**Atmospheric
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Interactive comment on "A new method to detect long term trends of methane (CH$_4$) and nitrous oxide (N$_2$O) total columns measured within the NDACC ground-based high resolution solar FTIR network" *by* J. Angelbratt et al.

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Reply to Referee # 2

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.

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Reply: In Table 5 the comparison with two other trend methods is shown. It can for example be seen that the methane trend for Zugspitze is 0.13 with the multiple approach while only 0.09 with the Bootstrap method, hence a 30% difference in trends. The wording "strikingly different" is hence incorrect.

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.

Reply: The biggest pitfalls when estimating trends is the noise and the autocorrelation not the non normal distribution, see both Weatherhead et al. 1998 and Tiao et al. 1990. The non normality can often be overcome with a data transformation, this is often done with ozone and uv-radiation measurements.

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.

Reply: In our opinion the method of Weatherhead et al. 1998 cannot be used on non equidistant data, which is the reason why we have not applied their formulas. This is also stated in the paper:" Methods have been developed to account for autocorrelation when estimating trends but they all need data which have equidistant time steps between the measurements ...".

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

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

Reply: In our opinion the circulation is zonal when averaged over time. We agree that shorter time scales are much more complicated.

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.

Reply: It has been shown, by other groups in the NDACC network (Sussmann et al. 2005 ACP), that the Tikhonov approach has advantages compared to the OEM; this result is what we have used in this paper.

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

Reply: We agree and have change to used micro windows instead

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

Reply: The method do not overcome the uneven sampled data but reduce the variability (noise) in the data to make the uncertainty of the estimated trends smaller. If the data were even sampled the uncertainty method described by among Weatherhead et al.

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1998 could be used instead.

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.

Reply: The tropopause altitudes from the model were in the Harestua case compared to balloon measurements at the nearby Gardemoen airport. The model and measurements showed in this case very similar 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.

Reply: A table like this will be added where the regression coefficients with uncertainties from the stepwise regression will be included.

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

Reply: We suppose "1" is the I in the summation of the anomalies (eq (1)). i to I indicate the number of a certain anomaly in the summation.

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

Reply: The betas are not the same as in Eq (1). Eq (2) is under section 4.4, deriving anomalies and explain how the anomalies are calculated.

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 significant only contribute less than 2 percent each.

Reply: A figure with the regression coefficients and their uncertainties is added in this section. Also references to the figures is relocated according to the reviewers

comment.

Why were polynomials of different degree chosen for the stations?

Reply: The order of polynomial are decided by the 1% criteria, this is described in section 4.4 and is related to the nature of the atmospheric parameter. Hence, the polynomial order is not chosen in a subjective way but with an objective calculating procedure.

p. 8227, l. 19-20: Again, the effect of tropopause height changes due to updates in the ECMWF model should be investigated or at least mentioned.

Reply: See answer above

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!

Reply: In the discussion related to Table 3 these "short term trends" are name growth rates instead of trends to highlight that it is atmospheric variability rather than trends that are shown. Since it has been shown (in many earlier publications) that methane can vary from different time periods it is of great interest to see if these features are captured also by the G-B solar FTIR techniques.

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?

Reply: Harestua has a very special location. In the winters it is located close to the polar vortex edge and is sometimes inside vs. outside. This makes the dynamic in the atmosphere special. When looking at Figure 2a and 2b one can see that the contribution from the anomalies to the trend model are similar for the different stations. This strengthens the choice of parameters in the regression model. If the contributions at

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different sites for a given parameter would have differed a lot this could have indicated not appropriate parameters.

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).

Reply:We do not understand the first comment regarding only the residuals in a plot. The best estimated Gaussian distribution will be added to all the histograms. The idea of plotting all stations in the same figure is in principal good but in this case there will be too much information, 4 measurements series + 4 models + linear trends + seasonal cycles.

For theMinor 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 ..."

Reply: All above are changed according to the reviewer

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