

Interactive comment on “Evaluation of the flux gradient technique for measurement of ozone surface fluxes over snowpack at Summit, Greenland” by F. Bocquet et al.

Anonymous Referee #2

Received and published: 4 May 2011

General comments:

This manuscript describes a method for measuring ozone deposition and emission rates from the remote polar snowpack. With some modifications this will be ready for publication in AMT. This especially relevant because ozone fluxes over the polar snowpack may be an important ozone loss mechanism in the Arctic. This is a good addition to the literature and describes some of the most detailed ozone flux measurements made to date.

Overall, with edits I'm very supportive of this manuscript being accepted for publication. I'm looking forward to seeing the final version.

C505

Specific comments:

Is the air really homogeneous in all directions? Is there an impact larger than the measured ozone gradients due to high NO_x in camp influenced air? Can the camp influenced air be removed from the data by filtering on the wind direction?

The authors should clarify why they have decided to do the flux calculations in these units (equations 1 & 2). It is my understanding that in most models the diffusion of gases is treated in terms of mixing ratio to ensure that there are not errors in the calculated diffusion rates. This is because differences in the air density (at a given temperature and pressure) result in different concentrations, but not different mixing ratios. In a situation without deposition towards the surface there still may be a vertical gradient in the concentration due to differences in the air density. Shouldn't calculations of the vertical flux take this into account?

The authors should comment on the conditions at Summit and if they are close to near-stable and near-unstable to understand if equation 3 applies.

More detail on the ozone losses discussed on P1030 should be included. Was this measured only in the lab, but not in the field? Was this a systematic, reproducible error that has been corrected for based on the tube length?

In Section 5, there is also an uncertainty in using these equations at all. How does this compare with the error from the detailed Monte Carlo simulations?

The authors should comment further on Figure 9 and discuss more the features and their possible meaning in the text.

Technical corrections:

Abstract: The authors should add the month and year of the measurements to the abstract. It is also a bit confusing to talk about the seasonal dependence, when the measurement period is only four months. It would be clearer to simply say summer vs. spring.

C506

General: I recommend using the same units for deposition velocity as well as surface resistance throughout the paper: either cm or m as the distance unit.

P1023 L5: The sentence that starts here is out of place, this paragraph should be reworded to make it more clear that ozone behavior and chemical interactions in the snow pack are coupled. It should also be noted that by measuring very accurately the ozone deposition, you are capturing the net effect of some of the influence of the snow on boundary layer air.

P1024 L1: A summary sentence describing why you discuss these measurements in such detail is needed.

P1024 L19-23: This should be reworded to be clearer. I suggest adding some additional references to these statements.

P1024 L28: Add numbers, what is the total signal and what is a typical gradient?

P1026 L21: at Summit, compared to other Arctic sites.

P1026 L22-25: The authors should consider adding other references, there is an ongoing debate as to what causes high ozone at Summit as well as what differentiates the ozone seasonal cycle at Summit from other Arctic sites. Some other papers in the literature should be referenced.

P1029 L9: Add a citation to "most of the literature... demonstrated".

Figure3: Is this representative of the other inter-comparison graphs?

Section 5: More detail on the range of values tested should be included. What temperature range, what wind speed gradient range, what range of sensor accuracy, etc?

Figure 5: In the current form this figure is confusing, is this during an inter-comparison experiment? The figure caption should be more detailed and clearly state the height of the inlets. If the change in temperature is associated with error, then this should also be stated in the figure caption. Given the sudden onset of the temperature disagreement,

C507

it would be interesting to include the specific algorithm for correction and if possible the sensitivity of calculated fluxes to using it.

General: The figure captions all need to be updated to be more clear and contain more information. Because DOY is not easily translated to month and day, the authors should at least include the date range in the figure caption. All of the figures with gradient and/or flux data should include a definition of positive vs. negative so it is clear if there is deposition or emission. Extra information not referred to in the text (for example the text on Figure 7) should be removed from the figure.

Figure 8: This figure should have two panels, one for the 10-2 m data and the other for the 2-0.75 m data.

General: I assume during this time the inlets were not always at the stated heights, can the authors include a table of day of year vs inlet height in the electronic supplement. Is there any difference in the behavior when the 0.75 m inlet was very close to the surface?

General: There are some small typos and wording changes needed. The manuscript should be read carefully for typos, wording, and clarity before resubmission.

Interactive comment on Atmos. Meas. Tech. Discuss., 4, 1021, 2011.

C508