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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.

**Anonymous Referee #1**

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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.

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 8°S-8°N 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.

Specific Comments:

p2, l24: accuracy, or precision? See general comment 1.

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

p3, l18: should this be actually O3 trends?

p3, l20: spatial->horizontal

p4, l21: focus “on”

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”?

p5, l12: Should make clear here that 2005 refers to the model year, not the sampling pattern (which is assumed constant over all years).

p5, l18 (and elsewhere): why capital T in temperature?

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.)

p6, l12: how are 60°S-60°N 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 60°S-60°N 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.

p7, l1: How is $\sigma_\epsilon^2$ estimated? It seems clear that the sampling affects the random error



in the time series, is this accounted for in the fitting procedure?

p7, l20: What values are used for $\phi$, and how is it estimated?

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.

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 $w_{TR}$ values, and look to lie above the 1:1 line for larger $w_{TR}$ values (this also appears visible in the timeseries plot, where the MLS sampled field appears to give larger values at the yearly peak on $w_{TR}$). 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).

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.

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