Atmos. Chem. Phys. Discuss., 3, S1089–S1096, 2003
www.atmos-chem-phys.org/acpd/3/S1089/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global distribution of total ozone and lower stratospheric temperature variations" *by* W. Steinbrecht et al.

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Received and published: 25 July 2003

1. Issues raised by both referees

(A) Both referees criticize the use of 2 standard deviations (2σ) of the terms $c_x X$ in equation (1) of the discussion paper. We used 2σ, in Dobson Units (DU) or Kelvin (K), to describe the size of ozone or temperature fluctuations attributed to the different explanatory variables X. The referees would prefer just the regression coefficients c_x, e.g. in $DU/m/s$ for the QBO or zonal wind at 60^o explanatory variables, or 10^{23} $DU/W/m^2/Hz$ for the 10.7 cm solar flux explanatory variable. Our reasons for using 2 standard deviations of the terms $c_x X$ were that this a.) gives a better idea of the typical minimum to maximum ozone/ temperature fluctuations, and b.) allows direct comparison of the size of fluctuations attributed to different explanatory variables. For these reasons we wish to keep the maps of $2\sigma(c_x X)$. However, because

$c_x = 2\sigma(c_x X)/2\sigma(X)$, and for nearly all explanatory variables $2\sigma(X)$ is constant over the map, the request of the referees will be addressed by adding a second scale to the maps in Figures 2 to 11. The maps will then give the regression coefficient c_x directly, as well as 2σ. For Figures 3, 400 hPa temperature, and 8, aerosol, this is not possible, because $2\sigma(T_{400})$ is not a constant, but varies with latitude and longitude. Here we will add some text giving the typical size of c_x.

(B) Regarding Table 1, we agree with both referees. As suggested by referee #2, Table 1 will be changed to give a summary of the observed magnitudes and will be moved to the conclusions section.

(C) We agree with both referees that data quality is an important issue and needs to be mentioned. Regarding total ozone, Fioletov et al. (2002) and Harris et al. (2003) have compared various total ozone time series, from ground-based spectrometers, from an assimilated TOMS/GOME/ground-based data-set at NIWA (Bodeker et al., 2001), and from the TOMS/SBUV merged data-set used in this discussion paper. For zonal means over large latitude bands, e.g. 35°N to 60°N, Fioletov et al. (2002) report differences between these data sets that vary over time, but are generally less than 1% ($\approx$ 2 to 4 DU). Harris et al. (2003) only report on low-pass filtered data where most known sources of variability, such as solar-cycle, QBO, 500 hPa temperature, etc., have been removed. They only consider fluctuations on time- scales longer than the QBO, and do find a smaller long-term trend in the merged TOMS/SBUV data set. Apart from that, they find time varying differences between the data sets that are typically smaller than 1% for selected smaller regions, such as the grid cells used in our analysis. Time varying bias of the merged TOMS/SBUV data-set will certainly affect our results. However, based on Fioletov et al.'s and Harris et al.'s results, it seems that errors should be of the order of 1 to 2%, corresponding to a few Dobson Units. This is comparable to the statistical uncertainty of our results, which is typically larger than 2 to 5 DU. Therefore we think that data consistency of the TOMS/SBUV data-set is not a major issue for our analysis.

The NCEP reanalysis data do exhibit quality changes over time which are related mostly to changes in the observing system. The largest known jumps in NCEP reanalyses occur at the introduction of satellite observations in late 1978 (Santer et al., 1999, Trenberth et al., 2001). Since our discussion paper uses mainly the 1979 to 2001 data, it should not be affected by this major jump. Even when the extended 1958 to 2001 period (major jump in late 1978) is considered, nearly all of our results, except for the linear trend, are very similar to those obtained for the more homogeneous 1979 to 2001 period. We would expect that possible smaller quality changes after 1979 would result in even less noticable changes. Independent support for the case of the 11-year solar cycle comes from vanLoon and Labitzke (1999), who found very similar results for NCEP reanalysis and Berlin stratospheric analyses for the 1968 to 1996 period, which includes the major jump in NCEP reanalyses.

A detailed investigation of temporal inconsistency issues for the TOMS/SBUV or NCEP reanalysis data-sets is clearly beyond the scope of our discussion paper. However, the above mentioned information indicates that there are no major inconsistencies in the two data-sets used here. Our results should, therefore, be representative for the "true" atmosphere. We will add paragraphs on data quality issues to section 2.

