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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Characterising the Seasonal and Geographical Variability of Tropospheric Ozone, Stratospheric Influence and Recent Changes" *by* Ryan S. Williams et al.

Anonymous Referee #1

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The paper by Williams et al. "Characterising the Seasonal and Geographical Variability of Tropospheric Ozone, Stratospheric Influence and Recent Changes" utilises satellite and ozonesonde observations and two chemistry-climate models to investigate the stratospheric influence on tropospheric ozone. The authors conclude that the influence of stratospheric on tropospheric ozone is larger than previously thought. The authors also assessed the tropospheric ozone over the periods of 1980-89 and 2001-2010, and find an overall significant increase in tropospheric ozone, and attribute 25-30% changes at the surface and 50-80% in the upper troposphere to the stratosphere-troposphere exchange. The paper is well written, and the analysis is generally thorough, but some clarifications and improvements are needed before the paper can be

accepted for publication in ACP. Detailed comments are listed below.

General comments:

A major concern is that this study includes only two CCMI model results, which reduces the robustness of the finding, especially in that "the influence of STE in the tropospheric ozone is larger than previously thought". Furthermore, using only simulations constrained with prescribed dynamics might obscure the changes due to dynamical feedbacks, especially when assessing long-term changes in ozone. Therefore, I suggest that the authors tone down the conclusion mentioned above, and instead focus on the uncertainty in the contribution of STE to the tropospheric ozone. The limitation of using prescribed dynamics CCM simulations should also be noted and discussed. A wider usage of CCMI models would address the first comment. As a minimum, the authors should give a reason for using only the two chosen models.

Regarding previous studies, I doubt that the paper by Lamarque et al. (1999) is still a very relevant reference that the authors focus their comparisons on, given that the approach used in Lamarque et al. (1999) was very simplistic compared to what can be achieved using more recent state-of-the-art CCMs. Also, there are a few more studies that have investigated the impact of STE on tropospheric ozone which the authors failed to cite, for example, Lelieveld and Dentener (2000), Hess et al. (2013), etc.

Jos Lelieveld and Frank J. Dentener, What controls tropospheric ozone? JGR, 105, P3531-3551 2000.

P. G. Hess and R. Zbinden, Stratospheric impact on tropospheric ozone variability and trends: 1990–2009, ACP, 13, 649–674, 2013.

Therefore, a more thorough review of the recent literature would be desirable.

Specific comments:

P2, L13: add "large" before "number"

P4, L17: please add references here

P5, L5-L20: Can you describe the models' characteristics in a more objective way here? Why do you choose these two models specifically (there are quite a few other models from CCMI that you could include)? Also describe the main differences between these two models.

P5, L28: Pease provide more details in chemical schemes used in EMAC.

P6, L4 & L21: Please provide more detailed information on how the O3S tracer is defined in terms of its chemical and dynamical nature in both models.

P7, L19-L21: Please clarify if the AKs have or have not been applied to the modelled and ozonesode data when you compare these two. It only makes sense to apply AKs when compare model/sonde data to the satellite data.

P7, L27-L29: I don't understand why "The 1000-450 hPa (0-5.5 km) OMI subcolumn data is considered a representative approximation of the full tropospheric ozone column amount, due to vertical smearing of information from above 450 hPa ($\sim$ 5.5 km)." is the case. Is it possible to show AKs?

P9, L1-L4: Please add references here. There are a series of publications on JOSIE by Smit et al.

Smit, H. G. J., and D. Kley (1998), JOSIE: The 1996 WMO International intercomparison of ozonesondes under quasi flight conditions in the environmental simulation chamber at JuÂÍlich, WMO Global Atmosphere Watch report series, No. 130 (Technical Document No. 926). World Meteorological Organization, Geneva.

Smit, H. G. J., and W. Straeter (2004a), JOSIE-1998, Performance of ECC Ozone Sondes of SPC-6A and ENSCI-Z Type, WMO Global Atmosphere Watch report series, No. 157 (Technical Document No. 1218), World Meteorological Organization, Geneva.

Smit, H. G. J., and W. Straeter (2004b), JOSIE-2000, JuÂÍlich Ozone Sonde Inter-

comparison Experiment 2000, The 2000 WMO international intercomparison of operating procedures for ECC-ozonesondes at the environmental simulation facility at JuÂÍlich, WMO Global Atmosphere Watch report series, No. 158 (Technical Document No. 1225), World Meteorological Organization, Geneva.

P10, L19: Ozone is not at its minimum in SON in the SH, but maximum. Biomass burning emissions and STE usually dominate the seasonality of SH O3.

P11, L21-23: can you provide more details on the difference between these two NOx emission datasets?

P12, L1-L2: This seems slightly mis-leading on the function of the AKs. The purpose of applying the AVKs is to compare like with like.

P12, L4-L7: Similar features seem exist in both models; it seems more likely due to transport barrier than STE (which the STE maximises in winter).

P12, L7-L9: Does the difference in chemical schemes between the two models play a role here?

P13, Fig 3: It is impossible to discern the RSD of ozonesonde data denoted as circles, due to a uniformed colour scale.

P14, Fig 4: The value of 100 ppbv O3 seems a bit too low for defining the tropopause. Using 100 ppbv O3 also deviates from the definition by Bethan et al. (1996) (cited in caption), which is based on the ozone gradient, defined as the minimum altitude where the vertical gradient of the O3 VMR is greater than 60 ppb/km, remains so for a further 200 m, and the O3 mixing ratio is greater than 80 ppbv, exceeding 110 ppbv immediately above the tropopause.

P15, L16-L32: Please note the figures that you are discussing throughout this paragraph. Also, the description/discussion in this paragraph can be simplified to focus on the main points.

P16, L1-L2: again, please can you refer to the figure(s) that you are discussing.

P16, L27: Do you also apply AVKs to model data when compare them with ozonesonde data? If so, it is not necessary.

P16, L28: do you mean “simplified” tropospheric chemistry scheme? It is unusual to use the word “conservative”.

P16, L29-L31: which comparison/figures you are talking about here? Please make it clear by referring to figures.

P16, L32-L33: It seems lacking context regarding “since vertical smearing of information is far more limited due to a higher tropopause.”. Could you be specific? Where is the information regarding a higher tropopause?

P17: L1: “must induce” should be “must have induced”

P17: L3-L7: Showing the AKs might help with the discussion here.

P21, L31: Please provide details on how you map the model data to ozonesonde measurements shown in Fig 7?

P23, L1: it is too general to say that “. . . are evident in the contemporary CCM simulation” while only two CCMs are used here.

P24, L11: What do you mean "even lower tropospheric ozone"?

P24, L20: What is the rationale for choosing these longitudes?

P25, Fig 8: There are large areas in the SH that are denoted significant in CMAM (SON 500 hPa and SON surface plots), which are not reflected in the relevant discussion. Please check.

P27, L22: Please note which figure(s) you are discussing here? Is Fig 8?

P30, L10-L12: is “subtle shifts in the height of tropopause” shown anywhere?

P34, L10-L12: What are the reference variables for these percentage changes? P35, L2: Please specify re “some regions of the world".

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