Atmos. Chem. Phys. Discuss., 10, C3846–C3851, 2010
www.atmos-chem-phys-discuss.net/10/C3846/2010/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Tropospheric ozone variations at the Nepal climate observatory – pyramid (Himalayas, 5079 m a.s.l.) and influence of stratospheric intrusion events" *by* P. Cristofanelli et al.

P. Cristofanelli et al.

P.Cristofanelli@isac.cnr.it

Received and published: 11 June 2010

Replied to anonymous Referee 3

The paper presents two year measurements of surface ozone at a high elevated Observatory (NCO-P, 5km) at S. Himalaya and an attempt to qualitatively describe the role of Stratosphere-to-Troposphere Exchange and quantify its contribution on ozone background. The location of the measurements site is unique and by only this the acquired data set is of great significance. There seem to be two more papers referenced in this

manuscript (Cristofanelli et al., 2009; 2010-companion) that deal with the entire or part of the same data set and again with the role of stratospheric intrusions. Towards acceptance of this paper there should be clear explanation on the difference between the three papers and thus the need for publishing this work separately. Overall, I suggest that this work merits publication in ACP after some issues are taken into account and re accordingly corrected.

We gratefully thank the referee3 for his valuable suggestions. In this reply we will discuss all the point indicated by the referee, trying to improve the manuscript and the related analyses following the referee suggestions. In italic characters are reported modifications now introduced along the paper.

The early paper by Cristofanelli et al. (2009) was based on only one year of data, with the (specific) explorative aim to set-up a reliable methodology to identify SI at the NCO-P. We think that in respect to the first paper, this work greatly improve both the climatological analysis of SI in South Himalayas as well as the assessment of tropospheric ozone linked to direct SI occurring at the measurement site. Moreover, this paper presents the first detailed discussion about tropospheric ozone variations (seasonal and diurnal) at the NCO-P. The motivation of the present paper was now clarified in the "Introduction": *"... this work presents the first complete description of surface O3 behaviours at the WMO - GAW station "Nepal Climate Observatory – Pyramid" (NCO-P, 5079 m a.s.l., Nepal), together with a systematic evaluation of the role of deep stratospheric intrusions in determining the observed O3 levels."*

The further paper (Cristofanelli et al., in preparation), will describe and categorise the principal synoptic scale scenarios connected with the direct SI detected at the measurement site during representative case studies. Thus, we think that the "2010" papers are each other complementary and that they can be published separately.

ACPD
10, C3846–C3851, 2010

1) The abstract is much too long. Parts of it easily fit to the introduction and the methodology sections. I would suggest removing those parts and stick to major results from this work. The role of ozone as GHG and its radiative forcing is not covered in this work, so there is no place for this in the abstract rather than a short discussion in the introduction.

OK, the abstract was modified.

2) In section 3, there is an interesting discussion on the diurnal patterns and their amplitude during the different seasons. I would like to stress out that in most cases, unless such constant conditions are met at the particular site, the amplitude of the mean diurnal cycle is much smaller than the amplitude one can find during each individual day. This is because the average cycle actually smoothes away the particular characteristics of each day cycle. The correct way to refer to mean amplitudes is to calculate the amplitude for each day and then extract the average and the respective statistics. The amplitudes provided cannot entirely correspond to the processes behind in case of future comparison with any modeling studies.

Yes, we agree with the referee. In the new paper we present also a description of the lowest and highest inner quintiles, to stress the importance of the day-to-day variability of diurnal cycle. To this aim we re-drawn the Figure 4. However, the authors think that the general description of the diurnal cycle shapes as well as the identification of systematic minima and maxima at NCO-P were robust enough to give new hints about the diurnal variability of ozone at high Himalayas. This has been supported by the analysis (now provided in the manuscript) of the occurrences of ozone diurnal maxima in function of day-time.

3) Section 4.1: The way the criteria are presented is not very clear and somewhat gives the impression that since the first set of criteria resulted to a limited number of cases, then an additional set was put into force to increase this number. Moreover, the second set of criteria includes parameters (e.g. AP, PV, TOC) already included in the first set.

ACPD

10, C3846–C3851, 2010

Do the second set criteria (i-iv) have to apply simultaneously, or they are independed from each other? A RH of 60% is too high, even though it can be found during specific SI cases, it cannot be considered as typical criterion. Overall, this section needs to be reorganized so that one integrated and well justified set of criteria is given. Additional discussion on the sensitivity of the results on each criterion and the robustness of the results would certainly help.

