

## ***Interactive comment on “A 3-D tomographic trajectory retrieval for the air-borne limb-imager GLORIA” by J. Ungermann et al.***

**J. Ungermann et al.**

j.ungermann@fz-juelich.de

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We thank the reviewers for their helpful comments and recommendation, which we used for improving the paper. We'd like to thank reviewer #1 for his attention to detail with respect to sentence structure and implied meaning. We'd like to thank reviewer #2 for pointing out that a detailed discussion of the observation geometry is missing, the addition of which improves the accessibility of our results.

In the following, we address the issues raised by the reviewers in detail except for simple typographical or technical corrections, which we simply applied.

We repeated the comments of the reviewers for convenience as indented blocks. When

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sentences of the revised paper are cited, we often applied cursive face to highlight changes.

## 1 Reply to Referee 1

### 1.1 Critical Issues

1. After reading the whole paper, we learn that trajectories are used as a-priori information, and not retrieved. I suggest the authors to change the title since as it is now it suggests that trajectories are somehow 'retrieved'

We changed the title to “A 3-D tomographic retrieval including advection compensation for the air-borne limb-imager GLORIA”. Only the abstract, the advection section and the conclusions mentioned that trajectories are used as a priori information. We added a clarifying sentence to the introduction: “A method for compensating advection *by including wind-fields from meteorological datasets as a priori information* is proposed and analysed.”

2. The retrievals are all performed perturbing only the target molecule (ozone) in the region (latitudes, longitudes and altitudes) interested by the simulated measurements. All the rest (ozone outside the sampled range, temperature, pressure and all interfering species) is kept at the same values used to simulate the measurements. This in my opinion makes the reconstruction of the ozone fields much easier and generates the misleading conviction that this technique will easily work on true observations. No attempt is made to consider possible systematic errors due to the unperfect knowledge of the sampled atmosphere.

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We did perform studies with fully unknown ozone concentrations (including the top column) that behave essentially identical to the presented results. We assume the top-column of ozone as known in the presented retrievals mainly for the 1-D retrievals, which were strongly affected by large systematic errors for the examined atmospheric situation caused by strong horizontal gradients within the top-column. It is the explicit intent of this paper to examine a simplified setup to concentrate on the artefacts and problems posed by the tomographic reconstruction compared to conventional retrievals. We are currently able to retrieve also multiple targets, e.g. temperature and CFC-11 with similar quality. The influence of pressure can probably be neglected as one can use rather accurate pressure values from meteorological forecasts or reanalysis. Also temperature is rather well-known from the same datasets. For several trace gases, one can find windows in the spectral range of GLORIA with only negligible interfering species. However, we fully agree that working on real measurements will be a different, more difficult matter than the presented results. However, the presented results provide an upper bound on the horizontal resolution, which we hope to achieve and certainly cannot surpass.

We emphasised in the paper the theoretical nature of the study and the use of optimistic conditions. In the abstract, we replaced “Consequently, it is examined by means of simulations, what results can be expected from tomographic evaluation of measurements made during a straight flight.” by “Consequently, it is examined by means of simulations, what results can be expected from tomographic evaluation *under ideal conditions* of measurements made during a straight flight.”. We added in the last paragraph of the introduction “The second part describes numerical studies assuming rather ideal conditions to demonstrate the principal feasibility of the approach and to derive first performance characteristics”. We also added the highlighted words to the first sentence of the third paragraph of the conclusion: “It was shown *by means of simulation* what performance can be expected from the tomographic evaluation of linear flight paths in combination

with a panning instrument swivelling the LOS between  $45^\circ$  and  $135^\circ$  *under ideal circumstances.*”

3. The results are all presented for the 12 km altitude. This is understandable since the amount of data produced by the retrievals is very high. However no discussion is made for the rest of the vertical range sampled by the synthetic observations. Does the 12 km altitude behaves as the others? Or is the 12 km altitude the best?

We agree that this is an oversight. The altitudes between 9 and 15 km behave very similar to the altitude of 12 km. To remedy this, we added figures of the relative error along vertical cuts for the linear flight track *“Panel (b) shows how the relative error behaves at different altitudes and is representative for the central portion of the flight. The volume below the aircraft, being devoid of measurements, expectedly exhibits the largest relative error. The volume with high tangent point density shows relative errors which are largely smaller than 5%, similar to the behaviour at 12 km. Behind the tangent points, there is also an area with relatively low errors, however with decreasing altitude and increasing distance from the aircraft, the quality drops rapidly. Due to the opacity of the air, the measurements are not very sensitive to these regions.”* and the circular flight track with perfect wind speed knowledge *“Panel (b) shows the relative error of the retrieval result compared to the true values. The reconstruction is best around 12 km altitude as this plane coincides well with the volume containing the most tangent points of measurements. But the reconstruction quality is still very good for several kilometres above and below. Only below  $\approx 9$  km, the relative error surpasses 5% in a larger volume towards the north. This is expected, because this volume is quickly blown away without having been measured by a noticeable number of images as depicted in Fig. XX.”*. For the other numerical experiments, mere textual additions describe the behaviour at other altitude levels.

4. It seems that the authors confuse the retrieval error (due to the mea-

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surement noise) with the accuracy of the retrieval (that is the difference between the true atmospheric status and the retrieved one)

We follow the mathematical definitions of absolute and relative error that are quite common in numerical analysis and also theoretical works on regularisation. This concept is not based on any assumption about error distributions and spatial or temporal correlations thereof, but is only applicable in such studies, where the true state is available.

