

Interactive
Comment

Interactive comment on “Measurement of atomic oxygen in the middle atmosphere using solid electrolyte sensors and catalytic probes” by M. Eberhart et al.

M. Eberhart et al.

eberhart@irs.uni-stuttgart.de

Received and published: 24 June 2015

We would like to thank the referees for their efforts in critically commenting our manuscript. The questions and suggestions haven been picked up and considered thoroughly for the revised version of the paper. All major points noted by the referees and some of the minor points requiring a detailed answer are addressed below.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

1 Reply to Anonymous Referee #1

1. Yes, we have considered the possible effects of both $O_2(^1\Delta)$ and ozone on the sensor signal. A series of experimental investigations has been carried out to determine the abundance of these species. For ozone the measurements could be done with classical mass spectrometry (quadrupole mass spectrometer, QMS) on mass number 48, for $O_2(^1\Delta)$ the procedure is more complicated and requires to scan the electron energy in the QMS ionizer to reveal the presence of excited molecules. For both species it could be confirmed that they are not present in the setup used for sensor calibration. Ozone is not produced directly in the discharge but only in its afterglow from recombining oxygen atoms. In our case this afterglow is very short as the plasma products are directly expanded into high vacuum. The transition from the discharge tube into the calibration chamber requires the molecules to pass through a relatively long (2.5 mm) and narrow (\varnothing 0.3 mm) orifice, where multiple wall collisions take place. This is likely to deactivate the $O_2(^1\Delta)$.

Though this influence on the sensor signal could be ruled out we agree that it is of interest to the reader to discuss this possibility. We have included a section about uncertainties and sources of error in the discussion part of the paper where we detail the described findings and also discuss possible influences on the sensor in cases where ozone and $O_2(^1\Delta)$ are present. On the other hand it is important to consider that both mentioned species actually exist in the MLT region, albeit with lower concentrations than expected in a microwave discharge. Atmospheric measurements have been made e.g. in the NLC-93 campaign (Gumbel et al., JGR, Vol. 103, No. A10, pp. 23,399-23,1998). We have inserted this fact in the section about possible sources of error.

2. In order to investigate the possible influence of a non-linearity in the sensor characteristics not covered by the calibration we have altered Figure 16, the compar-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



ison between fore/wake- and aft/ram-results during downleg. Instead of scaling the wake-values by an arbitrary factor of 3.0 we have applied the actual aerodynamic correction factors derived by the numerical simulation. The O density profiles then obtained by the wake sensors are about 6 times higher than the ram-results. This implies that the densities derived from high sensor signals are underestimated compared to those that result from the low signals in the wake. This could be explained by a non-linearity towards the upper signal levels.

As a further possible source of error to the absolute values not described in the initial manuscript we have identified the sensor voltage control in conjunction with electrodes made from different materials. This can (and did) lead to voltages that differ from those applied during calibration. Although a quantification of the effect on the sensor is difficult we estimate a possible reduction of the signals of 50% due to this effect.

We have detailed the discussion about these points in the section dealing with uncertainties and error sources.

Below 80 km the signals suffer from a very high noise level on the downleg (beginning instability in the flight state, flatspin) and from unstable initial conditions after nosecone ejection on upleg. For the sake of clarity these data have not been plotted in the diagrams. In the manuscript we have detailed the description of this unstable beginning phase that is marked by divergent profiles obtained from the single sensors. It continues on the upleg up to about 90 km. We have added the recommendation to house the sensors in an evacuated hood that is removed together with the nosecone/motor. This would minimize the desorption processes that are the cause of the observed instability and could yield reliable data right after the sensors are exposed to the atmosphere.

3. In fact, the results for the O concentration below 82 km coincide well with the values measured in the NLTE-campaign. A more appropriate comparison is the NLC-93 campaign, launched in geophysical conditions similar to the WADIS-1

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

flight (July-August, night-time but sunlight due to high latitude) (Gumbel et al., JGR, Vol. 103, No. A10, pp. 23,399-23,1998), which showed according results for this altitude. The values at 80 km are given as $3.2 \times 10^9 \text{ cm}^{-3}$ (NLTE), $2.4 \times 10^9 \text{ cm}^{-3}$ (NLC-93) and the present data shows $3.4 \times 10^9 \text{ cm}^{-3}$ for the aft sensors during downleg. However, the rise in O density to the peak value measured at around 95 km is smaller than in the mentioned references, and we suspect again a non-linearity in the sensors characteristics to be responsible for this underestimation of the high densities.

This argument is now included in the discussion part of the manuscript.

4. This “bite-out” in the profile and the divergence of the curves from the single sensors occur during the beginning of the upleg and are most likely due to the instability of this initial phase, as discussed in the reply to point 3. The aft sensors track well because the results stem from the downleg measurements, where the sensors are in stable condition.
5. The PHLUX profile was shifted by 16 km in an attempt to find coincidence with the FIPEX curves. The reason for this shift is yet unclear. Analysis of the PHLUX data was complicated by the (unplanned) sunlight during the flight and has to be deepened to be able to properly interpret these result. Following the recommendation of the second referee we left the presentation of the PHLUX data for a future companion paper focusing on this instrument and restricted ourselves to the explanation of the measurement principles.

