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Interactive comment

Interactive comment on "Impact of pitch angle fluctuations on airborne lidar sensing ahead along the flight direction" *by* Alexander Sergeevich Gurvich and Victor Alexeevich Kulikov

P.S. Vrancken

patrick.vrancken@dlr.de

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General: This paper aims at comparing airborne horizontal lidar measurements of aerosol clusters (or possibly hydrometeor clusters / small clouds) is the presence of beam angle fluctuations obtained during an airborne campaign with numerically modelled lidar signals for the same case. The declared purpose (in the abstract) is to give a boundary condition in order to separate lidar signal variations attributed to turbulence (molecular density variations) from variations due to beam angle fluctuations. However, the suite of the abstract and the whole paper concentrates on the effect of such beam movement on the observation of (hypothetic) aerosol clusters with limited vertical extent (such as small Cirrus). Such is also the declared goal of line 10 of the abstract to “formulate criteria” for identifying signal variations resulting from beam angles deviations probing such clusters. However, the whole paper does not deliver such criteria. It gives long introductions into cloud clusters, the measurement geometry and lidar signal analysis. Then, when comparing simulated lidar signals with real-world measurements, the paper falls short of expectations: - The assumptions taken on values of the beam movement are rather arbitrary and not well-founded as are the parameters of the aerosol “clusters” - Regarding the real airborne measurements, the flight data (from the IS, notably) are not taken into account for reference. - The individual analysis and the respective comparison are performed rather qualitatively and “by eye”, I cite here: page 13, lines 3-4 – “more distorted”, “more intense”. A quantitative statement is lacking here. - The respective chapters 4 and 5 are very short in content and discussion. - The conclusion effectively is none, because there is no quantitative measure that has been determined. The only conclusion that is valid is that such pitch variations of the sensing beam does generate fluctuations in the (normalized) signal when encountering a vertically extended object. But this is geometrically obvious. - The promised conclusion on separation of turbulence signal fluctuation (which is on the sub-percent level, Vrancken et al. 2016) from pitch/cluster variation is not given since mere arbitrary color bars are given without any notion on temporal characteristics. In addition to above described contentual shortcomings, the text also yields many conceptual mishaps, some of them described here below. Some of them may be founded in language problems, other in hastily written assertions (page 6, lines 25ff, e.g.). In summary, I suggest at least a major revision of this document with not only incorporating the points listed here, but also delivering resilient numbers. This should include a thourough study and analysis of a whole range of possible pitch variations (amplitude/time constant-bandwidth), cluster vertical extent, cluster scattering/concentration distribution and temporal evolution of the latter. This was promised in the abstract and introduction. Regarding turbulence, this aspect has either to be omitted in the abstract or also to be addressed in detail.

