Reply to the second reviews of Referees #1 and #2 on the manuscript acp-2021-1066 (Ozone-gravity wave interaction in the upper stratosphere/lower mesosphere, by A. Gabriel)

1) Reply to the comments of Referee #1

Many thanks again to Referee #1 for critical comments. Please find here the reply (again, for orientation, the comments of Referee #1 are included in *Italic*).

General Comment:

The reviewer appreciates the efforts undertaken to prepare a revised manuscript. Unfortunately, the revised manuscript did not pick up some of the suggestions and even contains statements that are questionable. The reviewer provides below a detailed list of reasons why the manuscript in its present form is suggested for rejection.

I am a bit irritated, because I do not really see that I did not pick up some of the earlier suggestions. However, the manuscript is revised again carefully following the list of main concerns given below.

In parts, some of these aspects originate from different viewpoints between theoretical approaches and experimental data analysis and the current understanding of vertical coupling processes and so forth. Some simplifications are justifiable for theoretical solutions as the proposed ozone-gravity interaction, but a simple relation to observations is not adequate to support the conclusions. This is really a problematic aspect of the revised manuscript.

I do not really understand these statements. The theoretical approach of the paper is very clear: it is based on standard equations describing gravity waves in a constant background flow, with an additional equation for the ozone perturbations due to gravity waves but excluding other processes to provide clear understanding of the potential effect on the GW amplitudes (such an approach is usual before introducing a sophisticated parameterization into a model); the resulting differences between GW amplitudes with and without ozone-gravity waves gives an idea of this potential effect in comparison with cited GWPED values already published in refereed journals by other authors; the paper does not include any experimental data analysis, and it does not want to explain entirely the GWPED values derived from measurements *only* by ozone-gravity wave interaction or to give a simple relation to observations. In the revised manuscript, this is somewhat more highlighted from the beginning (abstract: ll. 28-30, introduction: ll. 86-89, discussion: ll. 553-555, ll. 571-574).

Obviously, the paper can still lead to some misunderstanding, perhaps because only single GWs are considered, with focus on the relative change in the GW amplitudes. Therefore, an additional Figure 7 is added illustrating the amplification of the GWPED for mean values averaged over representative mesoscale GWs with different horizontal and vertical wavelengths, which gives an additional idea on the potential effect (abstract: ll. 39-42; new section 2.2.5, ll. 483-509; discussion: ll. 544-555). Figure 7 includes not only the relative but also the absolute amplitude amplifications for moderate initial GW perturbations in the middle stratosphere (1 K), which might illustrate somewhat clearer that this effect is not *tiny* as claimed by reviewer #1 below.

Main concerns:

Lidar data interpretation:

The manuscript is now motivated entirely by one single lidar paper (Baumgarten et al., 2017). The main narrative of the paper is a gravity wave lidar climatology leveraging a daylight capable Rayleigh lidar at the mid-latitudes. Although there is one statement in the conclusion about the day and night differences associated with atmospheric tides and the corresponding filtering, it is only speculated on these differences. During daylight, these lidars show an increased noise floor due to the sunlight, which causes a less good signal-to-noise ratio compared to nighttime measurements, which results in larger or increased fluctuations for the analyzed temperatures. However, this increased variability is mainly the result of hydrostatic integration applied for the temperature inversion and not a sign of geophysical variability. Rüfenacht et al., 2018 show an impressive example of the day and night noise levels in the sister Rayleigh system at Andenes.

The manuscript is improved again to highlight that the cited daylight-nighttime differences are only one point of Baumgarten et al. (2017), and that these differences include uncertainties due to a less good signal-to-noise ratio during daylight (abstract: ll. 28; introduction, ll. 75-78). Please note again that the conclusion of Baumgarten et al. (2017) concerning the daylight-nighttime differences is cited accurately as published in a refereed journal, and that these findings were only one motivation of others. The aim of the present paper is to examine the potential effect of ozone-gravity wave interaction excluding other processes, and not to explain entirely total GWPED values derived from some few specific Lidar measurements. This is somewhat more highlighted from the beginning (see reply to general comment above).

