

## ***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 #1**

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The authors analyzed in a case study a low total ozone event above several European mid-latitude stations in January 2006 by means of measurements (Dobson and Brewer spectrometers and ozone soundings) and three different models. The aim was to separate three different contributions to the low ozone event, i.e. (1) the displacement of the polar vortex over the stations, (2) the combined effect of horizontal advection of ozone-poor low-latitude air in the UTLS region with the divergence of air out of the column caused by an uplift of isentropes, (3) instantaneous, in-situ chemical ozone depletion triggered by PSCs which appeared due to the local uplift of isentropes.

There is already some literature available about low total ozone events, some dealing

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with process studies like the present one others with climatology's. In that literature the influence of polar vortex displacement is seldom acknowledged. The present paper deals with a significant involvement of the polar vortex displacement to the event. This is the only issue which gives relevance for a publication in ACP. However, several parts, which are listed below, should be improved before a publication in ACP.

Specific comments:

1. The separation of contributions (1) and (2) is limited as pointed out by the authors (p6023 l20/21). Furthermore, there is no separation of the effects within (2), ie. the horizontal advection of ozone-poor low-latitude air and the uplift of isentropes. Such a separation is done eg. in Koch et al. (2005). It would be a second relevant output of the paper to distinguish both effects, since Koch et al. (2005) found in a climatology that vertical displacement of isentropes is less important than horizontal advection, whereas James et al. (2000) found the opposite. Such a separation will also help to separate the contributions (1) and (2) better. I strongly recommend to perform this additional effort. In this context two additional remarks. At p6029 l19 the authors cite Hood et al. (2001): "In the case of extreme minima, contributions from vertical transport processes contributed between 20 and 80 DU, ...". I don't find that statement in Hood et al. (2001) in particular not the value 20 DU. After referring to the effect of horizontal transport processes the authors continue to claim: "This is in good agreement with our findings that these two mechanisms are often of about the same magnitude, but varying from one location to the other, one mechanism can dominate." Hood et al. (2001) refer mainly to lower stratospheric transport effects not to vortex displacements. However, the authors don't separate these two effects, horizontal (lower stratosphere) and vertical transport processes, in their study. How can they claim that Hood et al. (2001) findings are in good agreements with their own ones?

2. The authors have been put much effort in showing that instantaneous, in-situ chemical ozone depletion is negligible. In fact a simple estimation would do the same job. On page 6025 the authors give already an estimate of 5 DU ozone loss within days un-

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der extreme conditions. That would be Antarctic conditions during August/September. Since the winter was a rather warm winter and we have a mid January event the result of a much smaller instantaneous loss is not surprising.

In fact, the methodology used raises some questions. Why have the output of two different models been used to show and explain the instantaneous losses (Figs. 12 & 13)? E5/M1 shows that practically all Cly, which is usually estimated to be in the order of 3.2 - 3.7 ppb, had been activated. However, KASIMA shows that only less than 1 ppb Cly had been activated. Where is the rest? That doesn't fit together. Obviously, the chemistry part of the models is not good enough to provide any estimate of the ozone loss. It adds to my opinion that many phrases in the manuscript emphasizing that the models agree "very well" with the measurements at least with respect to chemistry, eg. when looking at Fig. 11, are too optimistic and should be avoided.

The reason the authors deal with instantaneous losses is due to the fact that in this case the vertical uplift triggered PSC formation followed by chlorine activation. Therefore, they are interested in the ozone loss within 2 days due to this additional chlorine activation. If this is really interesting enough, one should at least mention that chlorine could had been activated before. Fig. 12 and the general meteorological situation of the whole winter supports that chlorine had been activated. In particular the sentence at p6019 l28: "If chemical ozone destruction by active chlorine had indeed happened during the low ozone episode, this would be indicated by a distinct reduction of the reservoir gases and a distinct increase of ClOx." is not correct. Ozone loss happens when ClOx is available and is not dependent on increasing levels.

3. The authors mention several times the ozone depleted vortex and provide some references. On the other side they also note that the winter was one of the mildest on record and cite WMO (2006) stating overall column loss in the order of 13%. Although the statement is correct that in the vortex ozone had been depleted, the statement implicitly suggest that this is the main reason for low ozone within the vortex and/or the low total ozone event which is not the case. This should be emphasized in the text at

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least once.

Technical corrections:

p6009 I12: I suggest to write: "The vertical resolution is in the order of 100 m." unless a good reference is given. The vertical resolution is usually dependent on the operation procedures and may even vary between BM and ECC sondes.

p6009 I26: Why haven't data before the Pinatubo eruption been used?

p6012 I4: photolysis

p6025 I3: Article Harris et al. (2002) does not exist.

Fig. 1: Senseless legend to "Brewer (last year)" given.

Fig. 2 & 3: Both blue colors are hard to distinguish.

Fig. 3: Why is the Uccle mean given as a reference and not a Payerne mean? A Payerne mean would make more sense.

Figs.: In general all writings in the figures should be easily readable (big enough) in a printed version. Currently, e.g. the numbers on the axis in Fig. 15 are hard to read and a minus sign in Fig. 12 does not show up in my print version at least.

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