

Interactive comment on “Variability and trends in total and vertically resolved stratospheric ozone” by D. Brunner et al.

Anonymous Referee #1

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The manuscript “Variability and trends in total and vertically resolved stratospheric ozone” by Brunner et al. discusses variability and trends in Candidoz Assimilated Ozone data set (CATO). The CATO data set itself is described by Brunner et al., (2006). The analysis of long-term trends in the data is a logical step for the CATO authors. The data set could be useful for many applications and it is important to know about the long-term component in CATO. The manuscript could be published after minor changes (see specific comments), but its title should reflect the fact that CATO data are analyzed in the paper. However, trends in CATO are not necessary the same as trends in “real” ozone.

General comment

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I am not entirely convinced that CATO is able to reproduce actual long-term variations in vertical ozone. This issue is not discussed in this manuscript. The original paper by Brunner et al. (2006) also does not provide enough evidence to demonstrate the suitability of CATO for analysis of long-term ozone changes. There is a possibility that total ozone long-term variations may have too much influence on CATO long-term variations due to the reconstruction method. This should be analyzed further, perhaps as a separate study. One way to verify that is to look at low stratosphere data from northern midlatitudes. Ozonesonde data there shows that below ~16 km ozone declined to the mid-1990 and then bounced back to the early-1980s level. Above ~22 km, sonde, SBUV, and SAGE data demonstrated that ozone changes followed the EESC curve: they declined till mid-1990s and then leveled off. Is CATO data able to reproduce this behavior?

Specific comments

1. The statistical model (1, 2) has 56 coefficients that are estimated from 312 data points. Is it really necessary to have that many coefficients? How many of them are statistically significant? Perhaps it is enough use one harmonic instead of 3 to account for the seasonality in (3). For example, the seasonality of the VPSC effects has already been taken care of by (4).
2. Surface aerosol area used as a proxy for volcanic signal suggests that the magnitude of the El Chichon signal in ozone is about 4 times smaller than the Pinatubo signal. Other proxies commonly used to estimate the volcanic effect (e.g., aerosol optical depth) have much less difference between the two eruptions. It would be interesting to estimate the El Chichon signal directly, for example, by reproducing Figure 2c from data with the 1991-1995 period omitted.
3. There have been many discussions recently about the “right” EESC function. EESC is not the same for polar and low latitudes and for the upper and lower stratosphere, not even mention the troposphere. It is unlikely that the choice of EESC would have a

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major impact on the results, but the authors should clearly state what EESC they use.

4. How large are the autocorrelation coefficients of the residuals for different levels (page 6328, l. 10)? Add a sentence or two about it.

5. It would be better to show deseasonalized EP fluxes in Figure 1. Otherwise it is difficult to see year-to year variability.

6. The authors used too many abbreviations and not all of them were properly handled. Some of them (SVD, NAT, AO, SAD) were defined, but used only once or not used at all. CANDIDOZ was used on pages 1 and 2, but was defined only on page 4; NAO was defined twice, EESC was used in the abstract without any definition.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 6317, 2006.

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