

Author comment's

The authors acknowledge to referees and the editor for the time spent to review this manuscript and also for their constructive comments.

Point-by-point response to the referee comment's 1: Lucas Vaz Peres

The authors acknowledge to Prof. Lucas Vaz Peres for the time spent to review this manuscript and also for their constructive comments.

The manuscript was revised and improving according the referee comments and suggestions.

The specific answers are **in blue**, while the referee comments are **in black**.

"Analysis of a southern sub-polar short-term ozone variation event using a Millimeter-Wave Radiometer".

General comments:

The authors present a study about an atypical event of polar vortex and ozone hole influence over Río Gallegos during November of 2014. This event was detected from the Millimeter Wave Radiometer (MWR) measurements at 27 and 37 km and the advected potential vorticity (APV) was calculated from the high-resolution advection model MIMOSA (Modélisation Isentrope du transport Mésoéchelle de l'Ozone Stratosphérique par Advection) at 675 and 950 K to understand and explain the atmospheric dynamic related to ozone rapid variation during the passage of the polar vortex. In addition, the MWR dataset were compared for first time with Microwave Limb Sounder (MLS) to 27 km, 37 km and 65 km and with the Differential Absorption Lidar (DIAL) installed in Observatorio Atmosférico de la Patagonia Austral (OAPA) between October 2014 and 2015.

This work is a useful representation of the important contribution made by the Millimeter Wave Radiometer (MWR) at Río Gallegos and certainly, understand the ozone hole influence over Río Gallegos is of fundamental importance in many environmental processes which can lead to increases in the UV radiation on the surface. This increase in the UV radiation related to ozone reductions can be dangerous to life on earth and it represents a significant scientific advance.

After the first review, major corrections were suggested and in general the authors were able to remedy the main failures observed. The Point-by-point response to the referee comment's was very clear and precise in most points, however small details can still be improved especially in discussing results in the literature, although this has already been greatly improved.

Because of these I would recommend to accept with Minor Revisions this manuscript. Also, because I am not a English language native speaker, I suggest to the editor to check if the English is proper for publication.

Specific comments:

* In the abstract: Requested modifications are made.

* In the 1. Introduction: Requested modifications are ok.

* In the 2. Materials and methods: Requested modifications are ok. However, a subsection should be created describing the UV radiation data used in this new version of the manuscript.

The following subsection was included (pg. 8, line 3):

"2.1.5 Solar Radiometer YES UVB-1

The ground-based radiometer YES UVB-1 (Yankee Environmental System, Inc.) installed in OAPA is used to measure the erythemal irradiance UVB at surface, and the UVI is retrieved. It is connected to a data logger, which is configured to acquire one measurement per minute. Due to the UVI is strongly affected by the ozone amount in the atmosphere, the time evolution of the daily maximum UVI is analysed during the period of the case study. We decided to present the daily maximum UVI instead of the UVI at solar noon due to the fact that most of the analysed days during the low ozone event were partially cloudy and the maximum UVI were observed near the solar noon. Thus, the daily maximum UVI is more representative in terms of the low ozone amount impact over the clear sky UVI at surface."

In addition, the following sentence was included in the "1 introduction" section and in the "2.1 Observation" section to introduce the UVI measurements, respectively:

Page 4, line 15: "Finally, the solar Ultraviolet Index (UVI) at surface is also analysed during the event."

Page 4, line 28: "Due to the relationship between ozone amount and the solar UVB radiation at surface, this parameter is also measured in the OAPA with a ground-based solar radiometer YES UVB-1 (Yankee Environmental System, Inc.)."

*** 2.3**

Why the comparison of the MWR with DIAL occurs only for the 27 km height. It could also occur for 37 km. Or is there any impediment?

The power of the laser used in the DIAL installed in the OAPA was not stronger enough to reach 37 km. For this reason, the comparison between MWR and DIAL are not possible at this altitude.

* In the 3. Results: Requested modifications are ok.

* Y axis of figure 7a (top) should contain values throughout the graph and contemplate values close to 230 DU observed.

The value "240" was added in the Y axis (page 30).

* In the 4. Discussion

creating a discussion section instead of discussing the results as they occur in the text is always a dilemma. You can make the mistake of not discussing some results in the literature. Comparing the results with what was discussed. It is observed that not all results were properly discussed as listed below:

- In Pg 11, line 5: "The difference between measurements can be attributed to the typical uncertainties of each instrument, although another source of difference is introduced due to

the non-collocated measurements inter-compared". This affirmation was discussed but not referenced.

