Review of 'Ensemble simulations of the role of the stratosphere in the attribution of tropospheric ozone variability', by P. Hess et al.

General comments

This is a well written and comprehensive paper. It makes strong and convincing arguments that the stratosphere is a key driver of tropospheric ozone variability (at least in the Northern extra-tropics). This is not a particularly new claim (e.g., the lead author has an ACP paper in 2013 which reached similar conclusions), but it is another study that supports this viewpoint, one which is rather different to that expounded by much of the recent literature on tropospheric ozone, so I think this is important. I have a few queries and suggestions (see below), but these are all relatively minor. If these are addressed, then I fully recommend publication.

Specific comments

Title: The paper is only about ozone at 30-90N (i.e. only about 1/3 of all tropospheric ozone), so I wonder if the title should be: '**Ensemble simulations of the role of the stratosphere in the attribution of Northern extra-tropical tropospheric ozone variability'**. It does make the title a bit overly long, but it is more exact.

p20462

l16 'to a surprising extent' – I suggest be more quantitative.

l17 'external' – by external I think you mean stratospheric, but this takes a bit of working out at this stage of the paper. I suggest clarify.

l21 '150 hPa 30-90N ozone flux' – clarify that you mean an ozone flux across the 150 hPa surface, averaged over 30-90N. Again, this becomes clear upon reading the whole paper, but at this stage, I wondered if the flux may refer to a chemical flux of some sort (production/destruction, etc.).

p20467

l9-20 I think I largely agree with your caveats concerning the use of a simplified tropospheric chemistry scheme. I wonder if your simple scheme has a significantly different tropospheric ozone lifetime compared to a more complex scheme, and whether this may be important for the downwards propagation of ozone anomalies? For example, your simple CH4-NOx scheme presumably doesn't represent PAN, and thus misses some long-range transport of NOx. I find it hard to gauge how important this might be. Have you compared local ozone lifetimes in this model to your results in Hess and Zbinden (2013), with a more comprehensive tropospheric chemistry scheme? This may be instructive. I am slightly worried that by using a simple tropospheric chemistry you significantly change the lifetime of ozone in the troposphere, and thus either over- (or possibly even under-) emphasize the role of stratospheric ozone relative to in-situ production.

p20469

l16 I would like a short explanation of how the four ensemble members differ – is it just a case of slightly different initial conditions?

p20473

l26 Just a grumble really: I do find figures etc. in a supplement annoying – I only printed out the main paper and not the supplement, so I have ignored them.

p20474

l8-14; l21-24 It is interesting that ozone data over Japan cannot be successfully simulated. Is all the Japanese data unreliable? Similarly for the European data prior to 1990 (or 1998). It seems a bit convenient/sweeping to discount all this data. Could you expand on why this data is considered unreliable?

p20476

l16-19 The sentence beginning 'To' is unclear to me. Do you mean: 'If the model variability arose purely due to internal model dynamics, we would expect the ozone records from the different ensemble members to be uncorrelated with each other and uncorrelated with the measurements.'?

l24 I think it may help to insert 'externally' before forced.

p20477

l5 The lower correlations nearer the surface could be related to the shorter ozone lifetime closer to the surface. I think you say as much later – it may be worth mentioning here.

Comments on Figure 7: I wonder if the trend in wstar could (at least partly) reflect an expansion in the latitudinal width of tropical upwelling (and shrinkage in extratropical downwelling)? I.e. is 30-90N all downwelling at 150 hPa, or does it include some times/regions of upwelling? (e.g., during NH summer.)

p20479

l4 I was slightly unsure – is the cubic fit with respect to time or CH4?

l8 'The long-term cubic fit to simulated ozone is not linear…' This seems a bit oxymoronic? If the cubic fit was linear, I'd hope you would have just used a linear fit, not a cubic?

p20480

l12 Not consistent with what? Presumably with emissions changes, but clarify.

l14 I think you are missing the word 'flux', which makes this confusing – i.e. I think you mean 150 hPa ozone flux.

l19 I wondered what the 5-6 months lag meant physically. Is this the ozone lifetime at 150 hPa? Or the (mean?) transport timescale from 150 hPa to the surface? I would like to understand the significance of this lag length.

p20481

l5 Suggest insert partly or largely before 'ascribed'

l14-15 The values are 0.84 and 0.73 in Table 3 (i.e. different to those in text).

p20484

l6 ‘physically deep’ – do you mean they span from 150 hPa to the surface?

p20485

l14 By large you mean ~0.4 ppb?

l18-19 ‘…high alpine sites over Europe…where amplitude of first EOF is also large.’ I can’t see high values over the Alps, either at the surface or 500 hPa in Figure 11. Clarify what you mean.

l20486

l17 I think these locations can also be usefully described as storm tracks?

p20490

l29 This suggests the external forcing is sometimes important, sometimes not?

p20510

Figure 3 (and subsequent related figures): Is the model line all stations, all of the time, or does it mimic the observations used? I guess it must be all + all, as it goes back beyond the first measurement data. Is this subtlety important?

Technical corrections

p20464 l15 decapitalise ‘Tropospheric’

p20471 l20 delete ‘the’

p20485 l5 selectED

l20486 l20 members

p20487 l26 due to

p20488 l29 entire the -> the entire

p20489 l26 four ensemble -> an ensemble of four

p20492 l23 appreciable amplitude -> high values?

p20493 l15 Should the ‘an’ be ‘any’?

p20504 Table 1: Debilt -> De Bilt