

Interactive  
Comment

## ***Interactive comment on* “Stratospheric impact on tropospheric ozone variability and trends: 1990–2009” by P. G. Hess and R. Zbinden**

**Anonymous Referee #3**

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### General comments

This important paper shows strong evidence that the stratosphere is a major source of large-scale trends and variability in northern hemisphere tropospheric ozone. The paper is important not least because this is a controversial topic, with divergent views across the research community. Perhaps I am being a little unfair, but these views are often due to vested interests in measurements from particular sites, or measurement by particular techniques, or indeed model results from a particular model (as here). In this paper, ozone measurements from a wide range of platforms (sondes, aircraft - MOZAIC, and surface sites) across mid- to high- northern latitudes are used in a consistent manner. Model simulations are from a single model: the Community Atmosphere Model with chemistry, CAM-chem. In general, I find that the relatively

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good agreement between the model and the observations provides quite compelling evidence that the stratosphere does exert a strong influence on tropospheric ozone. Where caveats exist, the authors have generally pointed to them. However, I would like the authors to address in a little more detail two important aspects.

The first is how the model simulates stratospheric ozone (using Synoz) and stratosphere-troposphere exchange (STE). The authors acknowledge that Synoz is relatively simple parameterization, and do check for the influence of any spin-up issues in Section 3. But given that the source of stratospheric ozone is crucial to the whole paper, some more details of the mechanism, including the interannual variability in STE and its location, and how reasonable this is, should be provided.

The second aspect is the role of seasonality. All of the analysis is based on 12-month running averages, so any seasonality is removed. This may well be a sensible approach, but it is well known that STE has quite strong seasonality, as does tropospheric chemistry, and that measured ozone trends differ between seasons. Some comment on what could be gleaned from further examination of seasonality would be appreciated.

My final comment is that although the paper is well written, it is overly long (it took me many sittings to get through it). There is undoubtedly a great deal of important information and analysis, but I think it could be written more concisely, and this would make it more accessible to the wider audience that it deserves. I strongly support its publication in ACP, given due attention to these comments and the specific ones listed below.

Specific comments

P22720

L19-24: Values are quoted for measured trends (necessarily) averaged over specific sites, but then for simulated trends over whole regions. It is then noted that the sim-

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ulated trends differ when sampled at the measurement sites. I think it would be more sensible to quote the directly comparable values, and then additionally mention that the simulated values for the whole region differ from the simulated values sampled at the measurement sites.

P22721

L5 (and throughout): It is Mace Head, not Macehead!

P22722

L3: Should trends be variability?

P22723

L1: 1% per year

L10: the net ozone response to climate change. Although the Stevenson et al (2006) study included 26 models, only a subset (10) simulated the impact of future climate change.

L13: from -> between

P22724

L10: clarify what is meant by 'net ozone flux' – presumably the net stratosphere to troposphere ozone flux.

P22726

L21: analyses (and also next page, l4)

P22727

L10: I think it would be clearer to say 'higher underlying ozone' than 'more ozone'.

L12-18: Is 'regionally robust' equivalent to 'sufficiently determined'. I suggest for clarity, just use one term throughout.

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P22728

L2: similar to

L10-11: It is not that useful to know that CAM-chem has been compared to measurements – it would be more useful to know if it compared well (ideally some sort of quantitative measure).

L14-16: Reword this sentence.

L17: has -> have

P22729

L4-19: What is the interannual variability in STE simulated using Synoz? Does Synoz simulate STE in the correct locations? Given the crucial nature of simulating STE for the main conclusion of the paper, more information about exactly how Synoz is implemented is required here.

L25: north of 30; delete concentration.

P22730

L9: north of 30.

P22732

L7: observational

P22733

L22: delete the.

P22736

L1-12: It is interesting that Central Europe seems more variable than other regions – could this reflect a greater anthropogenic influence?

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P22737

L23: overall

P22738

L1: the overall

L12: ..

L29: I'm not sure 'unfortunately' is the correct word here – it is just the way the Japanese measurements are.

P22739

I note there is some discussion of the simulations in Section 4, which is about observations. I recommend just keep it to observations, and save this discussion for later.

P22741

L4: Whilst strictly the 'interannual variability of emissions is constant', it is probably more usefully said to be 'zero'.

L10: Minor style suggestion – here you use 'R=0.64' etc. whereas earlier you write 'correlation of 0.64'. Keep it consistent.

L19: delete the.

P22743

L23-24: minima

P22761: Figure 1

Clarify that '12-month running mean . . . plotted as the annual average of monthly deviations' is what you refer to in the main text as 'AAMD'.

P22764: Figure 4

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It may be worth mentioning that the mean curves in Figure 4 are the same as the individual curves in Figure 3b. I wondered if it might make sense to reverse the order of these two figures.

P22770: Figure 10

The lower panel should not be entitled 'Anthropogenic ozone' – tropospheric is better – i.e. it must include ozone produced from natural sources in the troposphere.

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