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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Secondary maxima in ozone profiles" *by* R. Lemoine

Anonymous Referee #1

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1. Summary

Quite often mid-latitude ozone profiles show large secondary ozone maxima below the main ozone concentration maximum at around 22 km altitude. The author reports some examples for such secondary maxima and then proceeds to climatological features and long-term variability aspects of secondary maxima. His investigation is based on ozone-sondes flown regularly since 1969 at Uccle, Belgium, and on global SAGE II satellite data.

Some information on mechanisms leading to the formation of secondary maxima is presented. The paper cites older work, but does not provide very detailed examples or major new insights. To me the regression of total ozone variations by parameters describing secondary ozone maxima in last part of the paper is highly questionable. However, the reported trend in secondary maxima is very interesting by itself.

My problems with the regression relating total ozone and aspects of secondary maxima are the following: Total ozone variability is almost entirely driven by lower stratospheric variability. Secondary maxima occur in the lower stratosphere and are an intrinsic aspect of total ozone variations. By regressing total ozone with characteristics of secondary maxima very little new information is brought in. This is similar to regressing total ozone at Uccle with total ozone from another European station, say Arosa: A very large fraction of Uccle total ozone variations could be explained, but without new insight.

I feel that the paper by Lemoine requires major revisions in two aspects, before it can be moved on from ACPD to publication in ACP.

1.) The discussion of the examples for secondary maxima should be expanded substantially. A more detailed discussion and appropriate PV-maps are needed.

2.) The regression of total ozone variations by aspects of secondary maxima should be omitted, or, better, should be replaced by a regression of secondary maxima characteristics by parameters that are related to the global circulation or to Rossby wave breaking.

2. General comments

1 Introduction: The introduction part seems very long (4 out of 15 online pdf-pages). As correctly indicated, substantial work has been done on ozone laminae. Is the author aware of the interesting work started by *Mlch and Lastovicka* (1996)? While keeping most of the referenced concepts, the author should go less into the details and try to shorten the introductory description.

4 Examples/ Fig. 1: Is there a substantial difference between the two examples? If not, one example is sufficient.

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Fig. 1/ Fig. 2: It would be much better to use the same date for ozone profile and back-trajectories, instead of showing one aspect of one case and another aspect of another case. I think the author should say that the back-trajectories end on 20010330, not that they start on 20010330. This should be corrected.

5 Secondary maxima and Rossby wave breaking: This is a key-section of the paper and a much more in-depth investigation is required here. The author should show PV-maps for illustration. An important aspect is that small scale features like laminae or secondary maxima may not be resolved in the coarse ECWMF fields. Higher resolution RDF trajectory models may be necessary, like the French MIMOSA model, or the work done by *Manney et al.* (1998). Trajectories are problematic because they give only partial information about air-mass origin. While air may have been over the arctic 5 days ago, it still might be a tropical air-mass, see e.g. *Teitelbaum et al.* (2003). These issues should be addressed.

While Rossby wave breaking and erosion/abrasion of the vortex are important, secondary maxima may also be created by the injection of tropospheric air into the lower stratosphere, the *Dobson* (1973) hypothesis. This can happen, e.g. through warm conveyor belts over the Atlantic and subsequent isentropic air exchange through the jet-stream. The author should not simply dismiss this possibility, but rather look for some examples, especially since otherwise little new and original work is presented in section 5.

8 Trends in atmospheric circulation and ozone variation As mentioned, I find the regression of total ozone variations with characteristics of the secondary maxima very problematic. Essentially the author is regressing two aspects of the same thing, since total ozone has to be high when there is a secondary maximum. No new and independent information is added in this regression. A regression of Uccle total ozone with e.g. Arosa total ozone might give $R^2 = 0.95$, so one could state that Uccle total ozone is largely determined by ozone transported from Switzerland. Clearly this would a mistake based on the fact that no new information was added.

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Interactive Comment

I strongly suggest to completely omit this regression and its discussion. The author assumes that secondary maxima are related to planetary wave breaking and ozone levels in the polar vortex, and that they can even be used as a measure for these "quantities". However, he does not really show this in his paper. It would be much better to try explain such a physical connection in more detail, and then to regress aspects of the secondary maxima with quantities like Eliassen-Palm flux or typical arctic-vortex ozone levels.

3. Specific comments

Abstract: As argued above, I dont think that the author has really proved that "Occurence and amplitude of ozone secondary maxima ... can be used as a measure of the influence of atmospheric circulation on the ozone distribution at mid- latitudes". The two are likely related, but it would be the job of the author to elucidate that. As the paper stands, I think at least part of this last sentence of the abstract must be deleted.

Page 1793 (online-pdf), line 4: should be "Varotsos"

Fig. 1 : The ozone concentration in the secondary maximum is much higher than the typical SAGE measured concentration at $60^\circ - 70^\circ$ N. So horizontal advection of polar vortex air alone cannot explain the secondary maximum, you also need downward motion. Where does that happen? Where is it in Fig. 2? Add a paragraph.

Page 1799 (online-pdf), lines 11 to 14: Sentence is not complete, or I don't understand it.

Page 1800 (online-pdf), line 6: March to April is also the time of the year when the tropopause is lowest. This must increase the probability for secondary maxima, simply because there is a larger altitude range where they can occur. Similarly with the high tropopause from August to November, there is less space where secondary maxima

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can occur. The author might want to mention that.

7 Zonal distribution: The author should compare his results in much more detail with the study by *Appenzeller et al.* (1997). How does his annual cycle and distribution compare to Appenzellers more theoretical results? Several paragraphs should be added.

Less frequent blocking anticyclones in the southern hemisphere very likely has something to do with the less frequent secondary maxima there.

9 Conclusions: page 1805, lines 9-11: I think "often" should be used instead of "systematically", because the author mentions only about 20 cases.

page 1805, lines 21-22: Again, I would say that "for the 20 cases studied, troposphere to stratosphere exchange as suggested by Dobson (1973) was not found and did not create the secondary maximum." I don't think it can be entirely discounted on the basis of 20 examples only.

page 1805, line 23 to page 1806, line 5: This entire paragraph should be omitted.

4. References

Appenzeller, C., and Holton, J.R., Tracer lamination in the stratosphere: A global climatology, *J. Geophys. Res.,* 102, 13,555–13,569, 1997.

Dobson, G.M.B., The laminated structure of the ozone in the atmosphere, *Q.J.R. Meteorol. Soc.*, 99, 599–607, 1973.

Mlch, P., and Lastovicka, J., Analysis of laminated structure in ozone vertical profiles in central Europe. *Ann. Geophys.,* 14, 744–752, 1996

Teitelbaum, H., Basdevant, C., Hertzog, A., and Semane, N., Comment on "A Lagrangian analysis of stratospheric ozone variability and long-term trends above Pay-

erne (Switzerland) during 1970 to 2001" by G. Koch et al., *J. Geophys. Res.*, 108(D21), ACL 7-1, doi:10.1029/2003JD003687, 2003.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 1791, 2004.