In the revised paper, we introduce the notion of relative error upon the first mentioning thereof “The relative error (*which here and in the following is the difference of retrieval result and true state divided by the true state*) stays ...”. In this context, the relative error would correspond roughly to the sum of precision and accuracy in relative terms. As we do not include systematic effects in our simulations, the relative error is rather small. The current sources for error are indeed noise, which would fit into the precision category, and the more difficult to analyse influence of measurement geometry, flight patterns and gridding which would fall more into the accuracy category.

We added two noise error figures for the linear flight and the circular flight, respectively. The noise error is generally in the order of 1.5 ppb and thereby in the order of  $\approx 1.5\%$ . This indicates that the deviations from the true state within the region with high tangent point density is largely caused by noise whereas the deviations outside are caused by missing measurement information. We added to Sect. 5.2 “*Figure XX shows the error induced by noise added to the simulated measurements and the relative error on a vertical cutting plane in north-south direction. The noise error in panel (a) is  $\approx 1.5$  ppb in the volume with high tangent point density. This corresponds to a relative error of  $\approx 1.5\%$ , which is roughly in the same order of magnitude as the total relative error of the retrieval result in the same region. Below the aircraft and behind the volume with high tangent point density, the noise error becomes small as the result in this volume is more de-*

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*terminated by smoothing due to the regularisation constraint and less by individual measurements.” and to Sect. 7.3.1 “In Fig. XX, a vertical cut through the volume is depicted to show the behaviour at other altitudes. Panel (a) shows the error induced by the noise added to the measurements according to the linearised diagnostics. The error is large, where many tangent points are located and small, where no measurements are present. The smoothing influence of the a priori constraint reduces the influence of individual measurements and thereby the influence of noise. The vertical regularisation constraint is not very strong, so the influence of noise increases towards the top column.”.*

5. Most of the work is performed using the  $778.5 \text{ cm}^{-1}$  channel. GLORIA spectral resolution is  $1.2 \text{ cm}^{-1}$ , and in the spectral interval of that width centred at  $778.5 \text{ cm}^{-1}$  there are strong transitions of CO<sub>2</sub> (5 lines) and water. This won't change the conclusion reached in the paper, since only ozone is perturbed, but it may pose serious problems when dealing with real observations. Surprisingly, the additional channels ( $1020$ ,  $1043.75$  and  $1055 \text{ cm}^{-1}$ ) used to complement the retrieval informations are free from strong interfering species. I suggest the authors to use one of those channels as the main one.

Under the given assumptions of perfect knowledge of other gases, the presented results do not change as long as the channel has about the same transparency as the one used in the paper. We chose  $778.5 \text{ cm}^{-1}$ , as we also use it for retrievals of measurements by CRISTA-NF (Weigel et al., 2010; Ungermann et al., 2011) (higher spectral ranges were inaccessible for practical reasons) and also want to use it for the first retrievals using GLORIA to generate comparable results to these CRISTA-NF results. Only in a second step, we plan to exploit the whole spectral range of GLORIA, possibly also including spectral ranges of different optical depths. For this reason and to remain comparable with the simulations presented in the predecessor paper (Ungermann et al., 2010), we prefer to use

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the chosen channel regardless of its potential shortcomings.

6. The forward model (FM) makes use of Lookup Tables already convoluted with the Instrumental Line Shape. This makes the FM very fast, but given the spectral resolution of GLORIA, it may cause systematic errors due to the wrong evaluation of the absorption of interfering species.

We perform retrievals for the CRISTA-NF instrument using this technique with arguably good results (e.g. Weigel et al. (2010); Ungermann (2011)). For 1-D retrievals in a polar environment involving real measurements by CRISTA-NF, the standard deviation of the difference between radiations calculated by our lookup-table based radiative transport model (with ILS folded into the emissivity tables) and corresponding line-by-line calculations performed by the RFM (with ILS applied afterwards on the radiances) is between 0.1 and 0.7 percent depending on the employed spectral range. Using a simple regression scheme, this could be improved for the spectral ranges relevant for our major targets to a standard deviation of about 0.1 percent. This is in the same order of magnitude as the assumed stochastic error in gain in the paper under discussion. The same regression also removed the existing bias.

7. Only horizontal winds are considered in the work. The influence of vertical motion is completely neglected. This can affect the analyses in presence of strong vertical motion of airmasses

This is not completely true. We apply also the vertical wind speeds provided by ECMWF in the simulations. These are quite small compared to the horizontal wind speeds but not negligible. The split of the filament in the west-ward section, for example, is caused also by vertical motion as noted in the paper. However, strong vertical motions associated for example with thunder storms or strong convection will certainly pose problems, if only because it is poorly rep-

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resented in meteorological data. When planning the flight path, such regions should be avoided. Within the upper troposphere and even more so in the lower stratosphere however, strong vertical winds are less prevalent.

## 1.2 Specific comments

8. Abstract Lines 12-13 please specify better the improvements to the achievable resolution and stability of the retrievals

We replaced the sentence with a more specific “It is demonstrated that the achievable horizontal resolution in the line-of-sight direction could be reduced from over 200 km to around 70 km compared to conventional retrievals and that the tomographic retrieval is also more robust against horizontal gradients in retrieved quantities in this direction.”.

9. Abstract Lines 15 previously unused spectral samples? Unused by whom?

We clarified the meaning by replacing the subsentence by “...enabling the exploitation of spectral samples usually not used for limb sounding due to their opacity.”.