Furthermore, even in Baumgarten et al., 2017 there is no convincing signature of such a day and night difference (see Figure 3).

This statement is not right. A comparison between GWPED values derived from full-day and nighttime observations are given in Figure 6 and Figure 9 of Baumgarten et al. (2017). For July, the GWPED values are significantly stronger during full-day- than nighttime observations by a factor of approximately 2 (significant within one standard deviation). For clarification, a related comment is included in the revised manuscript (ll. 70-74).

Atmospheric tides:

Although it appears eligible to use HARMONIA without tides as background for the theoretical prediction of the ozone-gravity wave interaction, it is not advisable to ignore tides in the lidar data. Most lidar soundings are significantly biased due to the short record lengths and proper removal of tidal effects is often not possible in the GWPED. Most published amplitudes of individual tidal modes are in the order of a few K, which is often related to a systematic underestimation from the applied superposed epoch analysis.

Yes, I agree, it is not advisable to ignore tides when interpreting fluctuations derived from lidar data. Interpreting the fluctuations derived from lidar data has been done by those authors who presented the cited GWPED values as a reliable result of lidar measurements in refereed journals, including uncertainty ranges because of the difficulties in separating the GW perturbations and the fluctuations due to tides. In the present paper on ozone-gravity wave interaction, these GWPED values derived from lidar data are cited because they give a benchmark and orientation on the GWPED values in the stratosphere and mesosphere, nothing else (see revised introduction, ll. 53-67). Please note here again that the purpose of the present paper is to estimate the potential effect of ozone-gravity wave interaction on the daylight-nighttime differences in GW amplitudes as clear as possible excluding other processes like tides (ll. 86-89, ll. 315-316).

Uncertainties of the GWPED values derived from lidar data are already highlighted in the introduction, particularly the problem of temporal filtering methods and speculations whether diurnal or semidiurnal tides could contribute to the daylight-nighttime differences in the GWPED values derived by Baumgarten et al. (2017) (ll. 74-80). Possible effects of the modulation of the background conditions due to tides are already discussed (ll. 452-467). In the revised lidar part in the discussion, the possible role of tides for the daylight-nighttime differences is somewhat more highlighted (ll. 607-618).

Just looking at Figure 1 in Baumgarten et al., 2019 or attached MERRA2 data reveals a temperature difference between 50-60 km of about 30 K, which corresponds to a lapse rate of -3 K/km. This value is 20-30 times larger than the 0.1 K/km stated in the manuscript, which means that all the discussion and included factors are obsolete. In fact, all factors are increased by a factor 20-30 and, thus, become no longer negligible. The reviewer assumes that the small vertical temperature gradient was estimated from the vertical profile of the mean tidal amplitudes, which is misleading in this case.

I do not understand these estimations. Figure 1 of Baumgarten and Stober (2019) show total temperatures at mid-latitudes during May, illustrating the total lapse rate. In the manuscript, the total temperature and the related time-mean lapse rate are prescribed as background with a difference between 50-60 km of about 30 K at summer mid-latitudes produced by the HAMMONIA model (see Figure 1a, ll. 121-122, ll. 258-261). Figure 2 of Baumgarten and Stober (2019) show tidal variations between −5 K and +5 K and the following figures of Baumgarten and Stober (2019) show the amplitudes of the tidal variations with increase from about 0.5 K at 30 km up to about 4 K at 70km, derived from lidar data and MERRA2. This variation of the background lapse rate in the order of 0.1 K/km is used as input in the sensitivity test described in section 2.2.4 (ll. 459-465). Even an artificially assumed much larger change in the lapse rate (10% to 50% instead of 1%, corresponding to −1 to −5 K/km) does not change the amplification of the upper mesospheric GW amplitudes by more than 10% (ll. 465-467). This relatively weak sensitivity of the amplification to variations in the lapse rate γ is related to the facts that the stability in terms of $N_0^2=g\,(\Gamma-\gamma)/T_0$ is primarily given by the dry adiabatic lapse rate $\Gamma\approx10$ K/km and that the introduced ozone adiabatic lapse rate in terms of N_μ^2 is a linear function of N_0^2 (see Eq. (14)). Therefore, the discussion and the derived factors are not obsolete.

