Response to Reviews for ACP -2018-884, "Detection and Classification of Laminae in Balloon-borne Ozonesonde Profiles: Application to the Long Term Record from Boulder, Colorado", by Minschwaner et al.

We thank both reviewers for their detailed assessments and useful suggestions that have led to improvements in the manuscript. Below, our responses (indicated in blue text) follow each of the reviewers' comments.

Anonymous Referee #2

The authors tested the application of the method to tropical and midlatitude ozonesonde data. I would like to mention two points to be considered when applying this method to the Antarctic.

- In austral spring, ozone concentration inside the Antarctic ozone hole becomes nearly zero, so that the ozone lamina definition based on a relative amplitude of ozone perturbation will cause false detection of many small-amplitude laminae. Inside the ozone hole, ozone-enhanced layers have been studied as a measure of cross-vortex mixing (e.g., Moustaoui et al., 2003; Tomikawa and Sato, 2010). In order to apply this method to the Antarctic, the ozone lamina definition based on the absolute amplitude of ozone perturbation could be required.

- Thermal tropopause cannot be definitely defined over the Antarctic in winter, because temperature decreases with altitude even in the stratosphere. In this case, it may not be appropriate to use the thermal tropopause as a tropopause definition. On the other hand, ozone tropopause can be clearly defined even in the Antarctic winter (Tomikawa et al., 2009), so that its usage for the tropopause definition could be better.

Reviewer #2 has raised some important issues that should be considered when applying our method to winter-spring soundings in and around the southern polar vortex. We would most likely have to revise our criteria and thresholds for both ozone laminae and the tropopause in order to maintain a robust analysis under conditions of significant ozone depletion and/or changes to the thermal structure of the lower stratosphere. Note that these issues may also arise in the northern polar vortex during cold winters. On the other hand, a preliminary analyses of other midlatitude and tropical ozonesonde datasets using the standard set of RIO SOL parameters has been done, and the results appear to be similar in accuracy to those from Boulder. A detailed comparison of ozone laminae from different measurement sites is planned for future investigations, but in this paper, our emphasis is on a description of techniques and a general climatology from Boulder. As both reviewers have noted the obvious extension to datasets from other sites (see also response to Reviewer #3 below), the manuscript has been revised to include a discussion of the applicability of using RIO SOL with the current set of parameters for analysis of soundings from other locations (p. 9).

The authors reported that the GW lamina maximized around the tropopause. Is there a possibility that it was caused by false detection of ozone and potential temperature lamina around the tropopause? It should be discussed in the manuscript.

We examined this possibility and have concluded that the use of a narrower width filter in the vicinity of the tropopause effectively eliminates false detections due to sharp gradient changes, under most tropopause conditions. On p.6 we stated that there was no altitude dependence in the frequency of false detections in our simulated lamina tests. New text has been added on p.8 to stress this point in connection with our discussion of the maximum in laminae frequency seen just above the tropopause.

Is there a plan to disclose “RIO SOL” to the research community? If yes, please mention it in the text.

RIO SOL and its documentation are available upon request from the lead author. A short section on “Data and Code Availability” has been added after the Conclusion section.

p.9, l.4 Please put “-“ between January and February.

p.11, l.15 “Krizan et al (2016)” should be replaced by “Krizan et al. (2015)”.

p.13, l.11 Please put “2011” at the end of this reference.

p.13-16 Isotta et al. (2008), Schmidt et al. (2008), and Thompson et al. (2010) are not cited in the text.

p.17, l.3 “fight” should be replaced by “right”.

All of the above changes have been implemented, Isotta et al. (2008) is now cited in the text, and the Thompson et al (2010) reference has been deleted. Note Schmidt et al. (2008) was already cited, but Schmidt et al. (2006) was not cited and that reference has also been deleted.

Anonymous Referee #3

It would be useful for the authors to show a comparison of the prior approach(es) as applied to the Boulder data set to the current approach to better and more clearly identify the laminae. In the present version, the authors identify gravity wave (GW) and non-gravity wave (NGW) laminae. While the authors note the similarity in the frequency of NGW laminae to Rossby wave frequencies identified in prior papers, it would be useful to use those prior techniques to specify the Rossby wave frequency.

A comparison between RIO SOL and a prior approach for a single ozonesonde profile is presented in Figure 2, and we have also added a new paragraph discussing the Boulder laminae climatology from RIO SOL in comparison to a fixed-width filter and correlation window technique (p. 9). In terms of classifying “Rossby wave laminae”, we fail to see the physical mechanism(s) underlying their identification using correlation thresholds between ozone and potential temperature, as has been done in prior papers. While we expect some fraction of NGW ozone laminae are, in fact, associated with Rossby wave activity, their connection (or lack thereof) with perturbations in Θ is not as clear as in the case of GW laminae. We have attempted to clarify this view with new text on p. 11.

The new technique is applied to a single data set – Boulder. That midlatitude site is located fairly close to and just downwind of the Rocky Mountains. How well does the technique work in other locations? What constraints (if any) are there in application of this new approach?

Both of these issues - constraints on the application of RIO SOL, and suitability for other locations, are now addressed on p. 9 of the manuscript (see also response to Reviewer 2 above).

How would this technique do with laminae due to “notches” appearing due to SO_2 interference (e.g., Morris et al., 2010, J. Atmos. Ocean. Tech.) in the cathode cell reactions of the ozonesonde?

Any process which leads to a perturbation in the ozonesonde profile, whether real or an artifact in the measurement, will also lead to lamina identification in RIO SOL if the amplitude exceeds 10% and the vertical scale is between 0.2 and ~2 km. This is now discussed on p. 7.

The authors also note that negative laminae occurred more frequently than positive ones. What is the physical mechanism responsible for this difference in frequency?

The physical mechanism responsible for the higher frequency of negative laminae is not definitively known. Perhaps in future studies, when processes leading to the generation of NGW laminae are more clearly identified, the reasons for this difference will become clearer.

How does the detection of laminae relate to instrument response time and ascent rate? Given that the magnitudes of the laminae are critical to their detection, if ascent rates are too fast or response times too slow, how will that impact the detection of laminae?

The detection of laminae is not significantly impacted by ascent rates and response time under most conditions. However, we do anticipate a possible systematic offset in laminae altitudes on the order of 100 m. A discussion of this effect has been added on p. 7.