10. Introduction - I think the fact that this work is an extension of previous work should be stated clearly in the introduction

After the motivation, the last paragraph of the introduction dealt with describing the contents of this paper and notes that it is “extending the studies presented in (Ungermann et al., 2010).”. We moved this sentence to the beginning of the same paragraph to emphasise it. This fact is also already mentioned in the first sentence of the conclusions.

11. page 3807 Line 5 what do you mean by bad resolution? which kind of resolution ?



We replaced “bad” by “comparably worse spatial” to indicate the dimensionality and the relation to the spatial resolution of in situ instruments which arguably is often better.

12. page 3807 Line 6 ‘structures of interest’ Interesting for whom?

We removed “of interest” as it seems to be confusing. The structures are introduced in the paragraph before, where their relation to radiative forcing, exchange of air, etc., and thereby their relevance is mentioned.

13. page 3807 Lines 9-13 If small scale gravity waves are not measurable by GLORIA, why speak about them as an example? Moreover GLORIA has not been mentioned before, so please expand the acronym or describe the instrument before this part.

We removed the example, which also moves the first mentioning of GLORIA to the paragraph describing it.

14. page 3807 Line 16 limb-sounding measures infrared but also MW, sub mm and visible radiation (i.e. MLS, ODIN, SCHIAMACHY).

We clarified this by replacing “Limb-sounding measures infrared radiation emitted by ro-vibrationally excited molecules along the ray path or line-of-sight (LOS) of the instrument, which is directed towards the limb of the Earth’s atmosphere.” with “Limb-sounding measures incident radiation from the limb of the Earth’s atmosphere. In the case of infrared limb-sounding, this radiation is emitted by ro-vibrationally excited molecules along the ray path or line-of-sight (LOS) of the instrument.”

15. page 3807 Lines 25–30 The statement it’s true if across-track measurements are considered. If the measurements are along-track 2-D retrievals are possible

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Agreed, as is also explained in the following paragraph. We added “non-tomographic” to clarify this, even though tomography is only introduced in the following paragraph.

16. Page 3808 Lines 23-26 the sentence is too long and very difficult to follow, please rephrase it.

We split the sentence up: “The 3-D tomographic retrieval is thereby not a simple extension of 2-D tomography due to the different observation geometries between forward- or backward-looking satellite instruments and side-ways looking air-borne instruments on the one hand and due to the different carrier speeds on the other hand.” was replaced by “The 3-D tomographic retrieval is thereby not a simple extension of 2-D tomography: *first, the observation geometries differ as satellites usually are forward- or backward-looking and GLORIA looks side-ways; second, the carrier speeds and thereby the involved time-scales are quite different.*”

17. page 3810 Lines 9-11 As already said, don't you have problems with radiative transfer of lines that are well separated at high resolution but are merged into a single point by the ILS?

See above the reply to the critical issue 6.

18. page 3810 Lines 26-27 A constraint in a retrieval ALWAYS introduces a bias in the result. It's entity depends on the strength of the constraint.

Seemingly, the word 'bias' does not convey our intentions. The effect of the constraint is quite difficult to capture in a few words and we wanted to refer to a “bias” of absolute values in contrast to smoothening (i.e. a “bias” in the first derivative). We forego that distinction and express our intent positively by “The added constraint stabilises the inversion at the cost of a bias that depends on the type and strength of the constraint.”

19. Section 3.1 - The computation of numerical derivatives is not a common procedure for atmospheric retrievals. Usually analytical expressions are used, for example exploiting the Curtis Godson approximation and the use of LUT. The method described in the appendix is one solution, but not the unique one.

JURASSIC2 has no manually generated analytical derivative, so we did not mention this option in the main text to be brief. In effect, an analytical derivative is nearly equivalent to our approach of algorithmic differentiation, only that the compiler does all the implementation work. Another publication is in preparation dealing with this topic in detail from a computer science perspective, where the different approaches are more comprehensively discussed. Numerical differentiation is cheap to implement, but costly to compute. Analytical differentiation is difficult to implement, but cheap to compute. Ideally, algorithmic differentiation is both rather cheap to implement and cheap to execute. We added “*Another, more common, approach is implementing an analytical derivative of the model, which is computationally very efficient but requires elaborate, manual adjustments to the model.*” This was already mentioned in the appendix but probably deserves to be also mentioned in the main text.

20. Page 3812 lines 22 and 23 - The inverse of the diagonal elements of A give the resolution in which units? For instance if no constraint is used the diagonal element of A are all unity, and their inverse is 1 therefore the resolution is 1 (sampling steps?). Please specify

Just as the entries of the averaging kernel matrix are dimensionless, so is the reciprocal of the diagonal entry. One can however relate it to the spatial dimension via multiplication with the sampling grid distance, if this is uniform in the surroundings. By multiplying it with the sampling grid distance, one derives similar values as the FWHM measure (e.g. Rodgers, 2000). We clarified and added a cite to the original paper introducing this measure: “The simplest resolution measure is the

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inverse of the diagonal entries of  $\mathbf{A}$  (Purser and Huang, 1993). This resolution measure indicates the spread of information in relation to the sampling grid, but lacks a directional component.”

21. Page 3813 lines 1-7 The whole paragraph is not very clear. In particular a sphere has only one diameter by definition. Therefore clarify what you mean by 'smallest diameter'. Again why since the vertical resolution is smaller the diameter is a measure of the horizontal resolution? Please clarify.