Below there are two panels showing data from MERRA2 for an undisclosed mid-latitude location. The dominating feature is the diurnal tide in the wind and temperature.

The corresponding gravity wave activity for the same period is shown below. The mean and atmospheric tides have been removed. The waves discussed by the manuscript are partially resolved from MERRA2 and appear as the coherent structure at altitudes from 16-50 km. Above the coherence disappears, and a superposition of upward and downward phase lines becomes evident. However, it is not clear whether the data assimilation 3DVAR in MERRA2 captures the secondary wave generation around 60 km (fishbone structure Vadas et a., *2018a,b) or whether these waves are reflected from the model top and not sufficiently suppressed by the sponge layer.*

The reviewer is not convinced that the arguments listed in lines 425-438 actually are valid looking at the MERRA2 data, which is also confirmed by other meteorological reanalysis and model fields. Furthermore, the gravity residuals reflect almost no coherence above 55-60 km, which also does not support the proposed effect considering that a sun-synchronous phase relation of the gravity waves would be required to sustain a fixed phase relation to the ozone to ensure the day and night differences.

Thanks a lot for the figures. As mentioned in the previous reply, the secondary GW structures are indeed interesting, but I do not really understand why this should be discussed in detail in the present paper. The assimilation model of MERRA2 does not include ozone-gravity wave interaction in the upper stratosphere/lower mesosphere because the photochemistry module uses monthly 2-dimensional ozone production rates and loss frequencies derived from a two-dimensional chemistry model (Stajner et al., 2008; Wargan et al., 2015; this approach is sufficient for the purposes of the MERRA2 assimilations because ozone has a sufficiently long lifetime in the lower and middle stratosphere). Therefore, MERRA2 data seems to be not suitable to support or to reject the presented results on ozone-gravity wave interaction.

Stajner I, et al. Assimilated ozone from EOS-Aura: Evaluation of the tropopause region and tropospheric columns. J Geophys Res. 2008;113:D16S32. doi: 10.1029/2007JD008863.

Wargan K, Pawson S, Olsen MA, Witte JC, Douglass AR, Ziemke JR, Strahan SE, Nielsen JE. The global structure of upper troposphere- lower stratosphere ozone in GEOS-5: A multiyear assimilation of EOS Aura data. J Geophys Res Atmos. 2015;120:2013–2036. doi: 10.1002/2014JD022493.

Perhaps I may add that I see in the plots not only fishbone structures but also some coherent GW structures with increasing amplitude above 50 km, some up to 70 km. It is known that dissipation increases if the GW amplitudes increase with height, which might be the reason that a fraction of the coherent GW structures disappear. The origin of the GW structures above 60 km might be unclear not only because of reflection from the model top but also because of artificial non-geostrophic flow components forced by setting the upper boundary of the atmosphere at the model top. However, I agree with reviewer #1 that the GW structures above 60 km produced by the MERRA2 assimilation model are very uncertain; therefore, here again, they are not suitable to support or to reject the results of the present paper.

However, the concern of referee #1 might be related to short-term fluctuations due to tides and not to a modulation of the large-scale background estimated in section 2.2.4 (lines 425-438 are now at ll. 454-467). As mentioned above (and in the paper), short-term fluctuations in the balanced winds due to tides or other processes are explicitly excluded by the approach to focus on the effect of ozone-gravity wave interaction as clear as possible. However, in the revised manuscript, possible effects of these fluctuations and related nonlinear interactions with GWs are now more highlighted in the discussion (ll. 595-618; see also the reply to the related main concerns on *background winds* and *multistep vertical coupling* below).

