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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Analysis of HCl and ClO time series in the upper stratosphere using satellite data sets" *by* A. Jones et al.

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We would like to thank the reviewers for their comments regarding this paper. We feel that this paper helps highlight significant results which not only help confirm previous work, but also help by extending research to a more current date. Here are the replies to the comments made by the reviewers:

Sect 2 p8626 l. 20/21: are the combined systematic and random uncertainties really relevant to trend estimation? 'systematic' usually means fully correlated in the time domain, so that they cancel out when the trend is derived. Possibly the confusion arises from an ambiguous definition of the term 'systematic uncertainty'.

The reviewer is indeed correct that the systematic and random uncertainties in a trend

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analysis are of little relevance as described above. However, our motivation for including this information is simply due to give the reader an idea about the quality of data involved, thus including by including this information helps to provide this information.

Sect 3.1, p8631 l14/15: It should be mentioned that subtraction of the mean e.g. January value from each January data point in order to deseasonalize the data will affect the axis entercept b.

Agreed and added

Sect 3.1, p8631 l16/17: what does “each data set” mean? HALOE vs. ACE-FTS etc.? Or January vs. February etc.? I did not quite get the point here.

We have now changed this: “Hence, the resulting HALOE and ACE-FTS time series will be monthly mean HCl time series with the seasonal components removed, but still containing contributions from the QBO. In order to model the HCl anomalies from HALOE and ACE-FTS time series (with the seasonal component already removed) we apply a linear regression model, accounting for fluctuations related to the QBO”.

Sect 3.1, p8631, Eq 1: This equation would be much easier to understand if [QBO also had a time index, i.e. [QBO]t. This is because the generic term QBO cannot be represented as a scalar, and it is not quite clear how to add it to the other scalar terms. [QBO]t would be one component of the QBO-vector and would better fit in the equation.

This was an overlook by our part, this would have been added. We have now updated this equation which complies with the below comments. We thank the reviewer nevertheless for spotting this!

Sect 3.1, p8631, Eq 1: it is not quite clear what [HCl]t really is. If it is the regression model, then why does in include the white noise terms and the autocorrelated error terms? A regression function only includes predictable components of the time series. Or is [HCl]t actually the measured data? Clearer terminology is necessary here.

We have tried to clarify this ambiguity. Simply the [HCl]t are the HCl anomalies calcu-

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lated for each month t. I.e the anomalies produced after removing the seasonal cycle. We follow the Newchurch et al method here. To help distinguish the anomalies from that of a measured time series, we have changed [HCl]t to [ΔHCl]t. We hope this simplifies things.

Sect 3.1, p8631, l24ff: Doesn't evaluation of a power spectrum need a stationary time series, i.e. one with no trend? How did you solve this problem? By an iterative approach, where a first guess trend is subtracted prior to the FFT? Or are trend and amplitudes of the periodic function fitted in one step? Here it would be helpful to mathematically formulate the optimization problem (or, if applicable, the sequence of optimization problems), i.e. to report the cost function(s) to be minimized in order to make clear for each step which are the fit variables and which are the pre-fitted parameters.

The FFT, or fast fourier transform is an algorithm that essentially uses convolution techniques to efficiently find the magnitude and location of the tones that make up the signal of interest. The use of a FFT in this case is simply to help indicate the most powerful periods in a time series. We only apply the FFT to the deseasonalised anomalies, in order to examine which harmonics are believed to be related to the QBO. These harmonics are typically between 7 and 32 months [as found by Steinbrecht et al, 2004], which agrees with the typical cycles associated with the QBO. We can be quite sure that the specific harmonics chosen for a given deseasonalised time series are attributed to the QBO, by comparing the model output of the QBO component (calculated from equation 1) to a QBO proxy, such as that provided by the Singapore winds. For the tropics latitude band, we expect the phases between the QBO proxy and the modeled QBO to match quite closely, while in the extra-tropics there will be a phase shift in the modeled QBO output due to the delay in the transport of air from the tropics. We have added some more information to try and summarise this a little better.

Sect 3.1 p 8632 l1: Do you mean "Equation (1) can be solved for predetermined [QBO]t by ...", or are the amplitudes of the QBO-components of predetermined phases and period lengths retrieved also in this least squares analysis? Please make a clear state-

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ment on which the variables of the least squares problem are. Perhaps it might actually be helpful do include another equation which represents the object function of the least squares analysis (see above).

Yes the QBO terms are predetermined. This is now stated. We have now changed the equation 1 which now shows the sum of the individual sin and cosine components defined by coefficients c and d. Thus during the regression analysis, a, b, c, and d all need to be determined in order to determine the total QBO contribution from the deseasonalised anomalies. We have tried to explain this in the text and hopefully this is now a little clearer.

Sect 3.1 p 8632 l2: Are the residuals to be minimized weighted by the inverse variance of the related data point, or do they all have the same weight?

All residuals are weighted equally. This we believe is also done in the Newchurch and Steinbrecht linear regression models as well.