More correct, and probably clearer, is “diameter of the *smallest* sphere” instead of “smallest diameter of the sphere”. This sphere usually has a diameter of more than 30 km. This means that it encompasses all points vertically above and below. Insofar, the vertical resolution obviously cannot not affect the diameter. Consequently, it is determined by the horizontal resolution. We try to explain this better as “*Due to the difference in scale between horizontal and vertical dimensions, the extent of the sphere is entirely determined by the horizontal spread of the elements of the averaging kernel matrix row. Thus, the diameter can be interpreted as a measure for the horizontal resolution.*”

22. Page 3813 line 8. Gradients affect trace gases as well as temperature. We replaced trace gases by “atmospheric quantities” to capture, for example, temperature, pressure, or aerosol.
23. Page 3813 lines 25-26 there must be some typos because the sentence as it is is not clear.

We broke up the sentence as: “which resembles closely the usual form of linearised diagnostics (Rodgers, 2000). The main difference is the exchange of the true state  $\vec{x}_t$  and the Jacobian matrix  $\mathbf{F}'(\vec{x}_f)$  by representations including an additional dimension.”

24. Page 3814 lines 8-10 what do you mean by 'computationally much less involved'?

We elaborate: "as it requires only a single evaluation of the higher-dimensional Jacobian matrix and some matrix-matrix multiplications in contrast to a full non-linear retrieval."

25. Page 3814 lines 12-14 please explain better why this is the only method to incorporate the effects of horizontal gradients

The word "only" should express the difference to the conventional way of using a 1-D averaging kernel matrix. Obviously, there may be further methods. We removed the word "only" as the sentence becomes correct thereby.

26. page 3815 line 9. What do you mean by 'starting at 0.73 deg going'?

Some part of the sentence seems to be missing. We added "starting at 0.73° pointing upwards and going down from there on."

27. page 3815 lines 15-17 Did you try to analyse spectra simulated with the Reference Forward Model or using LUT without convolution with the ILS?

Using radiances generated by the RFM for the simulated measurements would be one way of generating more realistic estimates for the effect of systematic errors of the forward model. However, due to the significant computational effort of executing such a calculation with the RFM, we refrained from doing so. For handling systematic errors, we want to concentrate on evaluating real measurements, which we will acquire in autumn/winter for the first time. The tomographic approach works with any 3-D radiative transfer model, so more accurate models can be used should this become a major issue.

28. page 3815 line 23 add the sentence 'and disregard systematic errors' (see general comment above)

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We noted already in the preceding paragraph, that our measurements or unaffected by systematic errors. We still modified the sentence to the reviewer's suggestion.

29. Section 5.2 - For the 3-D retrieval did you use only the measurements pointing North or all the measurements? If the latter is true, you are analysing a lot more measurements, and that is one of the reasons for a robust 3-D retrieval

The 3-D retrieval uses all measurements. This helps the retrieval, however, the observation geometry with overlapping field-of-views from different viewing angles is much more important than the number of measurements. Reducing the number of measurements affects the resolution of the result more than the stability (for the given noise level). Obviously, there is a breaking point. Ungermann et al. (2010) examined this effect for a circular flight, by comparing a setup where the same number of measurements was used, once with panning and once without panning.

30. page 3817 line 8. You speak about relative error, but you never defined what relative error is. I assume it is retrieved minus reference in percent.

We added: "The relative error (difference of retrieval result and true state divided by the true state) stays. . ."

31. Page 3818 line 11 - what do you mean with 'deliver'?

We tried to explain it better as: "the two horizontal resolution measures . . . generate similar values".

32. Page 3818 lines 14-18. Not very clear, please rephrase the sentences.

We rephrase as: "Panel (b) shows that the dislocation is small within the well-resolved region. This indicates that there is sufficient measurement information to

reconstruct a spatial average of the true atmospheric state in this region. Outside this region however, there is an increase in dislocation, which indicates that the result there is largely derived by extrapolation from the well-resolved region.”

33. page 3819 lines 12-13 the fact that the volume towards the north shows lower improvement is due to the fact that lies after the tangent points, therefore the opacity of the atmosphere makes this region poorly known.

We find in our simulations that the intuitive thought that the atmosphere can be well reconstructed where the Jacobians assume their largest values, is asserted. Insofar the presented results are not surprising. However, there is also an improvement behind the tangent points, even though it is smaller. Also the diagnostics indicate a smaller smoothing in this region, which we did not expect to happen. We added “Due to their decreased optical depth, the added channels are to varying degrees more sensitive to the atmosphere in front of the tangent points, where we expect the largest improvement.”

34. Section 7 - All the test reported in this section are performed using a single channel or multi- channels?

Only section 6 deals with multiple channels. We added a remark to section 6 that the additional channels are only used for this section only. We also added to the introduction of Sect. 7 a remark that the setup of Sect. 5 is used except for the noted changes.

35. page 3820 lines 4-13 the whole paragraph is rather contorted, please make it clearer.

We tried to clarify: “This study assumed a static atmosphere so far. Now the effect of a time-varying atmosphere on 3-D tomographic retrievals is analysed. While the time required to acquire the measurements is usually negligible for a conventional 1-D retrieval, the atmosphere might change significantly during the time



required to fly a tomographic measurement flight, e.g. a full circle with 400 km diameter. The effect becomes the larger, the longer the timespan between two measurements of the same air mass becomes. Thus, a closed, circular flight path is a worst-case scenario, as basically all measurements are affected by the same enclosed air mass.”

36. Section 7.1 - This is the first time that we learn that advection is taken into account as a-priori information.

In addition to the reference of this fact in the last sentence of the abstract, we also modified the last sentence of the introduction to “A method for compensating advection by including wind-fields from meteorological datasets as a priori information is proposed and analysed.”

