Response to review from N.R.P. Harris

Reviewer comments in blue.

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?

We have added a rather extensive paragraph near the end of Section 1 that should address most of Dr. Harris' suggestions above. That addition should better motivate the non-specialist reader's interest in the paper.

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.

We apologize for any confusion regarding the CFs. In this paper, only two CFs appear: 1) those listed in the header of the ozonesonde data files, which appear to have been calculated with the WMO method, described in the second paragraph of our Section 2, and that were applied to profiles whose total ozone columns were computed by using the constant mixing ratio assumption for the above burst column amounts; and 2) those applied to profiles whose columns were computed using the McPeters et al. balloon-burst climatology. For clarity, when we mean the former, we use "CF" and when we use the latter, we use "CF_{BB}," as we note now in the revised text.

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).

This result is certainly interesting and unexpected, given the JOSIE findings. It should be noted, however, that the difference in performance between the KC-79 and KC-96 sondes during JOSIE, at least as measured by mean CFs, was not statistically significant. We have added a comment on these findings in the conclusions as Dr. Harris recommended.

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.

Dr. Harris is correct to note that the variability (as indicated by the scatter and the standard deviations of the CFs from the Japanese stations) increased after the eruption of Mt. Pinatubo. Table R1.1 below shows the mean and one standard deviation CFs (in parentheses) for the four Japanese sounding stations in the three years prior to the eruption of Mt. Pinatubo, from 6 months after to 3.5 years after its eruption, and from 3.5 years to 6.5 years after its eruption. Indeed, the standard deviation values increase by 10 – 80% during the Pinatubo period as compared with the three years prior, although we should note that the number of soundings during those three prior years was severely limited. Nevertheless, the standard deviations decrease by 5 – 40% in the period labeled "4 – 6 Years" Post Pinatubo. The CFs drop by large amounts moving from the 3 years after Pinatubo to the 4 – 6 year after Pinatubo periods. The physical mechanism(s) that might explain such changes, however, continues to elude me.

	3 Years Prior to Pinatubo	3 Years Post Pinatubo	4 - 6 Years Post Pinatubo
	6/88 - 5/91	1/92 - 12/94	1/95 - 12/97
Naha	0.939 (0.068)	0.973 (0.123)	0.899 (0.108)
Kagoshima	1.000 (0.125)	1.020 (0.139)	0.908 (0.085)
Tsukuba	0.957 (0.085)	0.978 (0.101)	0.882 (0.095)
Sapporo	0.932 (0.059)	0.986 (0.094)	0.937 (0.080)

Table R1.1. Summaries of the mean and one standard deviation correction factors at the 4 Japanese sounding stations before and after the eruption of Mt. Pinatubo.

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.

To investigate this interesting point, we examined histograms of the Japanese sonde station data during the same three periods illustrated in Table R1.1, but aggregated rather than separated by station. The data in Figure R1.1 suggest that while the mode and the tail on the lower end of the distribution did not change much from the Pre-Pinatubo to the 3 Years Post Pinatubo (labeled "Pinatubo" in the figure), the high-end tail is more populated in the three years post Pinatubo. The more dramatic change is seen by the leftward shift in the 4- 6 years after Pinatubo (labeled "Post-Pinatubo" in the figure). The tail on the lower end of the distribution is much sharper than either of the other two distributions, the mode shifted down by 0.075, and the high-end tail very similar to that seen in the 3 years after Pinatubo. The number of soundings in the three years prior to Pinatubo was somewhat limited (187) compared with the two post-Pinatubo groups (486 and 544 respectively). Most importantly, for the distribution 4 – 6 years after Pinatubo, the lower mean CF cannot be attributed to the influence of a small number of soundings. Rather, as noted above, the low end of the distribution is sharper and less populated than the high end of the distribution.

(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.

Thanks for another interesting comment. I have looked carefully at the CFs as a function of season. Table R.1.2 shows the aggregated results over the four stations separated into the same three periods of time as in Table R1.1 with the final column covering all years of all Japanese sonde data. There does not seem to be any regular, statistically significant differences between the correction factors in the various seasons.

	3 Years Prior to Pinatubo 6/88 - 5/91	3 Years Post Pinatubo 1/92 - 12/94	4 - 6 Years Post Pinatubo 1/95 - 12/97	All Years
winter	0.977 (0.098)	1.006 (0.110)	0.898 (0.085)	0.928 (0.101)
spring	0.975 (0.071)	0.967 (0.101)	0.921 (0.089)	0.941 (0.103)
summer	0.992 (0.100)	1.022 (0.139)	0.940 (0.120)	0.966 (0.123)
fall	0.959 (0.108)	0.984 (0.118)	0.904 (0.114)	0.948 (0.111)

Table R.1.2. Mean and one standard deviation (in parenthesis) of the correction factors from the four Japanese sounding stations.

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?

Yes, we do think it necessary for all authors of previous studies who included the Japanese sonde data in their trend analysis to revisit their results. In fact, one reader of our paper has suggested we perform that work in this paper, but the scope of such an enterprise is well beyond what could be accomplished here. Rather, we prefer to alert the community through the publication of this manuscript and let the various groups involved in trend analyses pursue the various problems independently. I think that the present statement is clear and expresses our recommendation well, although I am open to suggestions from the editor.

6. (15603, 12) '. . .mixing ratio assumption..'

Thank you for correcting that typo. We have fixed the error!

Figures

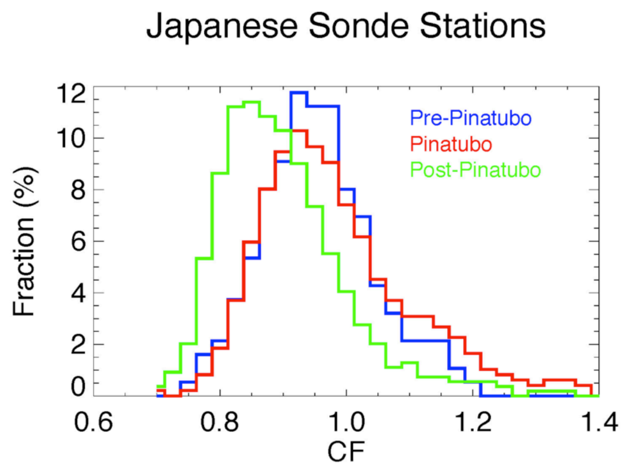

Figure R1.1. Histograms of correction factors for the four Japanese sounding stations in three periods: “Pre-Pinatubo” = the 3 years prior to Pinatubo, “Pinatubo” = Jan. 1992 – Dec. 1994; and “Post-Pinatubo” = Jan. 1995 – Dec. 1997.