Sect 3.1 p 8632 l4: It is not easy to get the point here: Why has the seasonal component be removed again? Hasn't the time series already been deseasonalized?

Yes, this is a typo. The anomaly time series have the seasonal component already removed. This has been changed.

Sect 3.1 p8632 l9: Since the Reinsel method seems to play a key-role in this paper, this method should be shortly summarized here. If necessary, even another equation could be included for this purpose. Particularly, it is not clear if the Reinsel method is applied after the least squares fitting to get the trend, or if these methods interact somehow. Due to the importance of the Reinel method for this paper, a mere citation is not sufficient.

We have now added a short paragraph summarizing the Reinsel et al method.

Sect 3.1 p8632 l11: I did not get the point why interpolation is necessary, and how these additional data points, which do not add any information but are fully dependent

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on the other data points affect the fitting procedure and the significance estimation.

Indeed, this is true. We had some bugs in the code which initially made us interpolate to fill in the gaps so as to fix this. Since then we have gone back and fixed the bug such that interpolation is not needed. Hence, we have removed this sentence which was overlooked during reading of the initial manuscript.

Sect 3.1 p8632 l15: Again, it is not clear why extrapolation is necessary and how it affects the data analysis.

Here, we are simply trying to apply an offset to the ACE-FTS data so as to align and have it fit relative to the HALOE data in terms of time. As HALOE terminated in 2005, it means that there is very little overlap with ACE-FTS. We use the word "extrapolate" as we are extending the fit line applied to the HALOE anomalies beyond its termination date so as covers the whole ACE-FTS period. From here we can calculate the offset. The most important assumption here is that there are no sharp apparent changes in trend in the ACE data.

Sect 3.1 p8632 l21: The removal of the offset certainly is valid but depending on the variability of the atmosphere and measurement noise - which both are reflected by the scatter in the time series - there will be a residual offset uncertainty. Is this small enough that it can be disregarded? If not, please note that this residual offset uncertainty contributes with a correlated error term to the error budget.

Yes, this is a valid point. We have added this information.

Sect 3.2 p8634 l 24: The term "scaled vertical profiles" is misleading. A scaled profile ususlly is understood to be a profile where each profile value is multiplied by the SAME scalar. I understand that the opposite is true here: The correction term is altitudedependent. Instead of "scaled vertical profiles" I suggest to write "profiles corrected for diurnal variation according to Eq. (2)."

We have changed this to the suggested sentence.

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Sect 4.1 Fig 3: The discontinuity in the HALOE data in 2001 (the sharp increase) needs some explanation or discussion. Has the atmosphere abruptly changed or did the instrument characteristics or measurement mode change? Or is this an artifact because the Reinsel method might assume an abrupt change in trend while the actual trend changed rather continuously than abrupt?

We have commented on this apparent departure from the apparent post 1997 downward trend of data points in section 3.1. It is our current understanding that this feature is not understood. "Interesting features are the peak in HALOE values until around 1997 after which values start to slowly decrease until instrument termination at the end of 2005. However, there is a large degree of variability in the values between 1997 and 2002, which is also seen in previous findings for HALOE HCl observations at 55 km [Waugh et al., 2001]. This variability is currently not understood [Waugh et al., 2001, WMO, 2006]." We have however, written a brief sentence or two saying that the removal of the seasonal and QBO cycles do not remove this feature.

Sect 4.2 p8637 l1 " diurnal correction factors" instead of "scaling factors" would be clearer, see above.

This has been changed to the above suggestion.

Sect 4.2. Fig 7, lowermost panel: same problem as with HCl: how is the 2000/2001 discontinuity explained?

See above

Sect 5 p8637 l1: since the trends are negative, the term "lower trend" is somewhat ambiguous (is the number smaller or is the absolute value smaller?). "less negative" would be clearer.

This has been changed to the above suggestion

Sect 5 p8638 l26: not quite clear what 'model errors' are. CMAM errors? Errors of the regression model? From the context I would guess this refers to the regression

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analysis; but is it really the model errors? I think 'regression parameter errors' would

This has been changed to Regression parameter errors.

Sect 5 p8640 l4: "magnitudes found are typically smaller than those of the chlorine species reported here" would be clearer. Otherwise the reader might wonder where in this paper ozone trends are reported

We have changed to the above suggestion

Technical comment: Sect 3 p 8631, around Eq 1: The grammar looks funny to me: Either include Eq (1) in the sentence, like:...takes the approach

[HCl]t = b + at + [QBO]t + Nt; (1)

where ...or start a new main clause, like ...takes the following approach:

[HCl]t = b + at + [QBO]t + Nt (2)

Here ...

We agree to this and have changed to the first approach.

Finally, the reviewer maybe wondering why our HCl trend estimates have changed from the initial manuscript? We have tried to improve our use of the FFT to determine the harmonics of the QBO for each deseasonalised set of anomalies. We believe that we were possibly overestimating the QBO contribution for HCl.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 8623, 2010.

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