To clarify this sentence, it was re-write as follow:

"The difference between measurements can be attributed to the typical uncertainties of each instrument, although another source of difference could be explain by the non-collocated measurements inter-compared"

This sentence is followed by the "This point is discussed in section 5". Into the discussion section were included the reference Sugita et al. (2017). The following sentence references the suggestion in *Discussion* (Page 15, line 13):

"Comparisons between DIAL and MLS were realized by Sugita et al. (2017) for an unusual case of persistence of the AOH over Río Gallegos occurred during November 2009, who also attributed part of the differences to the non-collocation of the measurements."

- Discussion on results related to UV radiation should be attached.

The following paragraph is added (pg 14, line 15):

"The time evolution of the daily maximum UVI was also analysed during the study period. As expected, we find an opposite behavior respect the total ozone column, which is in agreement with other results reported (Casiccia et al., 2008; Wolfram et al., 2012). It is observed a local minimum when the measurements of the ozone amount retrieved by the MWR presents a maximum at both altitudes."

- Discussion on results related to AOH influences should be improved, mainly in relation to the advection of potential vorticity process which caused the observed ozone reduction.

The following paragraph is added (pg 13, line 30):

"The influence of the polar vortex during the analysed period was confirmed in the APV from the MIMOSA model at two isentropic levels (675K and 950K). We observed filaments of air-mass from the polar vortex at both potential temperature levels passing over Río Gallegos. Similar cases of filaments travelling toward mid latitudes in the South Hemisphere have been reported analysing the APV (Vaugh, 1993) without the possibility to report the stratospheric ozone amount with the time resolution reported here."

- The first paragraph of the discussion has affirmations without reference.

Reference were added (pg 13, line 25):

"It is well known that the southern part of South America is affected by the frequent abrupt intrusions of the AOH during the spring (Wolfram et al. (2012), Kirchhoff et al. (1997), WMO, 2013; WMO, 2012; WMO, 2011b)"

- The present affirmation was not discussed: "When we compare the MWR with the MLS, it is considered that both instruments are measuring the same air masses, although the location of the satellite measurements differs from the location of the MWR measurements, which can introduce a difference in the ozone mixing ratio measured."

This sentence is a kind of introduction of the discussion that it does is discussed in the next two paragraphs. It is not an affirmation. It suggest that the difference between the MWR and MLS can be attributed to the difference in the location of the measurements under the

criterion of closeness between measurement described in the "2.3 Methodology and consideration".

This sentence can be separated in three parts:

1. "When we compare the MWR with the MLS, it is considered that both instruments are measuring the same air masses"
We can consider that both instruments are measure the same air mass under the criterion of closeness that is explain in the second paragraph of the "2.3 Methodology and consideration" section. Strictly, both measurements are not in the same location because the location of the satellite measurements differs from the location of the MWR measurements, which is expressed in the second part of the sentence.
2. "The location of the satellite measurements differs from the location of the MWR measurements"
This part of the sentence was analyzed in the section "2.3 Methodology and consideration" and in Figure 3. It is a fact.
3. "which can introduce a difference in the ozone mixing ratio measured"
In this part of the sentence we put the word "can" to emphasize the suggestion that the non-strictly-collocated measurements could introduce differences in the intercomparison, which is obvious.

In addition, a reference was included in the discussion of this affirmation in pg. 15, line 14 which suggest the same (*"Comparisons between DIAL and MLS were realized by Sugita et al. (2017) for an unusual case of persistence of the AOH over Río Gallegos occurred during November 2009, who also attributed part of the differences to the non-colocation of the measurements."*)

To clarify the sentence, we decided to re-write it as follow (change the "it is" by "we" and change the word "can" by "could"):

"When we compare the MWR with the MLS, we considered that both instruments are measuring the same air masses, although strictly the location of the satellite measurements differs from the location of the MWR measurements, which could introduce a difference in the ozone mixing ratio measured."

- The present affirmation was not discussed: "One reason why the correspondence between the MWR and the DIAL is greater with respect to the MLS may be that the two instruments installed on the ground (MWR and DIAL) are monitoring the same air mass, while the distance with the location of the MLS observations could be introducing differences in the comparison".

It is a suggestion at the same way than the previous comment.

Here, what we want to say is that as the ground based MWR and DIAL instruments are monitoring ozone in the same place, the comparison are expected (and it is) to be better than the comparison between the MWR and the MLS, which are monitoring the ozone in the different location. We suggest a possible reason about why the intercomparison between the collocated instruments (MWR and DIAL) is better than between non-strictly-collocated instruments (MWR and MLS).

