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Atmospheric Measurement Techniques Discussions

Interactive comment on "Profiling wind and greenhouse gases by infrared-laser occultation: algorithm and results from end-to-end simulations in windy air" *by* A. Plach et al.

Anonymous Referee #1

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General comments

As the authors state, this is the first study to analyse the performance of a realistic line-of-sight wind speed retrieval for an ACCURATE-type mission. It is more robust than an initial scheme dating from 2010, in which a constant wind speed along the observation path was assumed. The authors are very active in developing simulations and tools to demonstrate the feasibility of an ACCURATE-type instrument. Certainly this paper goes some way to demonstrating the feasibility of determing l.o.s. winds from such an instrument, although there are some unanswered issues (see below). This work is suitable for publication in AMT after the following areas are addressed.

By the very nature of the simulations carried out in this work, in which the same line parameters used to simulate the initial spectra are also used in the retrieval, the authors are neglecting systematic errors arising from uncertainties in spectroscopic line parameters. Such errors will add to the simulation errors reported in this work.

The spectral simulations are made using the Voigt line parameters from the HITRAN database. It is well know that these parameters can have errors up to 10% or even higher, and that the Voigt lineshape is only a simple approximation to the true lineshape, meaning that differences between forward-model-calculated and observed spectra are generally above the noise level. In order to extract winds from spectroscopic lines, very accurate line parameters are required. What are the spectroscopic requirements for real-world applications? The authors need to address this question.

Specific comments

Page 5, line 9: The problem in citing an ESA document here is that it is not peer reviewed and not in the public domain, meaning that the reader has no access to it. I would recommend either citing another source, or adding a summary of the main in an appendix / supplementary material.

Page 7, lines 17-18: Same comment as before. Noone has access to the referenced document. I would also prefer to see a more in depth discussion on the derivation of the Abel transorm, either in an appendix or as supplementary material.

Page 8, lines 10-15: The l.o.s. Doppler shift is smaller in magnitude than the kinematic Doppler shifts. How do typical l.o.s. shifts compare with typical uncertainties in the larger shifts? Again, note that readers will not have access to the cited mission proposal.

Page 8, lines 16-18: Give an estimate of the typical bias caused by the neglect of l.o.s. wind.

Page 9, line 3: Please define the impact parameter.

Page 10, line 4: How does the reader access these derivations? Please include them in an appendix or in supplementary material.

Page 16, lines 3-4: HITRAN 2012 has been out now for some time. Why has this not been utilised?

Page 16, lines 11-22: I find this whole section somewhat unclear. Was the pressure shift parameter for the C18OO line set to zero for the initial simulation of the atmospheric spectra? If so, this should be clarified. Please briefly describe how the tunable Tx laser design would compensate for the observed pressure shift. To achieve the best signal, the Tx laser lines must sit at wavenumbers corresponding to the inflection points of the line. Given that the true lineshape is non-Voigt (not the simple Voigt assumed by HITRAN), how easily can this be achieved? There also needs to be discussion of how the delta(k0) term compensates for the observed pressure shift. Which of these compensations is more important? Would they contribute any error to the derived winds? I note that the authors claim that ignoring the pressure shift parameter for the C18OO line is a 'reasonable choice'. Without further supporting information, it is difficult to ascertain just how reasonable this is. Certainly this is not clear to me, particularly as the entire point of this work is to demonstrate the feasibility of retrieving wind from observations made by an ACCURATE-type instrument.

Page 19, lines 14-17: It is clear that the Abel transform-type retrieval algorithm is better than the simple approach, which only assumes a constant l.o.s. wind speed. But is it the best? Have the authors looked into alternative schemes of extracting wind from spectroscopic measurements?

Page 19, lines 26-28: I would like to see some plots of the VMRs associated with these errors. How do the retrieved VMRs change with the inclusion of wind, not just their errors?

Page 20, lines 20-22: The authors claim that ECMWF short-range forecast wind fields can generally do a reasonable job. It is not clear to me exactly what they are referring

to. To setup their simulation the authors used an ECMWF analysis field for the 'true' profiles, with a shortrange forecast field providing initial/background profiles for the retrieval. But are these two sets of profiles independent enough? Would these forecasts be accurate enough for a real ACCURATE-type mission?

Technical corrections:

Page 3, line 1: Please expand the acronym ACCURATE. Page 3, line 20: contrast, not contrary Page 6, line20: Rayleigh scattering Page 15, lines 4-6: There is no need to explain the acronym EGOPS and xEGOPS twice since the names are so similar. Page 15, line 16: Planning Page 17, line 19: Remove "of the basis" Page 28-29: Define FOM and OPS in the Figure 2 caption

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