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Interactive comment on "A transboundary transport episode of nitrogen dioxide as observed from GOME and its impact in the Alpine region" *by* D. Schaub et al.

D. Schaub et al.

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I would like to thank the anonymous referee #1 for his helpful reviews from 23 October 2004 which improved clarity and information content of the paper.

Below are the responses to the specific comments.

1) Concerning the structure of the paper, the referee pointed out that parts of the methodology are not independent of and in the text not properly separated from the example described afterwards. We improved the structure by

- Defining the (meteorological) situation for which the methodology is applicable (cloud

cover, large NO2 concentrations within/above clouds, lightning excluded, trajectories with ground contact, thus frontal transport can be assumed),

- carefully rewriting the methodology in order to be understandable independently of the episode.

2) The referee suggested elaboration of the NO2 lifetime discussion, especially with respect to daytime and night time chemistry. We expanded the discussion in the introduction as suggested. We included and discuss additional tests concerning the stability of our results assuming only half of the lifetime (new Fig. 11a and text in section 4.6.2).

3) The referee addresses the NOx partitioning and the fact that, during winter, the partitioning favouring NO2 is not as clear as in summer. We agree with this in general and include this in the introduction and in section 3.2.1 in more detail. For our method, however, we expect this to be circumstantial because the conversion from the proxy NO2 transported along the trajectories to NO2 is done via inclusion of the 'real' GOME NO2 column data.

4) The referee argues rightly that typically, in winter, pollution is often kept below 100-300 m and that, therefore, assuming pollution take up below 50 hPa (corresponding to about 500 m) might be not realistic. The reason, however, for assuming 50 hPa was twofold:

- Pollution transport connected to frontal movement is an event where a less well defined boundary layer height is expected.

- It is known that air mass trajectories near the ground are less accurate than at higher levels. In order to include all potentially polluted air masses, we decided to define the near ground layer generously. It must also be kept in mind that with our approach, trajectories are started from the GOME columns in discrete 25 hPa steps. Therefore, too much information might be lost when defining this height to low. These arguments are pointed out in the text and additional sensitivity tests on this issue are added to the

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paper (showing 50 hPa is a sensible choice and the method continues performing well for lower PBLs). We elaborated section 4.6.2 and include newly Fig. 11 and 12.

5) The referee correctly concludes the CTH is an important element in the suggested method and asks for verification of the assumptions concerning (constant) cloud top height. Originally, our assumption of a constant CTH was made compelled to the lack of cloud top height data in the satellite data and supported by meteorological considerations. More recent GOME data from KNMI showed that all the CTH of the individual pixels lie in a range of within 8% around the mean CTH which is sufficiently 'constant' for our case. We mentioned this additional information in the paper and also clearer pointed out this assumption to be an error source and the future need to include pixel-specific CTH for this method (sections 3.2.4, 4.6.1 and summary).

6) The referee is right when pointing out the importance of the altitude of the ECMWF model at the locations of the stations (and the difference to the real height). However, from our experience with alpine stations, the main issue is to choose the appropriate height above model ground. We have elaborated the text (section 4.6.2) and have given the relevant heights (pressures).

7) The referee mentioned some points (PBL height, constant CTHeff, night time chemistry, NOx emissions outside population centres) missing in the error discussion of the method. We included all issues into the error discussions.

8) For the referee's comment on page 5113 of the paper (cloud top height according to live cams and assumption of NO2 layer) see point 5. We also rephrased the passage. Concerning the NO2 layer we refer to preliminary trajectory calculations.

9) The referee points out that pollution uptake is unrealistic when the ground distance of trajectories exceed 50 hPa. We agree. However, the trajectory time steps below 150 hPa are only shown in the figures for better illustration and not used for modelling pollution uptake. We clarified the text.

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10) The referee suggests to better discuss the correlation (and the potential offset) between GOME NO2 and modelled NO2 for the different CTH and to show examples of this correlation. We followed the suggestion (scatter plot GOME - modelled in Figure 9 where it is also shown that there is no offset) and elaborated the discussion in section 4.6.1.

Minor comments: According the referee, we

- omitted as much as possible the citation of grey literature (some grey literature is kept because this is the only literature on the appropriate issue, e.g. for measurement networks),

- changed 'chemiluminescence' to 'ozone chemiluminescence' and

- expanded the list of substances contributing to the interference of the NO2 measurement (including particulate nitrate).

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 5103, 2004.