37. Page 3822 lines 4-9. Is the time step size related to the interpolation used to get the wind data? Here you speak about lookup-tables, but which lookup-tables do you refer to? Spectral or trajectories? If trajectories, you should say that you use trajectory LUT before this paragraph, not after

We clarified this by moving the first reference to the (advection) LUT to the following paragraph. I.e. the sentence “Similarly, the effect . . . studies” is moved to the end of the following paragraph, slightly adjusted to “The effect of the *time grid employed for the advection* lookup tables was estimated and a 300 s *grid* is used for the following numerical studies.”

38. Page 3822 line 26 The synthetic measurements have been generated with a 3-D or 4-D atmosphere?

The atmosphere is referred to as static, implying that there is no difference between a 3-D and 4-D forward calculation. So 3-D calculations were used for simplicity's sake.



39. page 3823 lines 3-4 again you speak of relative error without defining exactly what you mean by that.

We introduced “relative error” at the first instance mentioning it.

40. page 3823 lines 26-28 I do not understand the whole sentence. Please clarify

We rephrased as “The 4-D analysis shows that each measurement contributes an average of a not very well localised volume. Only the combination of the contribution of many such measurements increases the weight of the relevant element and lessens the weight of the surrounding elements.”

41. Page 3824 line 3 What do you mean by ‘remnants of LOSs’?

We expanded: “While still comparably well localised, some *parabolic artefacts* can faintly be seen in panels (a) and (b). *These are located close to the points of maximum sensitivity of the LOS of the contributing measurements or at regions, where many contributing measurements are sensitive. Such artefacts arise, when only a few measurements are available that have their maximum sensitivity in the relevant space.*”

42. Page 3824 line 6 What do you mean by ‘sweep over the data point’?

We rephrased “where tangent points of measurements sweep over the data point.” by the hopefully more precise “where *measurements are taken, the tangent points of which are close to the retrieved data point.*”

43. Page 3824 lines 12-13 The first sentence of the paragraph is not clear. Please clarify the conclusion

We replaced “Concluding, the smoothing in time emphasises those times when tangent points of measurements wander over the data point.” by “Concluding, the 3-D reconstruction generates a weighted temporal and spatial average of the

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time-varying true atmospheric state. Thereby, the atmospheric state at those times and places where the sensitivity of measurements is high (mostly close to the tangent points) contribute most to the final reconstruction.”

44. Page 3824 lines 23-24 Figure 12 shows only one of the atmospheric situations listed here

We replaced “The impact of the wind on the atmospheric state can be seen in Fig. 12, which shows the atmospheric situation at the beginning (first measurement) and at the end (last measurement) of the simulated flight.” by “The impact of the wind on the atmospheric state can be seen in Fig. 2, which shows the atmospheric situation during the first measurement, and Fig. 12, which shows the atmospheric situation during the last measurement of the simulated flight.”

45. Page 3824 line 27 What do you meas with: 'The use case at hand'?

We replaced “The use case at hand” by “This scenario”.

46. Page 3825 line 8 Why this setup 'demonstrate how well the retrieval can compensate for advection' if you use perfect knowledge of it?

Well, it “demonstrate how well the retrieval can compensate for advection *with perfect knowledge about the wind*”. Before demonstrated, it is not apparent to us that the retrieval will work even under such ideal circumstances. As the retrieval is a non-linear process, there could be all kinds of issues. For example, that the well-reconstructed volume is as large as it finally is was not fully expected by us. Further, as this is an most ideal scenario, any obvious artefacts would point to deficiencies and limitations of the method and instrument. It also serves as a kind of upper bound for achievable horizontal resolution for retrievals using real measurements, so we know what we might strive for resolution-wise when designing flight patterns in the upcoming campaign.

47. Page 3825 line 25 "it is expected that those air masses'. Which air masses are you referring to?

We refer to the airmasses mentioned before in that same sentence. We replaced "those" by "these" accordingly.

48. Page 3826 line 3 what do you mean with 'are even qualitatively well given'?

We mentioned "quantitatively". The static 3-D reconstruction did not produce very good results for the airmass southward of the circumflown area. However, the drop in ozone was shown, but only qualitatively, as the ozone concentrations shown quantitatively larger and larger errors. These airmasses are now driven by the wind into the circumflown volume, but are present there only during part of the measurement flight. However, the "even" is rather superfluous and we removed it, as an error of less than 2 percent is certainly a good agreement (even under the error assumptions of the study). The context should now be clear from the following sentence.

49. Page 3826 line 23 You say slightly surprisingly you have no convergence problems. I assumed that convergence was not a problem in this case, since the retrieval will produce averaged fields that may cause a high chi-test but not a convergence problem.

We misused the word "convergence", which does not carry any inherent implication about the quality of the solution. We tried to clarify by replacing "Slightly surprisingly, this setup had no convergence problems and delivered a result within the usual number of iterations." by "Slightly surprisingly, this setup did converge to a physically meaningful atmospheric state within the usual number of iterations."

50. Page 3827 lines 1-16 You use a whole paragraph to say something that is expectable given the retrieval set-up. I will shorten the whole section.

