Please note the following format:
- Reviewer's comments are in *italics*, followed by a “>” denoting the authors' reply.
- Paragraph numbers, Para(n), in the responses refer to the revised manuscript.

The authors present a new version of the well-known NOAA-1 ozone instrument described by Proffitt and McLaughlin (1983). This new version NOAA-2 differs from the older one in many features and properties. I am basically happy with the actual manuscript, but have some minor concerns which I would like to see considered before publication in AMT.

Minor concerns
Abstract: I don't understand why you give the specification “polarized” in line 7/8. That the light gets polarized by the beam splitters has no implication, or? I guess, you would like to say that the light path is folded, which is much more important here.

> Agreed. The sentence has been changed to: “The design and use of a new, dual-beam, UV photometer instrument for in situ O_3 measurements is described. An optical-isolator type configuration is utilized to fold the UV beam inside the absorption cells, giving an effective 60-cm absorption length with a 30-cm cell.”

The instrument precision should be given in more detail. Most/many optical instruments are limited by shot noise which is usually given as $\Delta O3 \sim f^{0.5}$*, with f the measurement frequency. This is important here, as most UV photometers have a response time of ~10s. Moreover, the value “O3 molecules cm-3” is certainly fine, but most readers will first convert this value in ppbv. Thus, please (also) give a precision such as “ppbv at 1 bar and 2 Hz”.*

> Agreed. The sentence has been changed to: “The instrument has a fast sampling rate (2 Hz), high accuracy (3%), and precision (1.1×10^{10} O_3 molecules cm^{-3} at 2 Hz, or 2.8 ppb at 200 K, 110 hPa, 0.41 ppb at 273 K and 1013 hPa).”

Introduction: It is very general and only in the last paragraph you start with UV photometers. Please, add at least a further paragraph listing the many other O3 instruments for airborne application, inter alia, Kalnais and Avallone, JAOT, 2010, or Hintsa et al., JAOT, 2004.

> We agree with the Reviewer that more instruments need to be cited. However, the current length of Introduction is already 15% of the manuscript. We are reluctant to further lengthen it. As a compromise, a sentence instead of a paragraph is added: “An incomplete list of airborne UV O_3 photometers includes Proffitt and McLaughlin (1983), Bognar and Birks (1996), Ross et al. (2000), Price et al. (2003), Mao et al. (2006), Brenminkmeijer et al. (2007), and Kalnajs and Avallone (2010).” Since Hintsa et al. (2004) only described two ground-based instruments, we did not cite it. Instead, we cited Bognar and Birks (1996) and Ross et al. (2000), since these papers described essentially the same two instruments.

p.3477: What absorption cells are used (material, dimensions)?

> The information has been added now: “An optical-isolator type configuration is utilized to fold the UV beam inside the absorption cells (see Figure 1, Teflon-lined aluminum, 30 cm long with an inner diameter of 1.6 cm), giving an effective 60-cm absorption length with a 30-cm cell.”

p.3480, l.5: Why turbulence increases noise, due to light scattering? Give an explanation and a reference.

> Explanation and references have been added: “The turbulence causes fluctuations of the index of refraction of air, which scatters the UV light (Booker and Gordon, 1950; Munick, 1965) and leads to a slight degradation of instrument precision.”

*p.3482, l.7: Are the intrinsic precision (5 x 109 O3 molecules cm-3) and the total precision (11 x 109 O3 molecules cm-3) completely independent of pressure and the O3 mixing ratio? In this respect I don’t understand the residuals in Fig.8a. For instance, the light blue point at 300 ppbv and 600 hPa. With the above total precision I calculate a precision of 0.7 ppbv. On the plot I see -5 ppbv. The Beer-Lambert equation for inferring the O3 m.r. is [O3] = c(λ,T,p,...) * ln(I0/I) with I0 and I the intensities without and with O3. That is, the noise should almost completely be determined by the precision or ln(I0/I) and not by the other parameters like the absorption coefficient, absorption length etc. that determine the accuracy.*

> The intrinsic and total precisions should both be independent of O3 mixing ratios seen in the atmosphere. The intrinsic precision should have a small dependency on pressure due to Rayleigh scattering, but in practice this effect is probably too small to be seen. The total precision is pressure dependent due to turbulence. This is why the sample flow settings are pressure-dependent. The Reviewer’s analysis of Figure 8 is correct. However, the results presented in Figure 8 should not be used for precision analysis, since O3 generated for the intercomparisons was not regulated. We have added a note to the text to clarify this.

Give a precision in ppbv (at x Hz) at 1000 hPa and at 100 hPa.

> Done.

Try to give an explanation for the increasing noise with increasing sample flow.

> It is due to increased turbulence. Since one explanation and references have been given at the end of Section 2.5 (see above), no action is taken here.

Write if you mean flow at normal conditions or volume flow.

> It is volume flow. Text has been clarified.

