

Review to 'Transport analysis of ozone enhancement in Southern Ontario during BAQS-Met' by H.He et al.

In their study He et al present a detailed case study of ozone enhancement in southern Ontario. They provide ample evidence for the stratospheric nature of the ozone increase, based upon surface measurements, vertical soundings, satellite data and models simulations with FLEXPART. There is little doubt that their interpretation of the event is correct. Nevertheless several major aspects need to be improved in the manuscript before I can recommend it for publication in ACP. These aspects are listed below and I encourage the authors to include them in a revised version.

Major points:

- 1) What is the really new aspect of this study? As mentioned before, all interpretations look correct and are well supported by observations and modelling results. But to my knowledge there exist many studies showing stratospheric ozone intrusions (SI) in the mid/lower troposphere and surface sites. Hence, the authors should make more clear which aspects are really new and how they relate to the existing literature. If there are only few new aspects, I recommend to substantially shorten the manuscript, both in text and number of figures.
- 2) The structure and presentation in section 4 could be considerably be improved. There are essentially four aspects which I think must be done:
 - At the moment the discussion follows a 'simple' one-figure/one-paragraph style, i.e. the whole section steps from the discussion of one figure to the next one. This is ok in the beginning, but finally leaves the reader with the impression that no coherent story is told. It looks as a simple listing of 'another supporting proof for SI'. I would very much appreciate if the authors could 'streamline' there text in such a way that this impression does not come along.
 - The whole section is very long and definitely needs some subsections. Indeed, the section is entitled 'Observations', but modelling results are also presented. I would suggest to structure the whole section 4 at least into the following subsections: 1) Ozone sounding; 2) satellite measurements; 3) FLEXPART modelling.
 - Most of section 4 is a presentation of results, but part of it looks more like a discussion to me. For instance, on page 15572 (L8-21) the modelling results are set into context and interpreted, which clearly separates this paragraph from the preceding ones. Please consider to more clearly separate discussion and results in the manuscript.
 - With the last paragraph in section 4 (p15572,L22 – p15573,L21) a 'completely' new topic is started, i.e. the chemical transport model AURAMS is introduced. I am not convinced that the extra benefit for the manuscript justifies the introduction of AURAMS simulations. In fact, at first reading I got the impression that it essentially shows a lack of focus of the whole study. Please, consider whether these AURAMS are really essential for the study and how my impression can be 'falsified'.
3. In many plots the radar-derived tropopause is shown (e.g. Fig.1, Fig.5). It is in good agreement with the thermal tropopause. But if you consider the crossing of ozone-rich air through the

tropopause, the dynamical tropopause, defined as an iso-surface of potential vorticity (PV), might be more appropriate. Indeed, when looking at the FLEXPART simulation the dynamical tropopause is used and a 'simple' PV/ozone relationship is used to initialize the FLEXPART simulations. At least I think that all plots should also show the height of the dynamical tropopause in addition to the radar-derived one. In particular, if in Fig.6 the high ozone values reach down to 7 km (on 29/06), but the thermal tropopause does not, it is still unclear whether the ozone intrusion is reversible or not. Note also that at some times, the ozone really shows such a deep intrusion, but the thermal tropopause is rather undisturbed. I guess that the dynamical tropopause shows much more fluctuations in line with the ozone signals.

Minor points:

1. P15562, L26,27: Give reference for 'other processes' related to STE.
2. P15564, L5: The reference for FLEXPART must go to Stohl alone; all the other references are applications for FLEXPART. Please separate the two.
3. P15568, L 23-24: 'A rapid ascent in tropopause height ... suggests the possibility of an ozone intrusion'. Why is the tropopause ascent related to an intrusion? Please explain in greater detail.
4. Fig.2: What is meant by 'radar tropopause vertical velocity'? Is it the velocity of the tropopause or the vertical velocity at the tropopause? And anyway, to my knowledge only the relative velocity of the two is of physical importance for STE and ozone crossing of the tropopause. Please explain!
5. Fig.7: These are nice 3D illustrations of the stratospheric intrusions. But possibly they can be shown much earlier together with some discussion of the meteorology of the events. Indeed, at the moment the whole discussion is strongly focused to the Harrow site, but nearly nothing is said about the weather situation related to the event. I would appreciate to see a short introduction to the meteorology of the event, possibly including Fig. 7.
6. If you compare Fig.5 and 6 you see that in the latter the high-ozone stratosphere is shifted to higher altitudes. Is this only related to the fact that a stratospheric tracer is used in FLEXPART whereas the 'full' ozone is included in the satellite (kriging) result? Please shortly discuss the vertical shift between the two.