2 Reply to Anonymous Referee #2

1. We agree that even though (or *because*) this paper describes the very first application of solid electrolyte sensors for the measurement of atomic oxygen on sounding rockets it is desirable to learn more about the accuracy and possible

sources of errors associated with this technique. For the calibration the main uncertainty is due to pressure measurements with a cold cathode gauge (accuracy $\pm 30\%$); additional uncertainties arise from a possible shift of the electron energy in the ionizer of the mass spectrometer that would lead to an underestimation of the O densities by 8%. We have introduced appropriate error bars in the calibration curves and show the related range of uncertainty in the processed density profiles.

The fact that the upper end of the sensor signals that occurred along the trajectory could not be covered by the lab calibration leaves an uncertainty due to a possible non-linearity towards higher signal levels.

During the flight we identified the control of the sensor voltage U_S , in conjunction with electrodes made from different materials, as a key source of error. The cathode is gold plated while the other electrodes have a platinum surface, and this leads, through the back door, to a dependency of the sensor reactions on the present molecular oxygen. This could affect both the qualitative and quantitative results of the measurements. We estimate that the absolute values could be low by 50% due to differences in sensor voltage between calibration and flight.

As mentioned in the reply to referee #1 we have inserted a section in the discussion dealing in detail with uncertainties and sources of error. We conclude with the recommendation to use sensors with all-golden electrodes in further campaigns to enhance the reliability of the method.

2. The differences in the results from the individual sensors of one side are especially apparent on the fore deck, where the single profiles deviate also qualitatively. Again we identified the voltage control as a source of this behavior. We included a sub-plot additionally showing the course of U_S on the downleg illustrating this situation.

During upleg, on the other hand, a second effect dominates: Gases that have been adsorbed on the electrodes under atmospheric conditions desorb after the

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

pressure drop following nosecone separation. Together with the voltage control this leaves the sensors in an initial unstable phase with results differing both qualitatively and in absolute numbers. These explanations are now all included in the discussion of the manuscript. As already stated in the reply to point 1 from referee #1 we recommend to house the sensors in an evacuated hood that is removed together with the nosecone, which would shorten this initial desorption phase.

The maximum difference in the results between the aft sensors alone, which also track well qualitatively, is below 15%, which leads to the conclusion that accurate measurements with the described technique are possible if the discussed sources of error can be ruled out in future experiments.

3. As given in the reply to the first referee we agree that the PHLUX data should be discussed separately in a future publication when interpretation of the results on the basis of a detailed analysis is possible. We would however like to keep the technical description of the instrument and of the measurement principle in this paper in order to give a complete overview of our contributions to the project.
4. We understand that it is desirable to interpret the results in terms of atmospheric-physical processes. This is, however, beyond the expertise of our institute. The interpretation of the profiles of atomic oxygen densities is greatly facilitated if the measurements from the other instruments, e.g. the electron densities, and the accompanying radar/lidar data are taken into account. We have detailed the description of all the instruments on the payload and have inserted appropriate references. The comparison to the O results deduced from the O₂ dayglow photometer measurements would also be of great interest in that respect. A future publication, probably led by the Institute of Atmospheric Physics, may link the findings on the various quantities.

As the effect of mesospheric ice particles onto the very thin coatings of the PHLUX sensors was unclear conditions with few noctilucent clouds (NLCs) were favored for the launch. The actual distribution of ice layers can probably be re-

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

- trieved from the NLC photometers measuring at 220 nm.
5. The background about techniques used in past campaigns for measuring atomic oxygen aboard sounding rockets has been extended in the introduction of the manuscript.
- Page 3250, line 22: The question if impurities on the gold surfaces of the electrodes are an issue for the measurements is answered by testing the O sensors additionally in pure O₂ atmosphere. We have added a “calibration curve” for the sensor C15 (gold plated cathode) in the manuscript that confirms its inertness towards molecular oxygen.
 - Page 3255, line 10: see above.
 - Page 3257, line 14-15: Yes, the results were divided by this factor. Factors at intermediate altitudes were interpolated linearly between those obtained directly from the simulations. This is now explained in the text.
 - Page 3259, line 16: Downleg data is now included in this figure.
 - Section 6.2: An approximate equation describing the total signal of a sensor with platinum electrodes as a sum of the contributions from O and O₂ is given in the dissertation of R. Foerstner (listed in the literature). However, in order to separate the measured currents into their components a detailed knowledge of electrode properties such as their surface area, total number of adsorption sites or adsorption probabilities of the species is required. We have added this argument in the explanations.
 - Figure 6: It would be desirable to have sensor with all identical calibration curves. The different slopes, however, are due to tolerances in the production via screen printing. This means deviations in the heater and electrode geometries and their

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

[Interactive
Comment](#)

relative position, which in turn causes small variations in the temperature of the electrolyte and the electrodes. Slightly different spacings between the electrodes produces differences in the conductance across the electrolyte. Oxygen densities were calculated by applying the linear relations from the calibration curves to the measured sensor signals. We have detailed that in the explanation of the calibration results.

- Figure 14: Although the plot easily gets busy we would like to show the direct comparison between the results from fore deck-upleg and aft deck-downleg, as they are all obtained under ram conditions and this also include the horizontal variability of the profiles. However, we have removed the PHLUX results which helps to tidy up the figure.

Please also note the supplement to this comment:

<http://www.atmos-meas-tech-discuss.net/8/C1676/2015/amtd-8-C1676-2015-supplement.pdf>

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 3245, 2015.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)