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

Anonymous Referee #2

Received and published: 16 September 2011

(1) This article needs to be greatly shortened and re-organized for clarity before publication.

(2) Further research is needed to convince the scientific community that the simulated ozone is credible. The problem is that the authors claimed to use Synoz but tweaked Synoz inappropriately by prescribing the concentration in the source region. It is likely that the simulated stratospheric ozone is way off and the STE fluxes are not realistic. The authors should either use Linoz (McLinden et al., 2000) to redo the simulation or validate the simulated ozone (e.g. zonal mean latitude-height distributions) and overhead column ozone distributions by comparing to the observations. There are plenty of satellite data (TOMS, MLS, ...) with global coverage out there that the authors can use to validate the basic profiles of the simulated ozone.

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(3) This article mainly focuses on the trend in time series. Why isn't there any discussion on the simulated downward trend since 2003 for tropospheric ozone in Figure 1. I find it perplexing that the annual NO_x emission is held fixed throughout the simulation, yet there is a clear downward trend in tropospheric ozone.

(4) If the upward stratospheric ozone trend seen in the simulation is indeed due to the strengthening of Brewer-Dobson circulation, isn't it relatively straightforward to verify it by plotting the time series of the downward mass fluxes of the BDC (defined by the residual circulation) from the meteorological data?

(5) Going back to point (1), I find this article very difficult to read through because it is very wordy and lack of focus. I believe that the authors can and should do a lot better job on presentation. In this regard, the authors can benefit from the other reviewer's comments.

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 11, 22719, 2011.

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