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Comment

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.

Anonymous Referee #1

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Review of “Tropospheric ozone variations at the Nepal climate observatory...” by Cristofanelli et al .

This paper presents an analysis of ozone and related data from the NCO-P observatory at 5 km in the Himalayas. It is significantly improved over an earlier version that I also reviewed. The analysis focuses on the influence of stratospheric intrusions and the fraction of tropospheric ozone due to S_T exchange. While the analysis appears to be reasonable and the data are extremely valuable, there are some key points that are not clear and several important caveats that the authors fail to discuss. I believe that once

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these points are clarified, the manuscript could be ready for publication.

Important caveats: The method to derive the total stratospheric influence relies on the identification of specific time periods with strat influence. This method fails to account for stratospheric contribution which is part of the background. Certainly the high value of average O3 during the pre-monsoon period is partly due to mixing of stratospheric air into the background. This would result in an under-estimate of the strat contribution to trop air. The other important caveat is the specific method to identify time periods with stratospheric influence. The method likely over-estimates the amount of time with strat influence (although it is a bit difficult to be sure given some confusing points in the description.) These two caveats MIGHT balance out, but not likely. While I don't think they completely invalidate the analyses, the authors need to discuss these uncertainties.

As for the confusing part, I found the values in Table 2 impossible to follow. First, is N the number of 30 minute data points? If so, then why are there only 1600 hrs (N=3183) in all seasons? Second, I can't reproduce the values for ppbv*hr. For example under pre-monsoon, for N=1396, assuming 25% time is strat influence, gives approx N=400 or 200 hrs. $200\text{hrs} \times 9\text{ ppbv} = 1800\text{ ppbv*hr}$, not $4.5\text{e}4$. The authors need to do a better job of explaining these values and how the calculations were actually done. Assuming this is a simple misunderstanding, then I would say the manuscript could be cleaned up and accepted.

Other points: 1484, Line 15: Whether S_T is the largest natural input to the trop is arguable. 1489, line 12: Is uncertainty for 1 min, 1 hr, ?? 1490, line 4: 273 or 298K? 1492, line 29: Are there significant emission sources within one day, if not, this explanation is not believable. 1494, line 5: The filtering method is not well described. Please explain what is the goal of this filtering. 1494, line 15: My sense is that this method is going to over estimate the amt of time of S-T exchange. For example, you calculate an array of trajectories and consider the time S-T if even ONE trajectory gets to a PV of 1.6. 1495, line 7: Again filtering needs better explanation. As described, one 30 min

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point can result in selection as an S_T time. This is likely to over-estimate influence. 1497, line 24: The equation does not seem correct. It seems from the description that only one summation is actually done. I assume n is each individual S-T event. Why is O3 used, shouldn't it be DELTA O3? Table 2: See comments above. Figure 2: Caption should mention that PV is maximum along xx trajectories, where xx is the total number of trajectories.

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

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