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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Spatial and temporal variability in surface ozone at a high elevation remote site in Nepal" *by* G. W. K. Moore et al.

Anonymous Referee #1

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Review of "Spatial and temporal variability in surface ozone at a high elevation remote site in Nepal" By Moore et al. (Note: This is the title on the version i have, but is inconsistent with the title on the ACPD website.)

This paper presents surface ozone data from the high elevation ABC site in Nepal at ~5 km elevation. The authors use the surface ozone data, meteorological fields from the ECMWF model and total column ozone from the OMI instrument to draw conclusions on the sources of ozone to the region. As near as I can tell, this is the same data that has already been published and described in several earlier publications (e.g. Semple et al 2008; Cristofanelli et al 2009). The question is, then, does this analysis present a new, original and important contribution that goes beyond the earlier work?

The primary tool used by the authors has been to show that total column ozone is

correlated with surface ozone measured at the site. From this they conclude that the stratosphere is an "important contributor" to ozone in the region. I find this qualitative statement not very helpful I don't think anyone will argue that some O3 is from the stratosphere, but how much? I do not believe that simply showing the correlation supports the statement that "most" ozone in this region is from the stratosphere. As mentioned in the introduction, the strat and troposphere are generally unlinked so that changes in the total column do not necessarily indicate S-T exchange. There are several statements that I found puzzling. For example in the abstract, the authors state "…indicate the presence of ozone at elevations from 5000 to 9000 masl", but the authors have no data on the vertical distribution of ozone, so what is this statement based on?

In doing this review, I felt it important to see what others have done with the same dataset. The analysis by Cristofanelli (2009) is much more convincing and provides a quantitative estimate of the contribution of ST exchange to surface O3 in this region. In that analysis, the authors used PV, TCO, surface pressure change and black carbon observations to interpret episodes of stratospheric influence at the site. The main conclusion was that stratospheric influence could be found on 25 days (for I think the same dataset) and that on average, these days had O3 enhancements of ~5 ppbv due to stratospheric influence. While this analysis will probably underestimate the overall influence due to the stratosphere, it at least starts to develop some quantitative information. I think there are other promising methods the authors. For one thing, I am surprised that the authors did not use any other locally measured observational data. I would assume that CO, water vapor and aerosols are also measured at this site, as these can provide important constraints on the sources of O3. If not, then you should start to measure these.

By the way, why did you only use surface ozone data from noontime? Nowhere in the manuscript do you state why you did this and it seems puzzling to ignore over 90% of your data!

So overall, I do not recommend that this publication move forward. I do not see how it provides a significant improvement on our understanding of this data beyond the analyses previously published. I do have some minor comments if the authors choose to substantially augment their analysis and resubmit:

Abstract: It does not seem that you have data on O3 between 5-9 km. Also it would be helpful to state the elevation of the observatory in the abstract.

Pg 16234, Introduction, first paragraph: Its unclear what elevation you are referring to here. The importance of LS as a source certainly varies with height. You should also mention the role that photochemistry (eg PAN) plays on the spring maximum.

Pg 16239: You should refer to the paper by Cristofanelli where the instrumentation and calibration methods are described. WHY DO YOU ONLY USE NOONTIME DATA?

Pg 16242, first paragraph: The lagged autocorrelation is only slightly asymmetric. One days one and two it is nearly symmetric, but by day 3 there is a larger difference in pos and negative regime.

Pg 16245, second line: I do not feel that you have shown that TCO is a proxy for trop folds. To do this, you will need to show the vertical distribution of ozone and show that high ozone values (eg 100s of ppbv) have in fact crossed into the troposphere.

Pg 16247: There is not much new in this section. This largely repeats information presented in the results section.

Pg 16248, bottom: Citing lots of references doesn't make it so. I looked at several of these papers and don't state that TCO is a proxy for S-T exchange. As mentioned already the Cristofanelli paper reports a relative modest contribution for strat ozone, even though that contribution may be underestimated.

Pg 16250, lines 10-15: This is not a reasonable conclusion. Ozone concentrations greater than 60 ppbv are very common in urban pollution. While I am not familiar with the details of the Naja studies, the statements in this section just don't make sense.

Pg 16250, last paragraph: The authors make some odd arguments here. First, the concentrations of ozone in this region aren't really much higher than in other regions, except for a few months in the spring. Second, the concentrations get higher as you go up, but this is also the reverse of the population pattern. Finally, if in fact this is largely due to an enhanced stratospheric source (believable- but certainly not shown here), then it's not clear what you do about it.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 16233, 2009.