

Interactive comment on “Evaluation of SHADOZ sondes, HALOE and SAGE II ozone profiles at the tropics from SAOZ UV-Vis remote measurements onboard long duration balloons” by F. Borchi et al.

Anonymous Referee #1

Received and published: 29 November 2004

This paper attempts to tackle the difficult task of making meaningful comparisons between measurement systems at the 1 to 2% level. Measurement systems have improved in accuracy and precision over the years and may now be beyond our ability to compare them and, more importantly, assess the reasonableness of the cited error estimates.

I found that the text could use some careful editing to aid in a proper translation to English, however, I did not find any instance where the meaning was hopelessly obscured. I will not list the errors in wording I have found in this review, but the final version should

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be carefully edited. I tried the URL link to SAGE II <http://www.sage2.larc.nasa.gov> and it did not work. Some searching yielded <http://www-sage2.larc.nasa.gov> as a working alternative.

I am a little concerned that the sampling from the satellite instruments may occur in a broader latitude band than was sampled by SAOZ. This could bias the estimation of precision as well as accuracy (bias).

Related to this is whether the author's method of separating the geophysical variations from the instrumental can and should be applied to the HALOE and SAGE II data? It is not clear from the text that it has. This is actually an important component of this assessment and a valuable technique for future work. The text needs to be clear about what exactly has been done to the various datasets as part of the comparison process.

It would be helpful to have the maps in Figure 2 include the locations of the SAOZ, HALOE, & SAGE II measurement locations.

The authors should assess the magnitude of the bias that results from using the Brion cross sections relative to the Shettle and Anderson results used by SAGE II? Does this account for any of the bias estimate?

I think it appears a presumptuous to state in the caption to Figure 11 that the SAGE II altitudes have been "corrected" since the discussion indicates that it is not statistically significant. Why not clearly state that they were adjusted by 250 m as per the text.

As noted by the other reviewer, I did not like the conclusions presented at the end of each section. I do not think the reader will have difficulty remembering what was presented and that the conclusions should probably be presented at the end of the paper.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 4945, 2004.

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