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Interactive Comment

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

Anonymous Referee #2

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GENERAL

There is a lot of good analysis of the relationships of ozone and temperature and a number of important geophysical quantities. Such relationships help us understand the linkages in the atmosphere and are important in assessing the performance of atmospheric models. However, it is quite hard to extract the main findings of the work.

SPECIFIC COMMENTS

The authors should consider presenting the results in more easy-to-grasp way, e.g., by including a table of the results that they discuss in the summary/discussion section. I think the most helpful quantity is the observed magnitude and uncertainty/variability for each relation, e.g. a typical solar cycle or a typical QBO cycle. The current Table 1 does not achieve this as the units are far from intuitive even the regression coefficients

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can be calculated. Personally I find the use of 2 sigma confusing, though whether that is a criticism or a confession I am not sure.

Great care needs to be taken with language describing complex relationships to avoid implying cause and effect accidentally. For example, the word `response' is used three times in the abstract. That is fine for the solar cycle, but not good for 400hPa temperature. `Influence' is also used a lot throughout the paper - sometimes accurately, sometimes not

It is not clear exactly how the longer NCEP analyses from 1958 on have been used. There are a few mentions of this which could be expanded slightly.

There is no comment about the data quality issues for either the NCEP or TOMS/ SBUV datasets which are relevant for this study.

Do the authors have any reason to think that using QBO (10hPa) and QBO (30hPa) is computationally more or less efficient than a single QBO time series plus the best time lag?

The regression analysis is done separately for each seasonal mean anomaly series and negligible autocorrelation is found. This is not surprising, but it does make one wonder what has happened to the autocorrelation / seasonal persistence that undoubtedly does exist (e.g. Fioletov and Shepherd, GRL, 2003). Have the authors performed any analyses using a combined series with different predictor time series for each season to investigate whether this affects their calculated uncertainties and the residual R2?

It is a bit of a worry that the concept that the temperature trends being consistent with changes in lower stratospheric ozone, CO_2 and H_2O is so widely quoted. It is true, but it is not a strong paper given the large uncertainties involved. In this paper (end of 3.1), it does not seem particularly relevant, so I would recommend deleting the last sentence of that paragraph.

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At the end of 3.2, the authors explain the variations in total ozone with tropopause height without reference to the work by Koch et al (JGR, 2002) showed that the tropopause height changes are accompanied by in(de)creases in advection of air from lower latitudes. The description here is too simple.

I do not understand the explanation in para 2 of section 3.3. Is there really downwelling or just reduced upwelling?

In para 2 of section 3.5, the authors credit McCormack et al as being the first to argue the aliasing between solar and volcanic effects. Is this right or was it Solomon et al., 1996?

In para 2 of section 3.7, the authors discuss the effects of El Nino and La Nina. Did they investigate whether there is a QBO modulation of the ENSO? In fact are there any significant differences in the other explanatory variable coefficients between the QBO-east and QBO-west analyses? There might be enough data to look at this now.

At the end of Section 4, para 2, the authors discuss recovery as if the relations quantified in their analysis will not change with climate change. Either they need to make this discussion more complete or (and my preference) they should remove the current short discussion altogether.

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

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