

Interactive comment on “Tropospheric ozone climatology at two southern subtropical sites, (Reunion Island and Irene, South Africa) from ozone sondes, LIDAR, aircraft and in situ measurements” by et al.

Anonymous Referee #1

Received and published: 29 July 2008

This paper provides an overview of data from multiple sources at two sites: Reunion and Irene. The authors examine the degree to which the data sets are consistent as a function of altitude, then examine trends in the tropospheric columns based on an extended profile record.

While the paper has some compelling data, it seems still in need of some additional investigation and some reworking of the text. Overall, the paper seemed to have three pieces that did not fit well together, at least in the current form.

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The first piece compared LIDAR, MOZAIC, and ozone profiles from the two sites, showing generally consistent results. The authors do a reasonable job explaining the differences at high altitudes, but are unable to make any comment on the most interesting difference between the LIDAR and ozonesonde data in January near 10 km. Some comment on the large difference seen there would be worthwhile.

The second part other the paper seems pulled from a proposal to justify an observatory on Mado Peak as representative of the lower tropospheric ozone seen in the soundings. While the comparison and explanation appear reasonable, the lack of error bars on the most important plot (Figure 5) make the differences difficult to interpret statistically. Furthermore, this section of the paper seems to come between two sections that COULD fit together more neatly. I would recommend removing this section entirely, as it does not seem to fit in with the rest of the paper very well at all.

The third part of the paper deals with trend calculations. The linear trend model employed is simple, but with the limited data sets available, may be reasonable. That said, I think the author's investigation of layer trends to explain tropospheric trends should be expanded into their seasonal trend analysis of Figures 9 and 10. A further investigation of tropopause height trends is also merited, as the height of the tropopause will influence column values, especially in the author's upper tropospheric layer.

Throughout the paper, the English could be improved. I will provide at the end of this review a list of paragraphs/lines at which I found typos or grammatical difficulties. I understand from the editor that he was addressing most of these to allow this reviewer to focus on the content of the paper, which I have done.

Below, therefore, please find a list of specific comments on the paper content followed by my listing of problem areas in the paper.

If revised appropriately, I do believe this paper is worth publishing. However, in its current form, I am not ready to recommend its publication. I would very much like to see a revised version of this paper with a clearer, more streamlined presentation as

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well as with some of the additional investigation that I recommend below completed.

Content comments

format is page , line(s) , comment

11065, 25-26, hopefully the conclusion on industrial pollution and biomass burning contributions are supported by emissions inventories.

11066, 12-15, references for the tropical STE and biomass burning seem needed here.

11067, 8-16, have you seen the work of Thompson et al.,? I think this work may be relevant to your study.

<http://www.atmos-chem-phys-discuss.net/8/5979/2008/acpd-8-5979-2008.pdf>

Thompson et al. (2007), J. Geophys. Res., 112, doi:10.1029/2006JD007441.

Thompson et al. (2007), J. Geophys. Res., 112, doi:10.1029/2006JD007670.

11067, 19; add reference for MOZAIC and for the LIDAR data you will use.

11069, 7-9, what was the result of the climatology of Randriambelo and how does that result inform your study?

11069, 9-13, I think this paragraph fits in better with the dynamical context than the regional sources.

11070, 7, /'and eastern (25 E)'/, but east of what?

11071, 18-20, you might want to site and review the work of Smit et al. (2007), J. Geophys. Res., 112, doi:10.1029/2006JD007308 for the performance of ECC sondes.

11093, Fig.2, why not just show the total number of profiles rather than the mean number each month?

11072, 25-26, I am a bit confused by the total number of profiles ranging between 20 and 25 per month. Based on figure 2, I am guessing that you mean total number from

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all sources, but I was confused by the previous paragraph into thinking that you were still discussing the SHADOZ soundings. Perhaps just some reorganization will help clarify your point here.

11073, 2-4, I think this could be reworded to be clearer. But perhaps it is obvious to the reader that campaign timing is important in determining the number of soundings and does not require explanation from the authors here.

11074, 13, ozonesondes cannot be launched in any weather. Strong convection, strong winds, driving rains 8211; these conditions prevent (or should prevent) ozonesonde launches.

11074, 23, please specify what you mean by /'whole database.'/

11094, Fig. 3, please remake the figure differentiating the standard deviation curves associated with radiosonde data from LIDAR data. Also, perhaps just using data points to show the number of lidar profiles at each height would make sense, rather than coloring part of the graph gray. You could use a third color for this data set and match it to the color of the x-axis labels and titles at the top of the plots. I believe your analysis in the text of the differences between the LIDAR and sounding ozone profiles is on target. Based on this figure, there seems to be little statistically significant difference between the two data sets at any other level, although the offset seems largest in DJF between 6 and 10 km, with the LIDAR high.

11075, 1, no need to defend the choice of 130 ppb.

11075, 7-9, I think the text here really needs to be clarified. The wording is confusing to me. Can you be more specific regarding how the peak varies according to the site?

11075, 9-12, I am not sure that I can follow your comparison. While you comment on the >90ppb values seen above 10 km in the SHADOZ data from Irene, the MOZAIC data from Johannesburg are not reported much above 10 km, so it is not possible to compare these two. Below 10 km, the two are comparable, with MOZAIC maybe being

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a bit lower than SHADOZ.

11075, 13-15, I think you have explained why the LIDAR is low biased above 10 km (in general). In Fig. 4, the curious feature (unexplained) is why the LIDAR is higher than SHADOZ between 10 and 12 km in January. Some comment on this difference would be interesting.

11075, 20-25, This section essentially repeats what you said earlier on page 11069. Maybe you could just reference the earlier discussion and cut this section out here?

11076, 8-11, Would your conclusion be supported by trajectory studies? Is that worth investigating in the context of this paper?

11076, 24-26, Could you use the meteorological analyses to determine how representative the launch days were? That could help explain differences you're seeing.

11096, Fig 5, can you please add error bars to this plot to aid with the comparison? It is difficult to recognize whether the differences in the figure are statistically significant without error bars.

11077, 25-27, Is this conclusion yours or that of Bremaud and Taupin (1998b)? If the former, I think you need to provide more evidence to support your conclusion, which is entirely believable, just not proven in your paper.

11080, When you examine the seasonal column trends for Irene and Reunion (Figs. 9 and 10), would it not be worthwhile to separate them into layers again? Although your layer analysis showed the relative importance of strat/trop exchange and biomass burning overall for each site, I am not sure it is obvious that those trends hold up in any given season. Perhaps you will find different weightings in different seasons. Also, although you have limited your columns to below 16 km, variations in tropopause height will certainly impact your upper tropospheric columns. If the tropopause height is descending with time, then you will see a positive upper tropospheric column trend. I think it would be worthwhile for you to look into this factor as well.

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reword, confused English, typo list:

11065, 6-12

11065, 16

11066, 21, 22-23

11067, 4-7

11069, 5

11071, 4-5, 6-7, 16-17

11072, 15, 20-22

11073, 13-14

11074, 3-4, 10-12, 15-21

11075, 5-7

11077, 6, add a paragraph break before discussion of Fig. 6.

11078, 7, 19, 21

11096, Figure 5, caption, /'night-time/'

11097, Fig. 6, caption typos

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 11063, 2008.

ACPD

8, S5353–S5358, 2008

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