**Atmospheric
Chemistry
and Physics
Discussions**

Interactive
Comment

Interactive comment on " **Coherence of long-term stratospheric ozone vertical distribution time series used for the study of ozone recovery at a northern mid-latitude station"** *by* **P. J. Nair et al.**

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This paper is a careful study comparing ozone profile measurements made at OHP with one another and with satellite measurements, over a period from 1994-2007. The results are consistent with those of previous studies; indeed, so consistent are they that there does not seem to be any new result in this paper. To proceed to publication in ACP the authors need to convince me that there are new scientific results here, and to change the abstract and conclusions accordingly.

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Interactive Discussion

Discussion Paper

I have a number of minor comments:

p.1 col 2 l.12 . . .measurements – (SPARC. . ..)

p.2 sec 2.1.1 l 4 . . ...of the OHP lidar. . . . ..

p.2 sec 2.1.1 l.9 give reference for signal processing algorithm

p.3 col 1 para 3 l.1 ....accuracy of the lidar. . . ..

p.3 col 1 2.1.2. l.2 ($\sim$0.2km) than other measurements

p.3 col 1 l.-3 . . ..in use. The acquisition. . .. . .

p.3 col 1 l.-2 . . .ECC sondes were coupled with Vaisala. . ..(the Vaisala company does not use the ä)

p.3 col 2 l.2 . . .radiosondes have been coupled to ECC. . . ..

p.3 sec 2.1.2 last para A better explanation is needed of the correction factor. This is the ratio of the total ozone column measured by the sonde (with an estimate of the residual above balloon-burst) to the Dobson. The last sentence should refer on to where this is discussed further.

p.3 sect 2.1.3 l.-4 the UMK04 algorithm

p.4 l.2 . . .on the NASA ( ) NIMBUS-7. . .. . ..

p.4 sec 2.2.4 l.7 manoeuvre

p.4 sec 2.2.4 l.-2 ..MLS version 5 from 1991-1999. . ...from 2004-2009

p.5 sec 3.1 l.7 . . .eruption (Guirlet et al, 2000)

p.5 sec 3.1 para 1 last sentence Here you say that Prinatubo had a big effect on SAGE-2, but that is not what fig.1 shows.

p.5 sec 3.1 para 2 l.-1 . . .ozone are excluded from the analysis for all. . .. .

p.5 sec 3.1 para 3 What do the phrases "when the sampling of the ozone field by the satellite instrument make it possible" and "for both ground-based and space-borne measurements" mean? Both phrases appear entirely redundant.

p.5 col 2 para 2 l.4 . . .around the OHP station. . ..

p.5 col.2 para 2 l.13 . . .onwards; the lidar provided. . ..

p.5 col 2 para2 l.15 . . .Umkehr provided more profiles at the beginning of the observation period. . .

p.5 col 2 para 2 l.20 . . .HALOE provided. . ..

p.5 col 2 para 2 l.-4 . . .number of profiles throughout the period. . ..

p.6 l.1 A great show is made of lidar being measured as a function of height, yet here you introduce lidar pressure levels. What are these?

p.6 col 1 l.3 . . .obtain the ozone profile. . .

p.6 col 1 l.8 (Griesfeller et al, 2010); this did not make a significant difference to the annual average

p.6 col 1 para 1 l.-1 AK smoothing

p.6 col 1 para 2 l.2 ......GOMOS) is similar. . ..

p.6 col 1 para 2 l.6 . . .for comparison, HALOE ozone values measured. . ..

p.6 col 1 para 2 l.10 ....using the corresponding MLS. . ..

p.6 col 1 para 2 l.14 The difference between geopotential and geometric height is 1km at 80km; this is not "very small"

p.6 col 1 para 2 last sentence What does this mean? What are "boundary altitudes"?

p.6 col 1 para 3 l.2 ..performed by convolving. . .

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p.6 col 1 para 3 l.6 . . .ozone AKs, and the. . ..

p.6 col 1 equation 1 Why do you use an a priori profile for lidar? Nowhere else is this mentioned

p.6 col 2 para 2 l.6 from 1985-2007

p.6 col 2 para 2 l.9/fig 2 I do not understand fig. 2. Clear differences between the brown and blue lines are evident at 23.9mb and the point below, but this is not shown in the third panel. Instead, the largest difference is shown in the points above 23.9mb. The text says the results do not differ significantly except at 2.51 and 1.58mb, yet the error bars at ~16mb are too small to plot and those at 2.51mb large enough to overlap. What is going on here? Also, what are the numbers on the third panel?

p.6 col 2 para 2 l.10 I can see why a smaller number of events would give a larger standard error, but not why it would give a statistically significant difference.

p.6 col 2 para 2 l.12 omit comma after levels

p.7 col 1 para 2 The time series of the discrepancy with ozonesondes merits some discussion

p.7 col 1 para 3 l.4 . . .HALOE provided fewer collocations. . ..

p.7 col 1 para 4 l.4 omit "Fairly. . .1994"

p.7 col 1 last para There is no sudden jump in the data at 6.3-4hPa and 4-2.51 hPa, and even at 15.8-10 hPa it is more of a systematic increase

p.7 col 2 l.1 . . .the increase in 2001 found at. . .. . ..profiles were compared. . ..were interpolated to SBUV(/2) pressure levels, and SAGE-II ozone number density profiles analysed as discussed previously. Relative differences were determined.

p.7 col 2 para 2 l.1 results were smoothed. . ..

p.7 col 2 sec 4.1.2 l.7 Aura MLS produced. . ..

p.7 col 2 last line 20-40km and somewhat higher differences outside this range

p.8 col 1 sec 4.2.1 Line 4 and all subsequent occurrences – use "compared" not "as compared"

Figs. 7 and 9 Add a pressure scale as well as altitude since much of the discussion uses pressure, (eg. p.9 col 2)

p.9 col 1 sect 4.3 l.2 were computed

p.9 col 2 para 4 l.2 ozonesonde – lidar comparison

p.9 col 2 para 4 l.5 drifts were also

p.9 col 2 para 4 l.8 slopes were computed. . .were less than. . .were more negative. . .

p.10 sec 5 l.10 ....40 km, because of the lower. . .

p.10 sec 5 l.20 How can a constant bias result from a change of sonde type?

p.10 sec 5 l.25 GOMOS were also

p.10 sec 5 l.28 relatively large. . .MLS shows good agreement

p.10 col 2 l.1 regressions were computed

p.10 col 2 l.6 2009), and are. . ../yr was calculated for SAGE. . ..

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 28519, 2010.

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