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Atmospheric Measurement Techniques Discussions

Interactive comment on "Model estimations of geophysical variability between satellite measurements of ozone profiles" *by* Patrick E. Sheese et al.

Anonymous Referee #2

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The paper discusses the important topic, influence of geophysical variability in data validation. To estimate this influence, the data from three chemistry-transport models have been used. Ozone profile data from two satellite instruments, ACE-FTS and OSIRIS, are considered in the paper. The interesting analyses are presented in the paper. However, I have several concerns on the methods and analyses. My comments are below.

MAIN COMMENTS

1) The authors characterize the natural variability of the ozone field using the data from 3 CCM/CTM models having rather low spatial resolution, from 1.9 deg to 3.75deg.

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This implies that the lowest spatial scale that can be probed with these models is ∼200-300 km. The smaller-scale variability is not resolved by the models and cannot be characterized. This should be at least stated clearly in the paper (in particular, p.2 line 29, “large scale” should be quantified). However, the optimal way would be the inclusion of simulations with a high-resolution CTM.

2) The characterization of ozone variability assessed using the model data is too simplified – both in general and for the considered application of satellite data validation. First, the variability ozone variability depends on latitude and season (in addition to diurnal variability). Second, the variability is not isotropic in latitude-longitude direction therefore a simple characterization by “separation distance” is too superficial. Using the model data, the spatio-temporal variability of ozone field can be characterized in more detail, and thus the collocation criteria can be used in more advanced way (see also below).

3) The idea of collocation criterion using the information about the natural variability obtained from modelling is good. It should be described in more detail how technically the collocation criterion “variability <10%” is applied. Do I understand correctly that you use time-space collocation criteria as shown by circles in Figure 4, i.e., these are globally for each altitude level?

My main concern is that 10 % threshold is not actually optimized. Why have you selected 10% as a threshold? It seems to be significantly larger than uncertainties of each satellite dataset, thus the objective stated in the introduction, “Collocated measurements should be close to each other relative to the spatiotemporal scale on which the variability of the geophysical field becomes comparable to the measurement uncertainties” is not satisfied. On other hand, in the tropical middle stratosphere, for example, the overall variability is ∼5%, thus the criterion <10% variability will be satisfied automatically for any collocation criteria.

I think that the maximization of number of collocations within a variability window is not

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the best approach, since selecting a broader spatio-temporal window increase both number of collocations and natural variability. Instead, reduction (or minimization) of uncertainty of the bias estimates (which depends on measurements uncertainties, natural variability, and number of collocations) would be a more concrete objective, and the advantages of "optimized" criteria can be quantified (for example, reduction of bias uncertainty from x% to y%).

Since the ozone variability strongly depends on location/season, it is expected that the optimized collocation criteria will also depend on location and season, or, at least, characterized into "low" and "high" ozone variability. At the same time, this would be reduce the drawback that you mentioned in the paper on page 7: " One drawback to having different coincidence criteria at each altitude is that it can potentially add biases between altitudes due to changing seasonal and latitudinal sampling".

DETAILED COMMENTS

1) P.2 , l. 29: please quantify "large scale" term

2) Section 2. Please add estimates of random uncertainties of ACE-FTS and OSIRIS ozone profiles.

3) Section 2.2. Why don't you use version 5.10, which, as you explained in the paper, is better than v.5.07?

4) P.5, lines 17-19: You use rather relaxed collocation criteria (12 h and 2000 km); what is the difference in pressure profiles for large separations and how this affects transformation of OSIRIS data to pressure grid? Is the pressure-altitude conversion using reanalysis data at OSIRIS locations less accurate?

5) Why do you define sigma_nat as 2* std (MOD_ace-MOD_OSIRIS)? One would expect sigma_nat= std (MOD_ace-MOD_OSIRIS)/sqrt(2).

6) I suggest revision of Section 4, according to the MAIN COMMENTS 2 and 3. The spatio-temporal ozone variability (time, altitude, latitude, longitude, season) can be

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in detail characterized using the model data. For optimization based on variability, I suggest a categorization at least of “low” and “high” ozone variability (alternatively, according to latitude zone and season). I suggest also quantitative estimates of validation improvement (for example, reduction of uncertainties of bias estimate, bias detectability, quality of the spread estimate) based on the optimized collocation criteria.

7) Section 4.2: The ozone variability in polar regions depends strongly on season. This should be taken into account in the analyses.

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