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Comment

***Interactive comment on* “On the use of the correction factor with Japanese ozonesonde data” by G. A. Morris et al.**

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Morris et al. assess the quality of the Japanese ozonesonde measurements since 1990. These measurements clearly have the potential to provide important information about the atmospheric ozone changes in Japan. Since there are few, if any, long-term in East Asia, they have great inherent value from a regional as well as a Japanese perspective. However the measurements have suffered because the technique used (carbon iodine) has not been used globally, so that there is a smaller user base and its strengths and weaknesses are less well characterised. This lack of familiarity with the instrument performance coupled with the lack of nearby ozone profile measurements has meant that meaningful comparisons of ozone trends and changes over other northern mid-latitude regions have been hard to make – whenever a difference is seen, the

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question always arises as to whether it is real or an instrumental artefact. The need for a thorough assessment is thus clear.

Morris et al. address these issues in a careful and thorough way. It is complex as there are many facets to bring together (satellite overpass comparisons, separation of tropospheric and stratospheric issues, use of the JOSIE laboratory studies, comparison with nearby in situ sensors). For the most part this is done clearly and thoroughly, and the conclusions are well founded. I have no major scientific criticisms, though I do make some suggestions as to how the paper could be made clearer (below). The paper should be published once the authors have considered these suggestions.

Comments:

1. It would help the non-specialist reader if the possible causes of correction factors and why they might change over time. A fairly general paragraph could be included possibly in the introduction which discusses the stability (or otherwise) of sonde manufacture, pre-launch procedures, radiosonde type, etc., as well as why there is good reason to consider the troposphere and stratospheric components differently – what is the effect of oppositely signed trends, for example?

2. It would help if the authors could be clearer when they say more precisely what they mean when they say when a correction factor is applied, as they discuss several ways in which a correction could be made. I think that they mean the WMO method described early in the paper. Perhaps the term ‘standard CF’ might be used anywhere there is any ambiguity. The particular example that brought this to mind is the paragraph starting on (15612, 16), as I do not understand why the use of a standardly applied CF using the local Dobson instruments would lead to worse agreement with the satellite overpasses when the Dobson/overpass agreement is itself good.

3. Para ending (15605, 5). This is an odd result which is at first glance incompatible with the other comparisons. As such, I think it should be at least mentioned in the conclusions (along with any other ‘further work’ issues).

4. Figure 4. (a) An obvious feature here is the much greater variability in CF in the early 1990s, possibly following on from the Pinatubo eruption. From this figure, it is hard to see if the shape of distribution has changed or if the variability is simply larger. If the former, it could be relevant to any discussion of instrumental affects of the aerosol as the influence may be dominated by a small number of soundings. (b) Is there any evidence for annual cycles in the CF? This might be expected since the stratospheric and tropospheric fractions vary as well as the total column.

5. (15598, last line on) Can this last point be clarified? Do the authors mean that new trend studies need to be done using the revised data?

6. (15603, 12) ‘...mixing ratio assumption..’

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 15597, 2012.

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