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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A model study of the January 2006 low total ozone episode over Western Europe and comparison with ozone sonde data" *by* A. Mangold et al.

Anonymous Referee #2

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This paper investigates the origin of ozone reduction during a marked low-ozone episode over Western Europe in January 2006. Although the occurrence as well as predominantly dynamical origin of such events are well-known, the article uses several transport and climate models, with or without stratrospheric chemistry, to investigate in detail the contributions of the various factors, such as advection from low latitudes or the presence of vortex air aloft. The amount of chemical ozone depletion due to halogen chemistry occurring in the cold uplifted layers outside of the polar vortex is quantified. The latter point in particular brings novelty to the paper.

The paper is well written, although it could be more concise. I find the paper well

worthy of publication, provided that the authors consider the minor comments below. I would also encourage the authors to shorten the manuscript, as some sections (e.g Discussion section 7, or model modules on page 8) are quite lengthy.

1) A few column ozone maps derived from satellite observations would be useful in describing the events.

2) p6, line 156. It is unclear whether the ECMWF analyses had a spatial resolution of 1 degree, or of 0.5 degree as suggested on line 159.

3) p8, line 218. It is unclear what the two standard deviations are. One measure the inter-annual variability, but what is the overall variability ?

4) p9, line 264. The authors do not mention PSCs observations from satellite. Are there some ENVISAT observations that could be of interest for confirming the occurrences of PSC in these regions.

5) p10, line 285. As the PV=2 contour delineates stratospheric air masses, it would be very useful to have it outlined in Figure 5.

6) p13, line 388. It is unclear why the active chlorine is shown only for KASIMA, but not for E5/M1. It would have been interesting to see the difference in chlorine activation in both models, rather than showing the chlorine reservoirs for one of the model.

7) p17, line 525. This whole paragraph is very unclear. The authors mention “Change” but it is unclear what “Change” means. Change with respect to what ? This paragraph would gain to be rewritten.

8) That the model reproduces well the formation and evolution of the event may be an overstatement. In Figure 11, the vertical ozone profiles from the models, except for CLAMS, do seem very different from the observations. Isn’t it more fair to say that the models reproduce analogous events but exactly not collocated in time and space with the soundings?

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Interactive Comment