Section "3.2 accuracy": You should indicate how your devices compare with the NIST Standard Reference Photometer, see e.g. Viallon et al. Metrologia, 2006. What (T dependent) absorption cross section is used?

> We did not compare to the NIST Standard Reference Photometer directly, but the TECO primary standard we used is NIST traceable. We used absorption cross section recommended by the NASA Data Evaluation Panel (Sander, S. P., Abbatt, J., Barker, J. R., Burkholder, J. B., Friedl, R. R., Golden, D. M., Huie, R. E., Kolb, C. E., Kurylo, M. J., Moortgat, G. K., Orkin, V. L. and Wine, P. H.: Chemical Kinetics and Photochemical Data for Use in Atmospheric Studies, Evaluation No. 17, JPL Publication 10-6, Jet Propulsion Laboratory, Pasadena, 2011 http://jpldataeval.jpl.nasa.gov.). Since we control sample air temperature within 2 K, the cross section change due to temperature variation is negligible. The text has been modified to include this information.

Also here, you should cite more references, e.g. Zucco et al., Meas.Sci.Technol., 2003.

> Done.

p.3484, l.24: What does "negative artefact" mean? The humidity artefact goes in both directions depending if you enter or leave a humid flight section, e.g. a cloud.

> We have clarified it in the text: "A negative artifact (measured values are lower than actual ones) has been observed under some conditions when the sample humidity abruptly changes from wet to dry". We have also added a short paragraph to address the positive artifact: "A positive artifact occurs when humidity increases suddenly. This artifact is much less severe compared to the negative artifact and is negligible in NOAA-1 and NOAA-2, and therefore is not discussed here."

p.3485, l.5ff: In my opinion, this test says little. Such a humidity test is usually performed by abruptly switching from dry to humid air or vice versa, comparable with a flight through a cloud. This artifact strongly depends (besides the scrubber) on the cuvette wall material and cleanness, as the wall controls the amount of adsorbed water and thus affects the transmission.

> The test is more extreme than the instruments will ever experience during the atmospheric measurements, and therefore represents the worst-case scenario.

Do you have an explanation for this strange O3 threshold of 130 ppbv above which no artefact is apparently detectable. At first view this contradicts the common explanation (scrubber, cuvette transmission).

> One possibility is that O3 molecules adsorb on surface and act the same optically as water molecules. Since we don't have any solid evidence, we opt not to speculate in the manuscript.

Fig.8: Important is the percental deviation of the two devices. Thus please choose a log-

log diagram. What tell the different symbols?

> Percent differences are now shown in the top panel. Symbols represent measurement dates. Symbol legends have been added. The graph scales have been changed to logarithmic.

Fig.9: graph a: Except at the beginning and at the end where the pressure is high, in tropospheric air or at low O3 m.r., respc., the blue line is ever below the red one, at ~74000s by at least 5 ppbv. Why?

> We thank the Reviewer for catching this discrepancy. The data have been carefully re-examined and it is found that the discrepancy has a pressure dependency, with the largest difference (on the order of 5%) in the 300-450 hPa range. Examination of the data and recorded instrument parameters did not reveal any problem, except that difference of absorption cell pressures in the two instruments was largest (~ 5%) in this pressure range. Each instrument has a backup pressure sensor. These pressure sensors did not show any indication that any of the two primary sensors are providing inaccurate readings in flight. We have rewritten the text in Section 3.4 to describe this apparent discrepancy.

The noise of NOAA-2 mostly appears larger than of NOAA-1, especially at ~71500s.

> The NOAA-2 data do have larger spikes occasionally, and these spikes are visible in the figure. However, the NOAA-2 data are slightly smoother overall. The last sentence of Section 3.4 has been changed to: "As it is, the NOAA-2 O_3 has a slightly better precision with the 2-point average except occasional large noise (see, e.g., spikes at 71000 s in Figure 9) likely due to air flow instability in front of the inlet."

Graph 9b: again, choose a log-log plot. The deviations sometimes appears high, e.g. around 400 ppbv, and basically far too high when considering the precision and accuracy. Is it due to non-perfect synchronisation or a not perfect consideration of the sample air transport time from the atmosphere to the cells? Are these time shifts considered?

> The plot scales have been changed to logarithmic. The times of the two instruments have to carefully synchronized to within two seconds. The resulting time mis-synchronization is far too small to cause the discrepancies shown in the figure. Figure 9b has been removed. Please see explanation below.

I suggest adding a figure c that shows a counting statistics of the deviation between the two instruments (with Gaussian fit and discussion).

> We thank the Reviewer for this suggestion. By making this figure we have discovered a peculiar problem in the instrument in the 300-450 hPa pressure range in flight. Since this figure contain all information in the old Figure 9b and is much more informative, we have removed the old Figure 9b to reduce paper length and redundancy. The newly discovered problem is thoroughly discussed in Section 3.4.