2. Specific points adressed by referee #1

(1) see (A) above

(2) We disagree with the referee. Our results show that significant ozone and 50 hPa temperature variations can be attributed to the explanatory variables 400 hPa temperature, polar vortex strength, aerosol, or ENSO. The purpose of our study is to present all the important contributors. The similarity of Figures 4 and 5 does indicate that polar vortex strength and QBO are reasonably independent (see also paragraph 4 and 5 of section 3.3). Contrary to referee #1 we would argue that 400 hPa temperature

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fluctuations largely reflect "chaotic" tropospheric weather and are not an immediate consequence of QBO or solar-cycle forcings. We do agree with referee #1 that restricting the paper to trend, QBO and solar-cycle only would leave room for presenting all seasons and a more detailed discussion. However, in that case other important influences would be left out. This is not the goal of our paper. As indicated several times in the paper, results do not change much when only a sub-set of the explanatory variables is used.

(3) We will follow referee #1's suggestion and include results for the solar- cycle term where the full data-set, without separation according to the QBO- phase is analysed using Equation (1). Note that while referee #1 suggests to omit the analysis of separate sub-sets for easterly and westerly QBO phase, referee #2 suggests to expand this type of analysis from the solar-cycle explanatory variable to ENSO and other explanatory variables. As indicated later in our response to referee #2, we feel that our present choice, presenting results for QBO sub-sets for the solar-cycle only, is a good compromise. As pointed out by referee #1, Equation (1) is used without QBO-terms for the sub-sets grouped according to QBO-phase. We will add a clarification to section 3.4.

(4) see (C) above

(5) The mentioned spelling mistakes are corrected in the published discussion paper. We do not see a typographical error in the last line of the caption of Figure 2.

3. Specific points adressed by referee #2

We agree, a regression analysis cannot differentiate between cause and effect. Therefore the terms response, influence or effect are often not appropriate. We will reword the manuscript, so that in general no causal relationship will be implied.

As mentioned in section 2 paragraph 2, analyses for 50 hPa temperature were done based on the 1979 to 2001 NCEP data and, separately, on the 1958 to 2001 NCEP data. For all explanatory variables, except the linear trend, the results are nearly the same. Although this is mentioned in section 2 paragraph 2, we will change the text to make this clearer. We will also add a sentence to sections 3.2 and to the conclusions section.

We are not aware of a linear regression that allows for the direct determination of an optimum QBO time lag. Thus, using QBO(10) and QBO(30) seems computationally more efficient, because only one stepwise linear regression needs to be done for each grid point. Using a single QBO time series with varying time lag is less efficient, because about 14 stepwise linear regressions, each with a different time lag, have to be tried at each grid point. From these 14 regressions the best one (highest R^2 ?) has to be selected.

As pointed out by Fioletov and Shepherd (2003), the strong seasonal persistence/ autocorrelation between late winter/ spring and the following summer only appears for large area averages (e.g. 30° to 60° zonal mean), but not for single station data, or individual grid-cells, as used in this analysis. In our analysis the Fioletov and Shepherd seasonal persistence shows up as a strong similarity between large scale patterns in the DJF, MAM and JJA maps (northern hemisphere), or in the JJA, SON and DJF maps (southern hemisphere). For nearly all explanatory variables, the winter and spring maps are very similar, and the summer maps retain many large scale features, but to a lesser degree.

We share the referees doubts about changes in O_3, CO_2 H_2O being the sole cause for the lower stratospheric temperature trend. We will delete the last sentence of section 3.1.

We agree, the description at the end of 3.2 is too simple. Horizontal motions also play an important role (Salby and Callaghan, 1993; Koch et al., 2002). We will change the last paragraph of 3.2. to include this.

Paragraph 2 section 3.3. The QBO induced secondary circulation is superimposed on the main meridional Brewer-Dobson circulation. Upwelling or downwelling in the secondary circulation will enhance or reduce the motions of the main Brewer-Dobson circulation, but the Brewer-Dobson circulation is not stopped or reversed. We will change the text to make this clearer.

Indeed, possible aliasing between volcanic and solar signal is already mentioned in Solomon et al. (1996). We will add this reference. However, McCormack et al. (1997) investigated the problem in more detail.

We did investigate a possible QBO modulation of ENSO (and other explanatory variables) by separating the data-set into subgroups with QBO easterly and westerly winds. The most significant difference between these subgroups is found for the solar cycle explanatory variable and is presented in section 3.4. For ENSO, we see some differences between the two QBO subgroups. We don't think that the differences are significant enough to be included in the paper. Similarly we find some differences between the QBO subgroups for the polar vortex strength explanatory variables, but don't think that they are worth mentioning at this point.

We agree. The results found in our analysis may be changing as climate changes. We will remove the two last sentences of paragraph 2, section 4.

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