- The present affirmation was not discussed: "It is important to note that the MWR and DIAL instruments retrieve ozone in different fundamental units. While the MWR provides the ozone mixing ratio, the DIAL provides the ozone number density as a function of altitude. The DIAL unit was converted to the MWR unit for the inter-comparison using the temperature and

pressure retrieved from the DIAL. Thus, uncertainties in these parameters could be adding uncertainties in the ozone amount in ppm from the DIAL”.

This paragraph refer the error propagation which increment the uncertainties when we convert the measurement from the fundamental unit of the DIAL (molecules/cm³) to other unit (ozone mixing ratio) using other measurements (pressure and temperature for this case). When we convert the unit of any measurement to another using other measurements, there is error propagation intrinsic that come from the error of each measurement.

* Conclusions

- Requested modifications are ok.

Point-by-point response to the referee comment's **2:Anonymous.**

The authors acknowledge the anonymous referee for the time spent to review this manuscript and also for their constructive comments.

The manuscript was revised and improving according to the referee comments and suggestions.

The specific answers are **in blue**, while the referee comments are in black.

Revision of MS No.: angeo-2019-17-R1

Title: Analysis of a southern sub-polar short-term ozone variation event using a Millimeter-Wave Radiometer.

Authors: Pablo Facundo Orte, Elian Wolfram, Jacobo Salvador, Akira Mizuno, Nelson Bègue, Hassan Bencherif, Juan Lucas Bali, Raúl D'Elia, Andrea Pazmiño, Sophie Godin-Beekmann, Hirofumi Ohyama, Jonathan Quiroga.

Overall evaluation:

The manuscript has definitely improved. However, this second revision evidences still several necessary corrections. Again, these comments may be considered as relatively “minor changes”, but in my opinion they are mandatories to consider the manuscript as acceptable for publication:

Specific comments:

- Abstract: sentence “The measurement shows a very short-term recovery in the middle of ozone mixing ratio decrease that could be detected by the MWR” is confuse. Please clarify.

To clarify the sentence, it was replaced by the following sentences:

“During the event, the MWR observations at both altitudes show a decrease of ozone followed by a local peak of ozone amount of the order of hours. This local recovery is observed thanks to the high time resolution of the MWR mentioned.”

- The concepts of potential temperature, isentropic surface, isentropic level, are introduced after temperatures 675 K and 950 K are mentioned, starting with the Abstract. Please define the concept as at the beginning as possible and unify terminology. In my opinion, the denomination of "isentropic level" is the most appropriate and should be used to avoid different denominations for the same concept.

The concept of isentropic level was introduced in the abstract in brackets:

*"The advected potential vorticity (APV) calculated from the high-resolution advection model MIMOSA (Modélisation Isentropie du transport Méso-échelle de l'Ozone Stratosphérique par Advection) was also analysed at two isentropic levels (**levels of constant potential temperature**) of 675 and 950 K (~27 km and ~37 km of altitude, respectively) ..."*

The terminology was unified to "isentropic level":

- Page 8, line 14: "Isentropic surface" was changed by "isentropic level".
- Page 13, line 6 and 7: "Potential temperature" was changed by "isentropic level".
- Page 33 (figure caption of the figure 8): "Maps show the evolution of the polar vortex for two isentropic levels with potential temperatures of 675K (left) and 950K (right)."

- In the same aspect, the fact that isentropic levels 675 K and 950 K correspond to approximately ~27km and ~37km is explained too late in the text (page 12). Please clarify this fact as at the beginning as possible in the text.

It was clarified in the abstract, page 1, line 30 in brackets (please, see previous comment).

- Page 2, line 32: sentence "Ground and space-based observations and models have shown an increase of the total ozone since 2000" is incoherent with the two following sentences: "Nevertheless, this increase is not significant for the period 2000-2013 (WMO, 2014). Ball et al. (2018) extended this period from 1998 to 2016 and concluded that there are non-significant changes in the total amount of ozone from merged ozone datasets". Please clarify.

To clarify the sentence, it was removed and the following sentence (**in bold**) was added:

*"Together with the banning in the use of ODS set by the 1987 Montreal Protocol, the general expectation was that the TOC would recover as the amount of ODS decreased in all regions. Recent studies showed a recovery of the stratospheric ozone column during September (statistically significant) and October (statistically insignificant) for the South Polar Region (Salomon et al., 2016; Weber et al., 2018; Pazmiño et al., 2018). **Outside the polar region (between 60 S and 60 N) Ball et al. (2018) concluded that there are non-significant changes in the total amount of ozone between 1998-2016 from merged ozone datasets, although they reported a decrease in the stratospheric ozone layer, which this findings imply an increase in the tropospheric ozone.**"*

- Page 3, line 20: Bresciani et al. (2018) named this phenomenon "secondary effect of the Antarctic ozone hole", concluding that "Data revealed the poor ozone air mass trajectory from some days before arriving in southern Brazil and Uruguay to some days after its passage, and confirmed its polar origin", but... could this sentence be interpreted as the authors refers to?: an actual pass of the polar vortex over Uruguay and Southern Brazil?.