Background winds and atmospheric waves:

Reanalysis data, as well as observations, indicate that the gravity wave amplitudes are strongly affected depending on the phase velocity and direction of the gravity wave relative to *the background winds. At the altitudes presented and discussed in the submitted manuscript, these changes are mostly driven by atmospheric tides with rather short vertical wavelengths and, thus, a sudden amplitude growth of the gravity waves is also explainable just by changes in the background winds, which are not captured or considered in the theoretical framework.*

The present paper assumes the usual standard approach of upward propagating GWs in a slowly varying background flow excluding short-term fluctuations of the background wind due to atmospheric tides or other processes (ll. 86-89, ll. 315-316). In other words, yes, of course, changes in the amplitude growth of gravity waves are not captured or considered by the theoretical framework because they are explicitly excluded from the beginning to estimate the effect of ozone-gravity wave interaction as clear as possible. Usually, such an approach assuming GWs in a slowly varying background is the starting point of GW parameterizations used in current general circulation models; therefore, the results of the paper could be quite interesting for many researchers.

However, slowly varying changes in the background wind are discussed in section 2.2.4 (ll. 459-465), whereas a quantification of the effects of short-term changes in the balanced winds (short-term changes in the background winds do not exist because they are not a slowly varying background for the short-term perturbations), and associated non-linear interaction between the upward propagating GWs and the short-term fluctuations of the balanced winds, needs much more sophisticated solutions or extensive numerical simulations with sufficiently high spatial and temporal resolution (to my knowledge this is an issue of current research), in case of examining ozone-gravity interaction with including an interactively coupled photochemistry model, which is beyond the scope of the paper. This is somewhat more highlighted in the discussion (ll. 595-618).

Multistep vertical coupling:

As already mentioned, the HIAMCM model, as well as reanalysis data including MERRA2, reveal fishbone structures, which are associated with local body forces of breaking gravity waves or jet-induced instabilities (Vadas et al., 2018a,b, and many others). The GWPED observations are not discussed whether the increase in amplitude could be caused by that effect. The manuscript makes an attempt to justify the amplification only by the ozone effect, which seems unlikely and needs to be quantified. In summary, the cited GWPED data is not supporting the conclusions of the theoretically predicted amplification.

Both the HIAMCM model and the MERRA2 assimilation model do not include the feedback of ozone photochemistry to GW perturbations, therefore these models are not suitable to provide any conclusion on ozone-gravity wave interaction. The fishbone structures and the related processes are indeed interesting, but it is not the purpose of the manuscript to examine and to quantify the possible effects of these processes on observed GWPED observations. I guess these fishbone structures will change significantly if introducing ozone-gravity wave coupling into the models; however, this would be an issue of another paper.

As already mentioned in the first reply, it is not right to claim that the manuscript wants to explain the daylight-nighttime differences in the GWPED *only* by the ozone effect. The purpose of the manuscript is to examine and to quantify the effect of ozone-gravity wave interaction excluding other processes, and then to have a look whether the resulting amplification is strong or weak in relation to total GWPED values published in refereed journals. If it is strong enough, the conclusion that ozone-gravity wave interaction is one significant component amplifying the GW amplitudes and GWPED values is justified. This usual procedure is meaningful before introducing this process in sophisticated GWD parameterizations used in circulation models or in extensive numerical model calculations with high spatial and temporal resolution and interactive photochemistry.

The manuscript is revised again to make this point clearer (ll. 53-89; ll. 539-555; see also reply to general comment above). An additional comment which explicitly highlights that also other processes like multistep vertical coupling could principally contribute to the daylight-nighttime differences is included in the discussion (ll. 595-618).

Recommendation:

The reviewer suggests minimizing the lidar part to the introduction and as motivation, but clearly describing that the lidar data does not permit to distinguish between the nature or source of the gravity waves and whether multistep vertical coupling or increased noise during the daylight could explain the tiny anomalies in GWPED as well.

Introduction and discussion are improved (ll. 53-89, ll. 539-555, ll. 566-574, ll. 595-618). I never claimed that any *lidar data permit to distinguish between the nature or source of the gravity waves* – I am still a bit irritated and surprised about such a statement.

A comment on increased noise during daylight is included in the introduction (ll. 75-79). A comment on the possible role of multistep vertical coupling is included in the discussion (ll. 595-618). The potential effect of ozone-gravity wave coupling on the absolute daylight-nighttime differences in the GWPED is not tiny in comparison to observed values, as now somewhat more clearly illustrated and discussed with the help of a new Figure 7 (see also reply to general comment above).

