and technical character of this

manuscript without much aid to the reader for understanding the results. The purpose of this paper is really the discussion of results based on the new Syndergaard and Kirchengast (2013; 2015) algorithm; with the algorithm derivation and all related details indeed covered in the JGR paper.

Author's change in manuscript #2

None.

Referee comment #3

There are only six profiles considered in this study and these consist of three pairs. Although the authors make the case that these profiles encompass “a good diversity of atmospheric conditions” a more comprehensive set of profiles would increase the confidence in the results.

Author response #3

Yes, we agree that this data ensemble is limited but since this manuscript is an initial and first-time introduction of the IR-laser occultation wind retrieval, we decided to settle with this small selection of six representative events, also since the computational efforts for these end-to-end simulations are not small. However, we internally checked our results with about twice as many events and the character of the results, and the wind retrieval performance at the level as reported, are very robust. Future follow-on papers with further advancements/refinements will clearly invoke a bigger ensemble of events.

Author's change in manuscript #3

None.

Referee comment #4

On page 408 line 5 it is stated that “we use the clear-air conditions as context”. It is not quite clear what is meant by this statement as in the paragraph it appears it seems to imply that a number of effects are being neglected, but later in the paper (e.g. page 421 line 5) it appears that these effects are being included.

Author's response #4

At the start of this paragraph (page 407 line 26) we explicitly say “...under clear air conditions, which means...no influence of clouds but other...effects like defocusing...Rayleigh scattering...aerosol extinction...scintillations”. In that sense we think our basic definition of what “clear-air” is meant to express is clear. We agree that a few lines later, where we say “...we use the clear-air conditions as context” right after discussion of cloudiness, it might be ambiguous again. So we agree to better change at this spot to “...we avoid this part of complexity and again use no clouds, since we focus...”

Author's change in manuscript #4

Starting on page 408 line 5: ...retrievals successfully perform over broken cloudiness. In this study **we avoid this part of complexity and again use no clouds, since we focus on the integrated retrieval of I.o.s wind speed**, which has its core range...

Referee comment #5

There is a great deal of detailed explanation of the sub-processes involved in performing these calculations – along with a large number of acronyms for these modules – but these explanations did not seem to further understanding of the fundamentals of the simulations. The problem is partially that this work relies on so much else, but more thought to clarity of explanation rather than the

technicalities of the modules would aid the utility of the paper.

Author's response #5

Thank you, we have carefully reviewed the manuscript and reduced acronyms and redundancies wherever possible. For example, we removed the MAP, FOM, OSM, OPS acronyms throughout the text (spelling out as forward modeling, observation system modeling, retrieval, etc), improving readability. And we implemented a series of other simplifications.

Author's change in manuscript #5

E.g. page 419 line 26: MAP acronym removed (anyway only used once). And a number of further (small) editorial changes; we refer to the revised AMT paper.

Referee comment #6

Figure 4 is redundant and could either be combined with figure 5 or the relevant data (six sets of lat/long co-ordinates) could be included in the text.

Author's response #6

We have carefully considered merging the two figures, but given that the content is quite different we then preferred to keep the two separate figures as is. We find it provides a better flow of figures and aids to a better visualization for the reader, if it comes separately in this sequences. And we did not want to drop Figure 4 altogether since this geographic visualization really helps the reader (as compared to some lat/lon coordinate numbers in the text) to better appreciate the geographic locations where the ECMWF profiles are extracted from the global field.

Author's change in manuscript #6

None.