Actually, as also indicated by the referee2 and 3, this section was not so clear. In the revised paper we re-organized this section also performing a sensitivity study to the detection algorithm. We also better explain the adoption of the 60% RH threshold. As now reported in the paper (Section 4.1): "To this aim we adopted an RH threshold of 60% to detect SI. Besides as being already applied at mountain stations to identify stratospheric air (e.g. Trickl et al., 2010), this value represents the typical "upper boundary" of RH at NCO-P able to contribute to the identification of SI events.".

Moreover, a discussion on criteria sensitivity has been introduced in the paper (section 4.1): *"It should be noted that the last criterion was the most active in selecting possible SI. As being based on the analysis of not completely unambiguous stratospheric tracers (i.e. RH and O3), we introduced additional thresholds for other tracers (i.e. AP, PV, TCO) to minimize this ambiguity in the SI identifications. Moreover, we performed a sensitivity analysis by adopting different RH values. For instance, assuming the widely used 40% threshold for RH (e.g. Stohl et al., 2000; Trickl et al., 2010) the total number of selected SI reduced only by 2%. On the other side, the (iv) criterion should lead to the selection of further 15 SI days if the additional thresholds on AP, PV and TCO were not considered."*

4) Section 4.2: A two years period is too short to deduce on interannual variability or possible trends. Please change accordingly.

OK, we rephrased: "...the seasonal SI frequencies y during the 2-year investiga-

ACPD

10, C3846–C3851, 2010

tion was were rather constant recorded at NCO-P (Figure 5).”

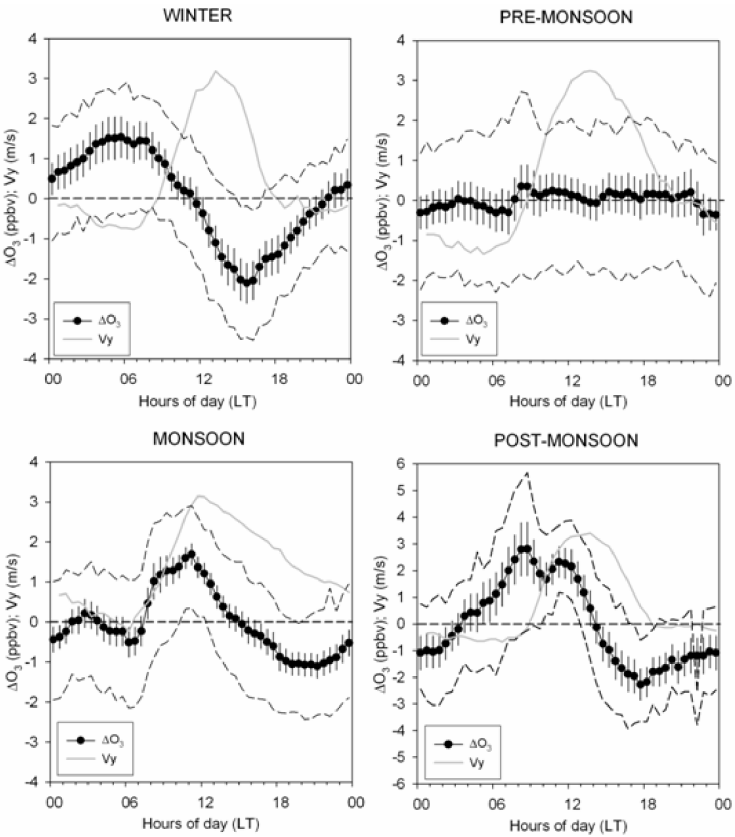
Technical comments: 1) Please use either TOC or TCO for total ozone column, but not a mixture of both.

OK

2) In Fig. 3 you may correct Ago to Aug in the x-axis

OK

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 1483, 2010.



Diurnal cycle of normalized O_3 values (ΔO_3) and meridional wind component (V_y) for the different seasons at the NCO-P. The vertical bars denote the 95% confidence level while the upper and lower dotted lines denote the 25th and 75th percentiles for ΔO_3.

Fig. 1.

ACPD
10, C3846–C3851, 2010