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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A new method to detect long term trends of methane (CH_4) and nitrous oxide (N_2O) total columns measured within the NDACC ground-based high resolution solar FTIR network" *by* J. Angelbratt et al.

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Summary:

Angelbratt et al. provide a new method for deriving trends of CH4 and N2O from ground based FTIR measurements. The new method takes more atmospheric parameters into account that may have a significant influence on the total column measurements. While the results are not strikingly different from the ones derived with earlier (and simpler) methods, their method should be more robust. It takes care of more systematic effects

and thus produces residuals that are closer to a normal distribution - as it is assumed by most statistical methods but is often not true. Unfortunately, it was only used on a very limited number of stations.

One general comment: the authors cite Weatherhead et al. 1998 twice but only on the issue of autocorrelation. In my opinion, the most important result from Weatherhead et al. 1998 (and the previous work by Tiao, 1990) is that it provides a way to calculate the statistical signifcance of an estimated trend. This method can also be used to estimate the minimum length of a time series that is necessary to detect a trend of given magnitude. Unfortunately, the authors did not try to calculate either of these numbers for their own trends. The article would certainly benefit from this.

Indiviual remarks:

p. 8211, l. 9-11: "... In addition, since the global circulation is zonal in the free troposphere and stratosphere, ..."

That is certainly a simplification in the troposphere and depends on local topography as well as season. In the stratosphere it is a question of time scale. There the long-term behaviour is determined by the Brewer-Dobson circulation.

p. 8213, l. 18-29:

Mathematically, the inversion schemes of Rodgers and Tikhonov are equivalent. The difference is the determination of the regularization factor. Rodgers tries to provide this factor in an objective way by weighting statistical properties of the noise in the measurement versus the variance of the expected results (a priori information). This will fail if you don't have the correct statistical properties or if the noise distributions are not strictly Gaussian. If the results were strongly oscillating, this was most likely the case.

Tikhonov tries to avoid oscillations and produce a smooth result. This looks nicer but the strict error propagation of Rodgers is gone. The result is not necessarily better just because it is smoother. It is also straightforward to tune the Rodgers method for stronger regularization and smoother profiles.

p. 8214, l. 1-2: Table 2 does not actually contain retrieval parameters as claimed in the text.

p. 8215, l. 11-16: I don't understand how multiple regression can solve the problem of unevenly sampled data and gaps.

p. 8216, l. 4:

Which ECMWF data set did you use? Only the ERAxx data sets use a consistent model over a longer time series. The operational ECMWF model is changed and updated many times over the course of several years. This may well affect your results.

p. 8216, l. 22-27: it would be good to show a plot, table or other results to back up your own findings on the significance or non-significance of the atmospheric parameters.

p. 8218, Eq. 1: What is "l"?

p. 8219, Eq. 2: Are the indexed betas the same values as in Eq. 1?

p. 8219, l. 20-25: "The anomalies shown ..."

It is actually not clear where this is shown until you mention Fig. 2 in the following paragraph. Maybe you should put the reference to Fig. 2 to the beginning of the section.

I would still be interested to see how the median and range for the other parameters looked like. All the contributions you have chosen to be signifcant only contribute less than 2 percent each.

Why were polynomials of different degree chosen for the stations?

p. 8227, l. 19-20:

Again, the effect of tropopause height changes due to updates in the ECMWF model

should be invetsigated or at least mentioned.

p. 8235, Table 3:

I am not sure if the 2-3 year trends are really meaningful. Please check their significance of these short-term trends with the method provided by Weatherhead et al. 1998!

p. 8237, Table 5:

The Harestua station seems to be the least consistent one in Table 5. Do you have an explanation? However, you chose your atmospheric parameters from the data of this station. Is there reason to believe that this choice might have been different if you had looked at another station as a reference?

Figures 6 & 7:

Compared to the small number of plots in this article, these figures use a lot of space. I think for the reader the distribution plots of the residuals for each station would be enough. Please add your best estimate of a Gaussian distribution to the bar plots.

If you really want to show the full time series, you should plot all stations on one plot so one could recognize correlations in time (if there are any). For the

Minor remarks:

p. 8207: as far as I know K. Petersen does not work at the University of Bremen any more

p. 8211, l. 20: "... evenly sampled ..."

p. 8215, l. 7: "... normally distributed ..."

p. 8212, l. 8: "... have been performed ..."

p. 8213, l. 1: The sentence is too long. Please break like this: "... temperature information. For some species ..."

p. 8212, l. 18: "... were carried out ..."

p. 8214, l. 16: "For the here relevant level-2 data version 2.1." Broken sentence?

p. 8215, l. 7: "... normally distributed ..."

p. 8215, l. 5-7: shouldn’t this rather be "(1) ... (2) ... (3) ..."?

p. 8215, l. 15: "... trustful trends ..." I guess you mean "trustable"?

p. 8223, l. 2: "Harestua ..."

p. 8227, l. 2: "... highly influenced by local sources ..."

p. 8227, l. 6: "These might be two possible reasons ..."

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 8207, 2011.