Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-356-AC2, 2016
© Author(s) 2016. CC-BY 3.0 License.

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Case Studies of the Impact of Orbital Sampling on Stratospheric Trend Detection and Derivation of Tropical Vertical Velocities: Solar Occultation versus Limb Emission Sounding" *by* L. F. Millán et al.

L. F. Millán et al.

lmillan@jpl.nasa.gov

Received and published: 13 June 2016

We thank the reviewer for his/her comments. Below are our responses in blue.

1 Reviewer 1 comments

General Comments:

This study investigates the impact of satellite instrument sampling patterns on the derived monthly mean, latitude resolved data set produced from such data, and extends previous studies by assessing the impact of sampling on derived quantities including trends and vertical velocities. This work is a valuable addition to the field, and should add to the understanding and appreciation of sampling on atmospheric studies. I find the subject matter therefore appropriate for ACP. The manuscript is generally well written, although some important details regarding the methods should be expanded upon(see below). I have a few comments which should be addressed before publication.

1. Are the trend and vertical velocity estimates based on solar occultation (SO) sampled fields really inaccurate, or rather are they simply more uncertain than those from the denser sampled fields? It is clear that the SO sampling adds noise to the timeseries. If this noise is relatively random, you might assume that the derived quantities like trends would be simply more uncertain. To say that the sampling leads to inaccurate derived quantities would require comparing not just the estimated trends and vertical velocities, but the error of those estimates. For example, it's not hard to imagine that the cloud of points in Fig 13 for the HALOE and ACE-FTS sampled fields might be consistent with the 1:1 line, which would be reflected by an estimated slope whose uncertainty interval contained 1. Therefore, to say that the SO sampling leads to biased trends and vertical velocities requires calculation of the errors in those trend and velocity estimates.
That is correct for noise relatively random, however the noise is determined by the sampling pattern of the geophysical parameter, that is why the trends and tropical velocities are different, which is what we are investigating in this study. If the noise due to the sampling was random, the trends and tropical velocities will be the same as in CMAM30-SD but noisier, as the reviewer point out, but they are not. Further, if something has big error bars that encompass the "1" does not imply that it cannot be biased, in fact, it can be bias and more uncertain, as it is pressumably the case for the

SO.

2. ACE-FTS samples the tropics in only 4 months of the year. It is therefore quite a stretch to compute full timeseries of monthly mean tropical upwelling from such sparse data, and I would be quite wary of any study that attempted this with real data. Very little detail is given on how the authors here fill in the missing months, and I am suspicious that the large error that they conclude results from the sparse sampling could be dominated by the interpolation used. A fairer method would be to compute the tropical upwelling only for months in which the SO instruments actually sample the full 8S-8N band. This would rule out the chance that the result here is produced by the interpolation. This might make the more complex analysis of Sec. 5 more difficult, or even impossible when applied to the ACE-FTS data. But, it could be argued that if the analysis doesn't work when applied to such sparse data, then it doesn't necessarily need to be proven that it doesn't work on highly interpolated data.
To compute the tropical upwelling the method correlate adjacent levels using a time lag of a year. Hence, an entire timeseries is needed. See page 8 line 27 to 31 of the original manuscript. The reviewer is correct to point out that for ACE-FTS data we are mainly doing the analysis in highly interpolated data. The following sentence will be added to emphasis this: For ACE-FTS gaps are filled in January, March, May, July, September, November and December, when no measurements are made over the tropics (8°S to 8°N), **as such, we are applying the analysis to highly interpolated data.**

Specific Comments:

p2, l24: accuracy, or precision? See general comment 1. We meant accuracy but this sentence will be deleted from the final manuscript due to a comment from referee 2.

p2, l34: the impact of sampling on the estimation of long-term trends ok

p3, l18: should this be actually O3 trends? It will be changed to: long term O_3 loss

p3, l20: spatial- horizontal ok

p4, l21: focus "on" ok

p5, eq 1: could be written and explained clearer. What is y? Is x the data (as written), or a placeholder variable for "r" and "s"? The text will be changed to: where N is the total number of points, y, belonging to a latitude bin l, and x is a placeholder variable for either the raw data, denoted by the superscript r, or the sampled data, denoted by the superscript s.