An experimental and convincing case would be to search for a resolved large-scale sun-synchronous wave in MERRA2 or HIAMCM and to run the ray-tracer GROGRAT to track the wave and its amplitude to search for a second case during nighttime to demonstrate the opposite behavior. However, it might already be helpful to identify gravity waves in ozone data to show that at least the ozone shows some response.

Generally, a theoretical approach like that of the present paper is quite convincing because of the evidence of the analytic solutions, and a helpful preliminary for performing new measurements or numerical simulations. Using current versions of the models suggested by reviewer #1 is not convincing because the feedback of GW perturbations to ozone production and loss rates are not included. Perhaps (or hopefully) the paper will stimulate further model developments including this process; however, this is beyond the scope of the present paper, as already mentioned in the discussion (ll. 582-584, ll. 608-615).

Yes, I agree, it would be very helpful to identify gravity waves in ozone data for the altitude range of the upper stratosphere/lower mesosphere region, together with identifying GWs in simultaneously measured temperatures. It is well known that gravity wave structures can be found in ozone but also in other trace gas constituents like methane or stratospheric aerosol in the lower and middle stratosphere; however, to my knowledge, suitable data sets with sufficiently high vertical resolution do not exist for the USLM region. A related note for stimulating such measurements is already included at the end of the discussion (ll. 619-622).

References:

Vadas, S. L., Zhao, J., Chu, X., & Becker, E. (2018). The excitation of secondary gravity waves from local body forces: Theory and observation. Journal of Geophysical Research: Atmospheres, 123, 9296–9325.https://doi.org/10.1029/2017JD027970

Vadas, S. L., & Becker, E. (2018). Numerical modeling of the excitation, propagation, and dissipation of primary and secondary gravity waves during wintertime at McMurdo Station in the Antarctic. Journal of Geophysical Research: Atmospheres, 123, 9326–9369. https://doi.org/10.1029/2017JD027974

Rüfenacht, R., Baumgarten, G., Hildebrand, J., Schranz, F., Matthias, V., Stober, G., Lübken, F.-J., and Kämpfer, N.: Intercomparison of middle-atmospheric wind in observations and models, Atmos. Meas. Tech., 11, 1971–1987, https://doi.org/10.5194/amt-11-1971-2018, 2018.

These references are included.

2) Reply to the comments of Referee #2

Many thanks to Referee #2 for acceptance and for the final comments. Please find here the reply (again, for orientation, the comments of Referee #1 are included in *Italic*).

Still, there are some minor points that could be clarified and modified in the text.

- from time to time, "temperature" is used instead of "potential temperature" (e.g. in line 203); I suggest to check the manuscript accordingly

Yes, thank you. The manuscript is checked and improved where necessary (see some few tracked changes in Section 2.1).

- the transition from the Lagrangian view (the total derivatives in equations (1)-(6)) to the results presented in the text and figures as "local" changes could be clearer: local changes are partial (..)/partial t: you write equations for d_0 (..)/dt. Maybe it's something quite obvious, but I always get stuck when I read these lines of results. Do you consider the total effect on Q and O3 that an air parcel experiences when traveling along a sinusoidal trajectory through the wave (integrated over one cycle or wavelength)?

Yes, indeed, this could be misunderstanding. The terminus “local” means here the amplification at a specific level (Section 2.1) which is a preliminary to the amplification during the upward propagation (Section 2.2). This is improved throughout the whole paper (mostly “at a specific level”, which is already used, instead of “local”).

I consider the total change of a sinusoidal GW pattern over one cycle or wavelength travelling within the background flow, where both the ascent and the descent branch of this sinusoidal wave perturbation are amplified. This leads to an increase in the amplitude (e.g., in terms of $|T'^2|$). However, I do not integrate over a wavelength, an integration over ascent and descent of the sinusoidal wave would be zero. A net effect at a specific level would occur if the amplification of the amplitude would lead to a change in gravity wave breaking processes, which is somewhat more highlighted (ll. 508-509, ll. 552-553).