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also because you draw similar conclusions to the ones already reported in section 7.2

The analysis of section 7.2 was generated using a static atmospheric state. So given that the problem is only slightly non-linear, it is probable that this conclusion carry forward to the case with a 4-D forward calculation, but not certain. Especially, it is not certain that the retrieval will converge to this solution; it might also be stuck at the initial guess or converge to something else. However, we agree that we can shorten the paragraph: “ As discussed in Sect. 7.2, the 3-D retrieval without trajectory model compensation averages the true atmospheric state over time. So comparing the result with the state at 12:00 UTC might not be the best frame of reference. Instead a uniform average of the true atmosphere over time might be better suited as is shown in Fig. 16. The averaged atmosphere resembles rather closely the retrieval result in Fig. 15a within the volume covered by tangent points and also towards the south, even though Sect. 7.2 showed that the average is non-uniform. This implies that the 3-D retrieval without any compensation of advection still generates reliable results, even though they provide only a time-averaged state.”

51. Page 3828 lines 18-20 Very bad sentence. What do you mean with 'state of affairs'? Section 7.3.3

We replaced “While the resolution in the centre of the volume is quite bad due to the large averaging in time (and correspondingly in space), the outer parts of the volume represent mostly the state of affairs at the time when the instrument is closest.” by “While the resolution in the centre of the volume is quite bad due to the large averaging in time (and correspondingly in space), the outer parts of the volume represent mostly the state *of the atmosphere during the time when the instrument is closest.*”

52. Page 3829 line 1 which units is 0.87?

As a ratio of wind speeds 0.87 is dimensionless. We added the unit “ $\text{m s}^{-1}/\text{m s}^{-1}$ ” to make this clear.

53. Page 3829 lines 23-26 the whole paragraph is not very clear. Please rephrase it

We rephrased “Comparing this averaged speed with the true speed used for generating the simulated measurements in Fig. 12 it becomes evident that the meridional speed is quite a bit too large as within the centre of the circle described by the instrument the true meridional wind speed is close to zero.” as “The averaged meridional wind speed is thereby quite larger than the true wind speed (see Fig. 12) in the centre of the circle described by the instrument”

54. Page 3830 line 21 calculate the Jacobian matrix ‘algorithmically’?

The “automatic differentiation” tools are not completely automatic to use so that the community nowadays tends to explain the AD abbreviation as “algorithmic differentiation”. We refer to that meaning of AD, which however is not clear and also superfluous. We removed the offending word.

55. Appendix - I found the appendix very difficult to follow and understand.

We reworked the appendix. The changes are subtle but rather pervasive, so they are not repeated here. We hope that it is now easier to follow. We generally tried to simplify long, complicated sentences within the whole paper.

## 2 Reply to Referee 2

### 2.1 Major concerns

56. This paper does not deal with the quality of the ozone that can be retrieved with GLORIA. It attempts to concern itself only with improve-

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ments in spatial resolution and accuracy given the inclusion of more dimensions in the retrieval scheme and its associated forward model. There are a few major yet easily addressable issues associated with this statement.

1) This fact should be made very clear from the start. The title, the abstract and the text should be modified to reflect that this work is associated only with the benefits gained when adding additional dimensions to the retrieval. This paper does not support or refute the fact that GLORIA measurements can be used to retrieve ozone.

We agree with the referee that this paper is concerned with the quality and benefits of 3-D retrievals compared to 1-D retrievals using a GLORIA-like instrument. We do not care so much about the trace gas used as example and do not strongly state that GLORIA will behave as shown in the simulations, but focus more on the improvements gained over simulated 1-D retrievals. We thought that the title, abstract and text already reflect this fact and are hard pressed to find statements with respect to ozone before rather deep into the paper in the section “Simulation Setup”. However, we made several small changes, sharpening our statements with respect to the improvement of spatial resolution and robustness against horizontal gradients. Within the conclusions we de-emphasised the use of ozone to make our point more clear.

We do not feel that the title of the paper does not fit our intent. We present a 3-D tomographic retrieval that incorporates wind-fields as a priori information that enables the 3-D evaluation of GLORIA measurements. We also evaluate benefits by this approach compared to basic 1-D retrievals. As referee #1 also requested a change of the title, we currently use “A 3-D tomographic retrieval approach including advection compensation for the air-borne limb-imager GLORIA” as a title. Adding, for example, “Increasing the spatial resolution and stability against horizontal gradients in the atmosphere by...” to the title would make it more

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precise, but quite unwieldy.

We made several small changes to the document, focusing on the abstract and the conclusions to make our point more clear. As these are pervasive and mostly rather subtle, we do not repeat them here. For example, we changed the last sentence of the conclusion from “The numerical studies show a vertical resolution in the order of 300m and a horizontal resolution in the order of 30km for ozone, including linear flight paths, which may be more frequent than tomographic circular flights, and in the presence of advection.” to “The numerical studies show a vertical resolution in the order of 300 m and a horizontal resolution in the order of 30 km, *for both linear flight paths and circular flight paths in the presence of advection.*”, putting the spatial resolution for the two examined main scenarios in the foreground.

57. 2) Tomographic retrievals are all about the sampling geometry. This is not adequately reflected within this paper. The presented results apply only to limb measurements of the specific resolution associated with the study. This should be made very clear. The authors attempt to address the sampling geometry by including contours and tangent points on plots but they never address the sampling geometry in detail. I suggest a new section in the paper that properly addresses the sampling geometry in terms of things like the number of and tangent altitude separation of measurements in each image, the image rate in terms of the azimuthal scan rate, the sampling density within each grid cell (not just the tangent point density), . . . This should be done over the entire retrieved volume, not just at twelve km, and it should involve a detailed discussion of the spatial size of the volume element associated with the retrieval.

We try to cover this more comprehensively.