Specific issues: Abstract: - line 3: "the difficulties encountered" – which difficulties do you mean? It sounds like a reference to some known issue. Please specify if so, otherwise express differently. - line 8: "Uncompensated" – Within the DELICAT setup, a compensating beam steering device was used. - The subject "turbulence" is not addressed again within the whole paper I Introduction: Page1: - lines 17-19: The given references are not related to DELICAT, please refer here to Vrancken et al. Appl. Opt. 2016 - line 19: No, they are not focused - line 21: in future - line 22: air density fluctuation: the right reference would be Feneyrou et al. Appl. opt 2009, or Vrancken et al. Appl. Opt. 2016 - line 23 : "absence of >particle< scatterers" – evidently, without any scatterers (as molecules), there would be no return Page2: - line 1-2: Unclear. What "filtration" are you talking about? Why should it be impossible to cut responses from all "types" of aerosol? Filtration by interferometric methods may reduce the aerosol part to an insignificant level, filtration by molecular (I2, e.g.) or atomic absorption (Cs, e.g.) to an even lower level. This is not addressed in the given references. Veerman et al 2014 (and in more detailed description Vrancken et al. 2016) only use depolarization, which is by no means a filtration method but rather a "hint" to depolarizing aerosols (as the respective authors state). - line 4-5: What do you mean by "signal at the background level"? - line 11 "significant concentration changes in the could split" – phrase fraction – revise! - lines 14-15: "GW also impact CAT" – the relationship between GW and CAT clearly is more complex than just "impacting" – please revise! - line 15: Reference Fritts and Alexander, 2003 refers to the Middle Atmosphere, there are more appropriate references than this one, also by the same author. - line 24: "Strong laser pulses may indicate a non-linear interaction with the medium, resulting in the filamentation effect (Kosareva et al., 2006; Kandidov et al., 2009)." – This effect is of no relevance for lidar, please suppress! - lines 32-33: "Variations of sensing angles for lidars mounted on gyro-platform should be within the error limits of these compensating systems" – unclear what you mean by this. "mounted on" ?? What exactly do you mean by gyro-platforms? Aircraft IRS? This measures the angles, laser-gyros are more precise and accurate than mechanical ones. A compensating platform steers the laser beam in order to “compensate” the pitch and yaw angles of the aircraft. It also features some “noise”, as does the estimation algorithm. This adds up to the fluctuations you refer to. Vrancken et al. 2016 have shown a combination of laser-gyro IRS and mechanical beam steering with an pitch/yaw “noise” of <0.05° rms (within a certain dynamics bandwidth). Page 3: - lines 8-9: I wonder if the Klyatskin references refer to the lower atmosphere, as you state or not to some very general flow. - lines 10-14: This refers to the VERY special case of a volcano eruption – this does not describe the occurrence of such clusters in general. I suggest removing this phrase. Page 4: - line 5: “distinguish impact from the natural changes caused by wind or time evolution. “ ?? What do you mean by wind or time evolution” Evolution of what? 2 Observation model and typical scales Page 4: - lines 12-13: The particle >concentration< does not scatter the beam, please revise! Page 5: - line 1: Wrong. The position is defined by x,y ,z. The >attitude< is defined by roll, pitch, yaw. - line 10: References Huffaker and Hardesty, 1996; Inokuchi et al., 2009; have nothing to do with DELICAT. - lines 11-12: “suggest that it may be possible to observe aerosol clusters with evolution time smaller than that of the measurement time interval.” Not logic. How could that be possible to observe a phenomenon that is faster than the measurement frequency? Possibly you mean something different, but please revise! - line 14: “imaging” is perhaps not the best notion - line 16: “volume is large Lsin(..)” – omit “large” - line 19: “technical requirement”? for what purpose exactly? Within DELICAT or similar (JAXA Inokuchi, e.g.) the requirement is NOT proper sensing of aerosol clusters. Page 6: - line 10: What do you mean with the max. distance scale Lext? How is it defined by mol. extinction (which depends on mol. scattering)? - line 12: How is Lmax defined? What value is reached by intensity I? - line 15: repeated mention of tau=10ns typical for lidars (cf. line 13) - line 17: the “sensing path” does not end somewhere (Lmax), only the intensity further drops with L^2. - line 19: well, usually it is the employed laser technology that sets a limit to the repetition rate. Theoretically, the definition (which is not clear to me) of an Lmax, sets the upper bound for a high rep system. - lines 25ff. Therefore, you state, the used repetition rate should be <10kHz; granted. How does this allow for the assumption of “delta-pulses” (and what are delta-pulses?), you possibly mean Dirac function, but this assumption has nothing to do with the repetition rate, BUT with the shortness of the pulse. 10ns/1.5m is short in comparison to the considered spatial scales (of turbulence or your clusters) here → thus it is a valid assumption. You conclude another time: “assuming, therefore, the backscattered signals to be independent for each pulse”. Again, this has nothing to do with the former, but with lack of coherence between the pulses (if and only if there is no coherent relation between the pulses; true for a Q-switched laser in the case of DELICAT. Not true for some other laser types.). - General: So far, there is no consideration at all, of how fast the sensing laser beam can move from position 1 to 2 within (t2-t1). This is an important topic, since for shorter time scales the residual pitch “noise” of a compensated platform (based on an angle-resolving IRS) surely is lower than when looking on longer time scales. Reasons: The movement of the aircraft itself is not erratically abrupt, but follows its own inertia (so essentially it depends on its mass/distribution). The movements of the compensating platform also have a certain bandwidth. Where is this aspect considered in your study? 3 Modeling of an aerosol cluster lidar image Pages 7 and 8: The derivation of a lidar signal has been published very often, there is no need to perform this in this detail here, I suggest to considerably shorten it. Page 8 - line 14: here you refer to aerosol backscatter but use subscripts M and MB for molecular. Further, in particular for aerosol, there is not a “typical” value. Cf. Vrancken et al. 2016 Table 3 and in particular references therein, or Groß et al. ACP 2013 (DOI: 10.5194/acp-13-2487-2013), e.g. Page 9: - lines 19ff: The discussion of the retrieved lidar signal (Fig. 3) is rather long, to obtain such “bars” (for lifespan> Lmax/u0) is rather evident. Please try to clarify and shorten. 4 The impact of measurement direction fluctuations on cluster lidar images Page 10: - line 2: Again misconception of the term “position”. You probably mean “attitude” - line 6: here is the first notion of a time constant of this pitch variation (as mentioned above): Why do you choose 20s? Please explain/justify. - Eq. (7): I do not see any periodicity. Is there lacking a sin/cos? - line 10: Why do you choose 3 and 6°?? On page 2, you talked about a tenth of these values. (apparently, this is a typo) - line 11:

