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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

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The authors present results of a multiple regression analysis of intercalibrated TOMS/SBUV satellite column ozone data and NCEP Reanalysis 50 hPa temperature data for the period from late 1978 to December of 2001. The regression model includes as explanatory variables 400 hPa temperature and zonal wind at 10 hPa, 60N and 60S as well as the usual linear trend, solar cycle, QBO, stratospheric aerosol, and ENSO variables. Selected results are presented for each explanatory variable in Figures 2-11 as a function of latitude and longitude. In some respects, the paper usefully complements a recent study of long-term ozone variations by Harris et al. (Atmos. Environ., v. 37, p. 3167, 2003). The current paper concentrates on presenting details of the geographical distribution of the components of ozone change and attempts some initial interpretations. I have a number of comments for the authors to consider in revising their manuscript prior to publication.

(1) Normally, results of analyses of this type are presented by plotting the regression coefficients (e.g., DU/year or K/year for trends). This is done by the authors in Figure 2 but for remaining figures, results are presented as a fraction of ``twice the standard deviation of the corresponding time series term". Table 1 allows one, in principle, to convert the numbers on the plots to regression coefficients (e.g., DU per 10 m/s of the QBO zonal wind). However, it is not a simple process and this may lead to some confusion by readers. The authors should either change their units or explain (in the text) why this procedure is being followed. I also can not find any reference to or explanation of Table 1 in my copy of the manuscript.

(2) A comparison of Figures 4 and 5 shows that the QBO response is not very different when only the QBO, solar cycle, and linear trend terms are retained in the regression model. Adding the other terms greatly increases the complexity of the analysis with only a small benefit in improved accuracy of the response distributions. The authors should seriously consider eliminating these other terms and presenting only results for the linear trend, QBO, and solar cycle components. This is especially true if analyses of the solar cycle are to be done in separate phases of the QBO (see comment below). This would allow presentation of results for all seasons (rather than for only a selected few as done here). It would also allow more detailed discussions of interpretations for the most important sources of long-term variability. It can be argued that the 400 mb temperature and polar vortex winds are not really forcings, but are instead merely symptoms or consequences of one or more fundamental forcings. For example, the polar vortex winds are not really independent of the QBO or solar cycle. Including them in the regression model only complicates the analysis, in my opinion.

(3) Presentation of solar cycle component results for separate phases of the QBO (as is done in Figures 6 and 7) is inconsistent with equation 1, which includes the QBO winds as explanatory variables. As shown by Labitzke and van Loon, the phase of the QBO does modify the solar cycle results at high latitudes, especially in the winter hemisphere. This QBO modulation is not amenable to analysis by a linear regression

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model of the form considered here. However, at lower latitudes, there is a solar cycle variation of both total ozone and lower stratospheric temperature that can be modeled to first order as independent of the QBO phase. I therefore urge the authors to present first the direct solar cycle component obtained when all of the data are considered and equation 1 is applied directly (or, even better, a simplified version of equation 1 which retains only the 3 main explanatory variables). If they wish, the authors may also present results after separation by QBO phase. But, in my opinion, doing so has more negative effects than positive ones on the manuscript.

(4) In addition to the merged satellite ozone data record developed by Stolarski et al., which is calibrated to EP-TOMS, there is another merged (or ``assimilated") satellite total ozone data set developed at New Zealand's National Institute of Water and Atmospheric Research (NIWA) (Bodeker et al., JGR, v. 106, 23029-23042, 2001). Trends derived from the latter data set, which is calibrated to ground-based Dobson measurements, can be significantly different from those derived from the Stolarski et al. data set (see Harris et al., 2003). It is therefore important to mention this other record in section 2 and to explain why only the Stolarski et al. record is considered in this paper.

MINOR CORRECTIONS:

(5) Several words, including symmetry, asymmetry, and Siberia are misspelled. There are also a number of typographical errors that need corrections. "Anti-correlated" should be replaced by "inversely correlated". The typographical error in the last line of the caption to Figure 2 is especially important since it prevents one from understanding the statistical significance of the trends.

Overall, this is a useful analysis of the available global satellite total ozone data. However, a few carefully chosen changes will significantly improve the manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 3411, 2003.