We added “*This corresponds to a spatial distance of tangent points of 130 m 2 km*”

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below the aircraft, a distance of 280 m 5 km below the aircraft, and a distance of 400 m 10 km below the aircraft. The vertical FOV of two rows is assumed to have a full width at half maximum (FWHM) of  $0.08^\circ$ . Thereby, the FOV of two vertically neighbouring measurements overlaps meaningfully. The horizontal FOV is  $4^\circ$ ." to the "Simulation Setup" section.

The following text is added to Sect. 5 (Linear flight path) including two figures visualising the sampling: "With a sampling rate of one image consisting of 64 individual observations every three seconds, one full azimuth scan takes 66 s. With the given flight track length, the flight allows for 54 full azimuthal scans with 1 408 observations each. Altogether these are 76 480 radiance values. To account for the effect of the FOV, 458 880 radiative transfer calculations are employed. The atmosphere is comprised of volume elements, which extend  $11 \text{ km} \times 11 \text{ km} \times 0.25 \text{ km}$  in the depicted regions. In Fig.XX, the sampling and sensitivity of the atmosphere with respect to the simulated measurements is depicted. This cutting plane is representative for the volume except for the beginning and end of the flight, where less overlapping measurements are available. Panel (a) shows the number of measurements influenced by each volume element. The number of measurements traversing a volume element decreases with increasing distance from the instrument as the measurements vertically separate. The number of  $\approx 300$  measurements northwards of  $45^\circ \text{ N}$  per volume element is roughly consistent with each measurement passing through  $\approx 3 \times 7$  (horizontal  $\times$  vertical) volume elements in 200 km distance due to FOV; in addition,  $\approx 15$  images are taken with different azimuth angles while traversing the 11 km side length of each volume element. As also minor contributions count as influence here, panel (b) shows the sum over the absolute values of the corresponding column of the Jacobian matrix. High values are a necessary, but not sufficient, precondition for a good reconstruction quality. One can see the highest sensitivity is given in the measured airmass between the aircraft and the volume of high tangent point density, as the employed channel is not fully transparent."



To the introductory part of Sect.7, the following sentences are added to the second paragraph: *“For this setup, 1775 images are taken, summing up to 113600 individual measurements. This requires the casting of 681600 individual pencilbeams to capture the effect of horizontal and vertical FOV. A volume element in the central, depicted region of the simulated volume is again  $11\text{ km} \times 11\text{ km} \times 0.25\text{ km}$ .”*

A new paragraph is added to Sect. 7.3 just before the start of Sect. 7.3.1: *“Before proceeding to the numerical experiments, the distortion of the spatial distribution of measurements by advection is examined. Figure XX depicts a vertical cut in north-south direction through the atmospheric state at 12:00. The colouring indicates, how often measurements pass through each volume element taking into account their displacement by advection. One can see clearly that airmasses towards the south are measured more often than those towards the north, especially at lower altitudes. With the prevalent direction of wind, this is expected. Still, the distribution of measurements is quite even so that a good reconstruction should be possible, except for the volume northwards of  $47^\circ\text{ N}$  below  $\approx 10\text{ km}$  altitude, where only few measurements pass through the volume elements.”* A corresponding figure was also added.

58. This paper only simulates results associated with one ozone field and only presents results at 12 km. It is not clear to me, and likely it will not be clear to most readers, whether or not the results will be as good (or as bad) with other ozone fields and at other altitudes. The quality of the results at other altitudes and with other ozone fields must be addressed within the paper in a significant manner.

As already noted with respect to a issue 3 by referee #1, we added vertical cutting planes depicting the relative error at different altitudes for both the linear flight and the circular flight including an extensive discussion of the results at other altitudes. To reduce the amount of figures, we restricted ourselves to one figure each and

only discussed the behaviour of other numerical experiments at other altitudes in the text. As the behaviour at different altitudes corresponds very well to the behaviour at 12 km, this should be sufficient (i.e. an improvement/worsening at 12 km corresponds to a similar improvement/worsening at other altitudes).

With respect to other ozone fields, we have done simulations for other ozone fields and also, more recently, other trace gases. For the same circumstances (single target, no systematic errors), the results are comparable. In effect, the presented use case is one of the most difficult due to the strong horizontal gradients and the large contribution of radiance from the top column (which we do not need to assume known for it to work!). Presenting an additional use case would therefore not add anything to the paper. We already noted in the simulation setup that we chose this scenario because it is a worst case scenario with respect to trace gas distribution. We added the following sentence to Sect. 4 “Simulation Setup” to make this more clear within the revised paper: *“We choose ozone as test target, as it is a comparatively difficult trace gas to retrieve for an airborne limb-sounder because of the large contribution of radiance from the atmosphere above the instrument. While we assume the top-column of ozone to be known in the presented simulations, the tomographic retrievals produce essentially the same results below 15 km altitude for an unknown ozone top-column. We tried different scenarios from GEM-AQ and also CLAMS (Grooß et al., 2005) datasets giving similar results as presented below. The presented scenario is basically a worst-case scenario due to large horizontal gradients at basically all altitude levels combined with a dominant wind speed orthogonal to the filament extent.”* As discussing further scenarios in detail would not add new information but require a significant amount of additional figures and corresponding description, we’d like to skip their inclusion in this paper.

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## 2.2 Minor concerns

59. Does the case of circular flight path with exact knowledge of the advection give the exact same results as a case with no advection? This should be mentioned.