instead of “position”, better use “altitude” (if this is what you mean) - Figure4: I do not see the utility of this figure – what does it show me? To what extent does it help the paper to arrive at its conclusion? 5 Airborne lidar measurements in presence of pitch angle fluctuations Page 13: - line 24: A more comprehensive source is Vrancken et al. Appl. Opt. 2016 - line 31: Please use the 24h format, p.m. and a.m. is not used in aviation nor science (as recommended by ISO 8601) Page 14: - line 1: The altitude is 9.46km (flown pressure altitude FL310), NOT the height which is defined differently! - General: You are presenting here lidar data obtained with the DELICAT instrument. o You infer that there is no pitch fluctuation in Fig. 6a. How do you arrive at this conclusion? o What is different to Fig.6b, 10min earlier? Why do you suppose a fluctuation here? o There is no relation to any quantity (pitch fluctuation range, time constant, vertical extent of “cluster” etc.) responsible for these signal variations. Please discuss! - line 11: aerosol cluster backscatter (not reflection!) 6 Conclusions Page 14: - line 16: “are the most important factor for the discussed airborne lidar sensing scenario” – factor in what respect. Phrase is not complete. Page 15: - line 2: “We also show that LIDAR sensing ahead along the flight direction can potentially provide information about aerosol temporal evolution characteristics.” – How did you show that? It appears to me, that you primarily showed that the pitch angle fluctuation (if there is such of a sufficient level, with an appropriate time constant/bandwidth) would impede obtaining such information. Between the lines one may also suspect that the “arbitrary” pitch scanning delivers information on the vertical extent of aerosol (cloud?) clusters. Under very rare circumstances (that you should describe), despite pitch fluctuations, one may retrieve information on the temporal evolution. But I understood that this was just the purpose of this paper to derive exactly the necessary conditions. But I do not see them described here. - line 5: The numerical simulations are based on somewhat arbitrary values (pitch values and time constant), so I do not see what to conclude from them. - lines 9-10: “We formulate criteria for distinguishing this impact from the temporal evolution of atmospheric aerosol clouds” – What criteria? I do not find them. If you mean page 13, lines 11-13 – this is rather obvious.

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