**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Impact of sampling frequency in the analysis of tropospheric ozone observations" *by* M. Saunois et al.

Anonymous Referee #2

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Review of "Impact of sampling frequency in the analysis of tropospheric ozone observations" by Saunois et al. This paper examines the impact of measurement sampling frequency on estimates of the intra-seasonal variability and trends in tropospheric ozone. It shows different sampling frequencies are needed to resolve ozone variaibillity at different heights depending on the intrinsic variability of ozone. It is an interesting study and could potentially be of great value in interpreting tropospheric measurements of ozone. However, I feel that the analysis is flawed in a number of aspects. My recommendation is that the paper should not be published until these flaws are addressed.

Major Comments:

(1) The analysis does not consider the autocorrelation timescale. This timescale will likely depend on level and time of year. It is likely that weekly ozonesondes produce

four statistically independent profiles every month. However, I am rather doubtful that the sampling frequency of 4/month prescribed in this paper also produces independent profiles. As a concrete example, if the autocorrelation timescale is on the order of a week then weekly ozonesonde sampling should produce a reasonable result; the sampling prescribed in this paper will not. Jennifer Logan also brought up this point. This has to be addressed before I can recommend publication. The autocorrelation timescale should be evaluated as a guide as how to sample the MOZAIC data consistently with the ozonesonde sampling. Implementing the above change in the prescribed sampling will likely considerably reduce the number of samples taken from the MOZAIC measurements and may require significantly different analysis throughout the paper.

(2) While I am not a statistician, bias in the MOZAIC measurements should also be considered in the analysis. This is particularly true when sampling MOZAIC measurements at less frequented sites. It seems that a more general, valid and interesting approach would be to construct a theoretical distribution of ozone measurements (most likely log-normal) consistent with the MOZAIC measurements at each level and season. Then the question becomes: how many monthly samples (with a given autocorrelation timescale) does one have to take from this distribution to get within a certain accuracy of the distribution mean OR given a number of monthly samples and a given autocorrelation timescale, how closely can one approximate the mean value. Please justify why this approach is not taken.

(3) I would agree with the first reviewer that the writing could be improved. The 1st reviewer gives some excellent suggestions for clarification of the terminology. However, I found it was also difficult to follow what the authors were trying to do and where they were going. For example, the analysis methodology is never clearly stated: the paper jumps into "Subsampling methodology", but never gives an overall outlook as to how this methodology will be utilized in analyzing the measurements. Another example is the fact that the authors really never explain that they are interested in the impact of

sampling methodology on ozone trends until section 3.5. In summary, the text is not really "reader-friendly". It would be helpful if the authors could step back and explain their overall analysis methodology and questions addressed.

(4) The results presented in this paper are particularly useful as a means to evaluate the significance of measured signals. They less helpful as a means to evaluate models. Ideally modeled ozone profiles should be instantaneously compared against the measured profiles. When comparing instantaneous profiles it does not matter if one really compares in the upper/middle or lower troposphere. However, a significant result that might be taken away from this paper (I write "might" because I have doubts about the analysis methodology – see points (1) and (2) above) is that it is not sufficient to evaluate monthly-modeled profiles against monthly measurements. Or to rephrase: The model output data need to be put on a format comparable with the measurement data (this is the converse of the statement made by the authors on 27110, lines 1-4).

(5) Throughout this paper the authors ascribe physical reasons for measured trends and variability (e.g., biomass burning plumes, stratospheric intrusions, changes in emissions) (e.g., see 27115, l. 14; 27116, l. 17; 27112, l. 16; 27124 l. 3; and other locations). While the author's may be correct in these ascriptions they often give rather vague reasons for their conjectures, suggesting they have not done the analysis necessary to back these conjectures up. Please give references justifying these claims, describe in more detail the model analysis supporting these conjectures or word the statements so it is clear that they are indeed conjectures.

(6) Nowhere is there a justification in the paper for comparing the specified ozonesonde stations and surface stations with MOZAIC. This comparison is only valid if the same airmass is sampled. While this may be true for the ozonesonde measurements (although it remains to be shown), it seems less likely to be true at the surface sites as these are governed by more local conditions.

Minor Comments. After the authors address the comments above I may have additional

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minor comments in the future. However, below are some minor comments that struck me as I read through the paper.

(1) 27108, l. 10: "uncertainty" – uncertainty in what? The monthly mean?

(2) 27109, l. 20-21: "limited" – please explain what you mean by limited.

(3) 27112, $\sim$ l. 5. Use of correction factor. Why was the correction factor not used to sort out some of the questionable sonde profiles? This has been the standard procedure following analysis by Jennifer Logan. The authors state: "the applied correction factor . . ..is small for most of the stations considered", implying that for some stations this factor is not small. Please justify the analysis procedure used.

(4) The sonde data and MOZAIC data is used at the surface. Please discuss some of the problems with this: (i) the interference of SO2 with the measurements; (ii) the fact that low altitude MOZAIC measurements are likely to be biased due to the vicinity of airports and thus may not representative of a larger area.

(5) It is not clear to me why the data is limited to morning MOZAIC profiles. Certainly the surface measurements better characterize surface ozone variability than MOZAIC. Why aren't the surface measurements used to characterize surface variability? Wouldn't this allow all MOZAIC profiles to be used?

(6) 27119, l. 19-20: Another interpretation is that neither data set gives a statistically significant trend (except for the winter MOZAIC measurements). I'm not it is very meaningful to claim two datasets agree with each other if neither dataset indicates a significant result.

(7) 27120, l. 2, "It is worth noting. . .." I'm not sure what the authors are trying to say here.

(8) 27122, l. 8-9. Please justify this statement.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 27107, 2011.

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