p5, l12: Should make clear here that 2005 refers to the model year, not the sampling pattern (which is assumed constant over all years). the sentence will be changed to: Figure 3 shows examples of the sampling biases for temperature, O3 and H2O for January 2005 CMAM30-SD data.

p5, l18 (and elsewhere): why capital T in temperature? it will be change for lowercase

p6, l3-4: the quantities shown in a Taylor diagram seem to compare the variability of two data sets, not really the trends in the data sets. I guess that two data sets could have the same trend, but perform poorly on these diagnostics. (I bring this up because the paragraph, and the section in general very much focuses on long-term trends, rather than the short-term variability that the correlation coefficient measures the agreement of.) That is correct, two datasets can have the same trend but perform poorly in a Taylor diagram due to either a lack of correlation or different standard deviations. Both cases will result in an increase in the number of years required to detect such a trend.

p6, l12: how are 60S-60N means calculated from the sparse SO fields if the full latitude range is not actually sampled in a month? The simplest method is taking the 60S-60N model mean, and subtracting it from the mean of whatever is available from the satellite sampled field. This creates large differences for sparsely sampled data. Another method, which is perhaps more fair to the sparser data, is to first take the difference between the model and sampled fields for each latitude, and then average the difference, which gives then the average difference over the latitudes where the satellite samples. Whatever method is used here should be clearly described, and the implications of the choice discussed.
The means were computed simply be averaging whatever was available. The difference were computed using the first method described, the second one will not be representative of the 60S-60N difference. We will add the following sentence: Means were computed averaging all the data available between 60N and 60S with no effort to use only latitudes where the satellites sampled. This was performed to show the representativeness of near global trends.

p7, l1: How is σ_ε^2 estimated? ${\sigma^2}_{varepsilon}$ is computed from σ_N using equation 8. It seems clear that the sampling affects the random error in the time series, is this accounted for in the fitting procedure? Not directly in the fitting procedure, σ_N is simply the standard deviation of the residuals between the data points and the trend model, hence, its magnitude is affected by the sampling artifacts. The sentence in page 7 line 21 of the original document will be changed to: (1) ϕ the autocorrelation of the residual between the data points and the trend model computed following Tiao et al. (1990)(2)

σ_N the standard deviation of the residual, which corresponds...

p7, l20: What values are used for ϕ, and how is it estimated? ϕ is estimated following the Tiao et al (1990) appendix A. We will add to the text (see comment above): computed following Tiao et al. (1990). We will also add: ϕ is computed for the raw as well as the satellite-sampled data.

p9, l27: it's not just the sampling that biases the trend, also important is the procedure of calculating 60S-60N means when the coverage is incomplete. A subtle point, but I think worth repeating. See response above.

Fig 13: The MLS-sampled scatter plot of Fig 13 looks surprising: to the naked eye, the points seem to lie along the 1:1 line for the smaller wTR values, and look to lie above the 1:1 line for larger wTR values (this also appears visible in the timeseries plot, where the MLS sampled field appears to give larger values at the yearly peak on wTR). However, the fit gives, surprisingly, a slope less than 1. In any case, a short description of the method used to produce the linear fits should be included, as there are a number of methods possible, which give different answers and depend on different sets of assumption (see e.g., Isobe et al., 1990). the reviewer is correct on his/her depiction of the figure. However, it is the density of points on the low side (for smaller wTR values) what is driving the slope. We will add the following sentence in the caption: Note that the MLS slope is driven by the vast number of points associated with small wTR values. We will also state that the linear fit is an ordinary least-squares regression.

References:

Isobe, T., Feigelson, E. D., Akritas, M. G. and Babu, G. J.: Linear regression in astronomy., Astrophys. J., 364, 104, doi:10.1086/169390, 1990.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-356, 2016.