Here, we want to describe the influence of the AOH affecting the Uruguay and Southern Brazil region. The sentence was changed by (in bold):

*"This phenomenon was first observed by Kirchhoff et al. (1996) and reported by Pinheiro et al. (2011) in South America. **Recently, based on satellite and ground-based observations in Uruguay and Southern Brazil, Bresciani et al. (2018) showed a decrease of ozone over these sites during October 2016 due to the influence of the AOH reaching mid-latitudes.**"*

- Page 11, line 13: $R=0.68$ is mentioned for altitude 27 km, but in item 3.1.1, table 1 and figure 5 it is referred to as $R=0.65$.

It was revised. The $R=0.68$ was changed by 0.65.

- Page 12, paragraph starting in line 15: an adaptation of the phrase from the answers to referee "We decided to present the daily maximum UVI due to the fact that most of the analysed days were partially cloudy with broken clouds, and the maximum UVI were measured near to the noon" must be included in the text of the manuscript.

The sentence in bold was included:

*"Figure 7b (blue dots) presents the time series of the daily Ultraviolet Index (UVI) maximum (near the **solar noon**) during the low ozone event described before measured with a radiometer YES UVB-1 (Yankee Environmental System, Inc.) installed in the OAPA. **We decided to present the daily maximum UVI instead of the UVI at solar noon due to the fact that most of the analysed days were partially cloudy and the maximum UVI were observed near the solar noon and it are more representative in terms of the low ozone amount impact over the UVI.**"*

- Figure 8: plots on the left (675 K) and on the right (950 K) look too similar given that the same colour scale is used in both, but in fact the actual values of APV are totally different. I suggest unify the scale of colours using the same scale on both sides with the total range APV (0-900), but still highlighting with colours and tones the features you want emphasize: for instance the same present colours in the range 0-200 (blue to yellow) but then passing to (red to pink) in the range 200-900.

The colour scale of plot on the right (950K) was changing to avoid confusion between both scales. We took the suggestion of the referee and some colour scales were proved and we decided to set the colour as we present in the new version of the manuscript. We considered that this colour scale highlight the features that we want to show in the event.

Minor comments:

- Page 2, line 5: Add comma: "It acts as an absorber of harmful solar UVB radiation, ...".

Comma was added.

- Page 2, line 7: please change "Although most production takes place in the equatorial region" by "Although most production takes place in the mesospheric-stratospheric equatorial region".

It was changed.

- Page 2, line 21: Please revise sentence "the total ozone column (TOC) and vertical reduction", it could be better simply "ozone reduction at different height levels".

It was changed.

- Page 2, line 25: "More recent study reported reduction of 40-45% in TOC over Río Gallegos", please specify the date of the reported TOC reduction.

It was specified as follow:

"More recent study reported reduction of 40-45% in TOC over Río Gallegos on October 2008 and November 2009 (Kuttippurath et al. 2010b)."

- Page 3, line 15: change by "The passage of the AOH is identified using the TOC threshold of 220 DU".

It was changed.

- Page 4, line 27: change by "and the unique installed in subpolar region".

It was changed.

- Page 4, line 31: Did you mean "improving the validation of the dynamical models"?

The word was changed by "validation".

- Page 10, line 4: remove word "system": 3.1 Inter-comparison of MWR with DIAL and MLS observations.

I was removed.

- Page 10, line 10: change by "campaigns become".

It was changed.

- Page 12, line 27: change "at both potential temperatures" by "at both isentropic levels".

It was changed.

English must be still revised in many sentences throughout the text, only for example:

- Page 1, line 30: "explain the dynamics".

It was changed.

- Page 2, line 20: "and they will remain for decades".

It was changed.

- Page 2, line 25: "A more recent study reported a reduction of 40-45% in TOC over Río Gallegos".

It was changed.

- Page 2, line 27: "respect to normal".

It was changed.

- Page 3, line 26: "... reaches mid-latitudes and produces".

It was changed.

- Page 4, line 31: "at these latitudes".

It was changed.

- Page 11, line 26: Remove word "on": "during November 2014".

It was removed

- Page 12, line 15: change by "daily maximum Ultraviolet Index (UVI)".

It was changed.

- Page 13, line 15: remove "from": "suffers sudden".

It was removed