No, it does not give the same results. The result of a static retrieval without any advection is shown in Fig. 9 while the result for a retrieval with advection in both forward simulation and retrieval is shown in Fig. 13. The differences between the two are discussed in Sect. 7.3.1, second paragraph. As different airmasses are measured by the images of the static/advectioned setup, different airmasses are well resolved.

60. Part of figure 12 appears to be missing in my copy.

There was a mistake in the text referring to the atmospheric state at the begin of the measurement period within that Figure. This has been corrected by pointing out that the atmospheric state at the begin is shown in Fig.2 and the state in the end is shown in Fig.12. See also item 44.

61. Are the authors using 1 or 4 optical channels for most of the simulations. This should be made clear.

Only section 6 deals with multiple channels. We added a remark to section 6 that the additional channels are only used for this section only. We also added to the introduction of Sect. 7 a remark that the setup of Sect. 5 is used except for the noted changes.

62. On page 3826 in line 1 there is a statement that says 'This is indeed the case as Fig 13 demonstrates.' I believe the authors are referring to the low relative errors within the circle but when I look at the retrieved field presented in panel a) it doesn't look like any input presented within

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the paper. I assume this is because it is time average or something like that but Fig 13 doesn't demonstrate the statement without more information.

We did not note the frame of reference properly. The true atmospheric state serving as reference here was depicted in Fig.2. And the difference of relative errors is referred to a static setup without advection depicted in Fig.9b, where there are large error >9% in the south-western corner. Some of this information is brought to the attention of the reader later in the paragraph, but seemingly to late. We reorganised the begin of section 7.3.1 to

“The first experiment takes advection into account for generating the measurements and also uses perfect knowledge about the advection in the retrieval process. As the advection moves air from southwards of the flight track into the circle described by the instrument, it is expected that these air masses are better resolved, possibly at the cost of reduced resolution for air lying towards the north within the circle *compared to the retrieval experiment devoid of advection presented in Fig. 9.*

*The results of such a retrieval, where the forward calculation and the retrieval were both calculated with the same wind fields is presented in Fig. 13. Comparing Fig. 13b to Fig.9b, the southern boundary of the ozone filament is much better reproduced. ...”*

63. On page 3827 there is a statement that says the 'average atmosphere resembles rather closely the retrieval result ...'. 'Rather closely' is a matter of opinion and I don't think I share the opinion. I would suggest that the retrieved results 'more closely resemble' but they are still not good enough in the entire volume to state they 'resemble rather closely ...'

We follow the suggestion of the referee.

64. I think the statement on line 11 page 3830 needs to be softened. It has in no way been shown that inaccurate wind information can improve the retrieval. It has been shown that for this one case it doesn't significantly decrease the accuracy of the result.

We agree that one cannot prove anything by example. However, the three experiments suggest that there is a rather linear correspondence between accuracy of employed wind speeds and retrieval quality. We weakened the statement “This indicates that even inaccurate information about the prevalent wind can be used to improve the retrieval result.” to “This *suggests* that even inaccurate information about the prevalent wind can improve the retrieval result *as long as it captures the main features of the flow*. However, this needs to be examined on a case by case basis.” and also the corresponding statement in the conclusions “Lastly, it was shown that also flawed wind speed information can improve the retrieval results to some extent.” to “Lastly, it was shown that also flawed wind speed information *could* improve the retrieval results to some extent.”

## References

- J.-U. Grooß, G. Günther, R. Müller, P. Konopka, S. Bausch, H. Schlager, C. Voigt, C. M. Volk, and G. C. Toon. Simulation of denitrification and ozone loss for the arctic winter 2002/2003. *Atmos. Chem. Phys.*, 5:1437–1448, 2005. doi: 10.5194/acp-5-1437-2005.
- R. J. Purser and H.-L. Huang. Estimating effective data density in a satellite retrieval or an objective analysis. *J. Appl. Met.*, 32:1092–1107, June 1993. doi: 10.1175/1520-0450(1993)032<1092:EEDDIA>2.0.CO;2.
- C. D. Rodgers. *Inverse Methods for Atmospheric Sounding: Theory and Practice*, volume 2 of *Series on Atmospheric, Oceanic and Planetary Physics*. World Scientific, 2000.
- J. Ungermann. *Tomographic reconstruction of atmospheric volumes from infrared limb-imager measurements*. PhD thesis, Wuppertal University, 2011.
- J. Ungermann, M. Kaufmann, L. Hoffmann, P. Preusse, H. Oelhaf, F. Friedl-Vallon, and

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- M. Riese. Towards a 3-D tomographic retrieval for the air-borne limb-imager GLORIA. *Atmos. Meas. Tech.*, 3(6):1647–1665, 2010. doi: 10.5194/amt-3-1647-2010.
- J. Ungermann, C. Kalicinsky, F. Olschewski, P. Knieling, L. Hoffmann, S. Hofer, J. Blank, W. Woiwode, H. Oelhaf, M. Volk, A. Ulanovsky, F. Ravagnani, K. Weigel, F. Stroh, and M. Riese. CRISTA-NF measurements during the RECONCILE aircraft campaign with unprecedented vertical resolution. *Atmos. Meas. Tech. Discuss.*, 2011. in preparation.
- K. Weigel, M. Riese, L. Hoffmann, S. Hofer, C. Kalicinsky, P. Knieling, F. Olschewski, P. Preusse, F. Stroh, R. Spang, and C. M. Volk. CRISTA-NF measurements during the AMMA-SCOUT-O<sub>3</sub> aircraft campaign. *Atmos. Meas. Tech.*, 3(2):1437–1455, 2010. doi: 10.5194/amt-3-1437-2010.

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